THE

PHILOSOPHICAL TRANSACTIONS

OF THE

ROYAL SOCIETY OF LONDON,

FROM THEIR COMMENCEMENT, IN 1665, *TO THE YEAR* 1800;

Abridged,

WITH NOTES AND BIOGRAPHIC ILLUSTRATIONS,

BY

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RICHARD PEARSON, M.D. F.S.A.

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1809.

CONTENTS OF VOLUME THIRTEENTH.

THE CONTENTS CLASSED UNDER GENERAL HEADS.

Class I. Mathematics.

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2. *Algebra, Analysis, Fluxions.*

3. *Geometry, Trigonometry, Land-surveying.*

Class II. MECHANICAL PHILOSOPHY.

1. *Dynamics.*

2. *Astronomy, Chronology, Navigation.*

4. *Mechanics.*

5. *Pneumatics.*

6. *Acoustics, Music.*

7. *Optics.*

8. *Electricity, Magnetism, Thermometry, &c.*

Class III. NATURAL HISTORY.

1. *Zoology.*

2. *Botany.*

3. *Mineralogy, Fossilogy, &c.*

2. *Physiology of Animals.*

3. *Physiology of Plants.*

4. *Medicine.*

5. *Surgery.*

Class VI. The Arts.

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2. *Chemical.*

3. *Antiquities.*

Class VII. Biography; *or, Account of Authors.*

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Fig. 5.

Fig. 3.

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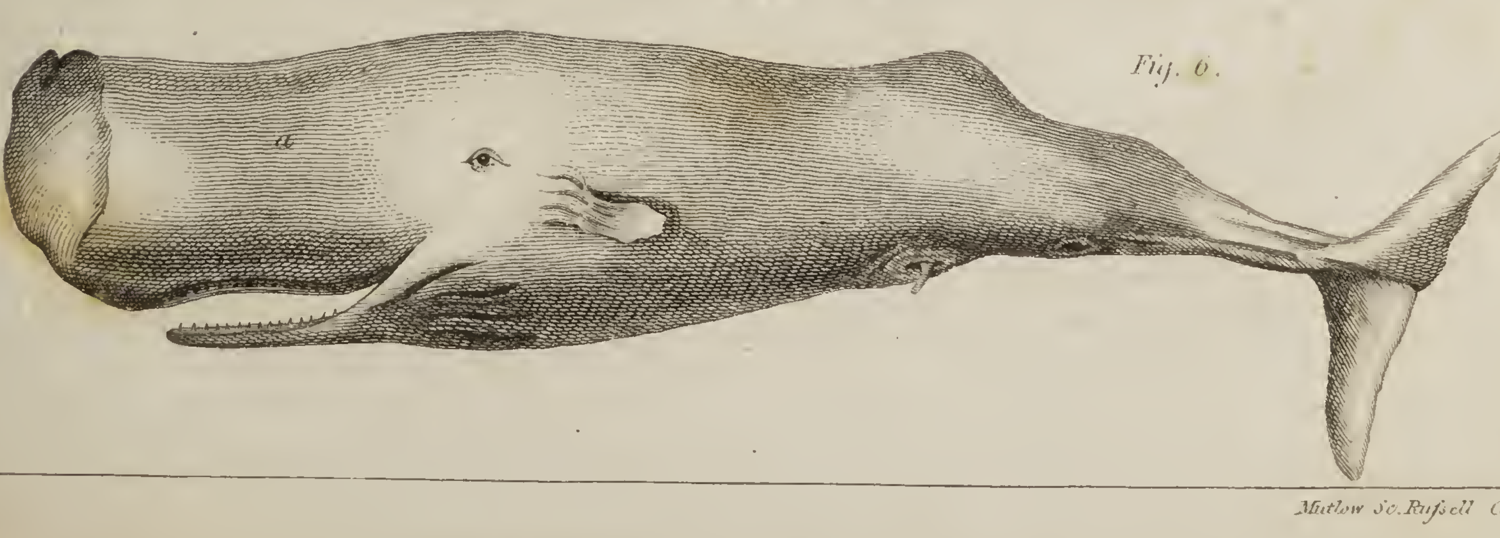
Fig. 6.



Mutlow Sc. Rußell Ct.

Published by C & R Baldwin, of New Bridge Street, London, 1807.

Gordonia Lasianthus. Vulgo Loblolly Bay. Pa. 85.

Illicium Floridanum. Vulgo Starry Aniseed Tree. Pa. 87.

Mutlow Sc. Russell Co.

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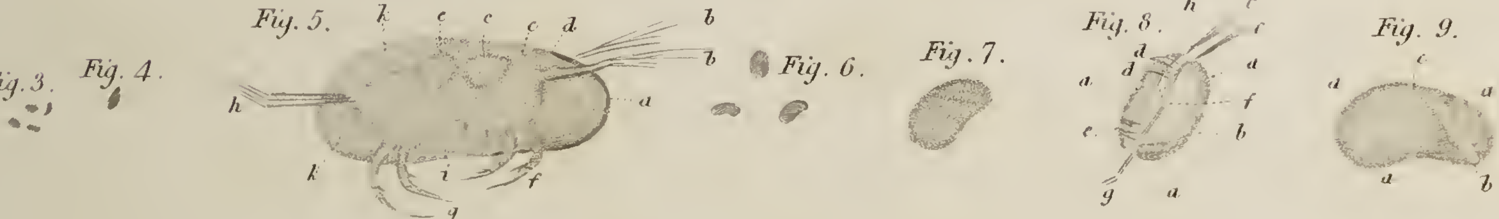

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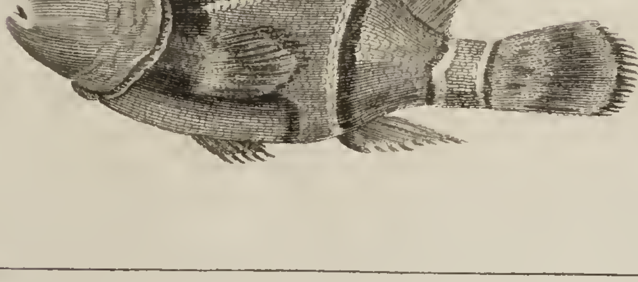

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Freshwater Turtle. Pa. 141.

Fig. 1.

Fig. 2.

Fig. 3.

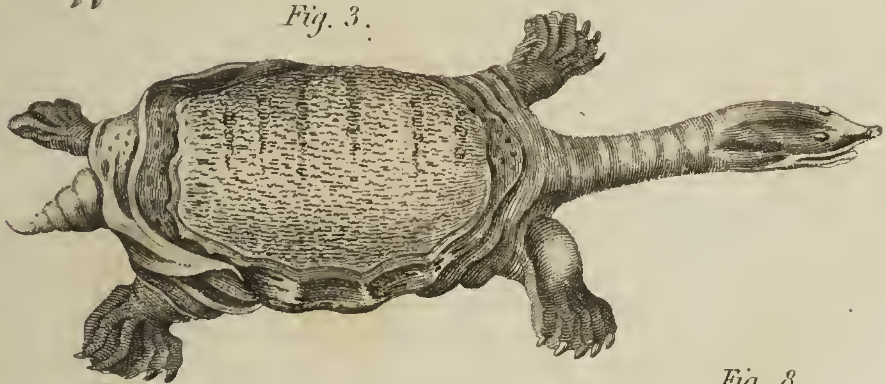

Fig. 4.

Fig. 5.

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Fig. 6.

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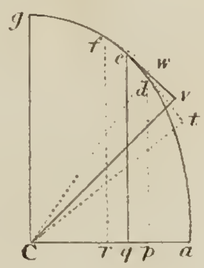

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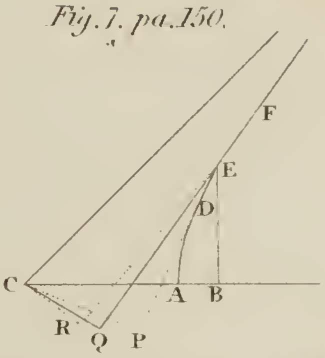

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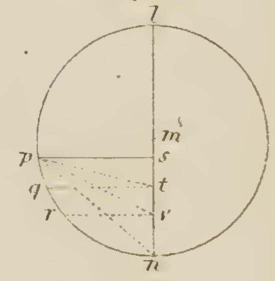

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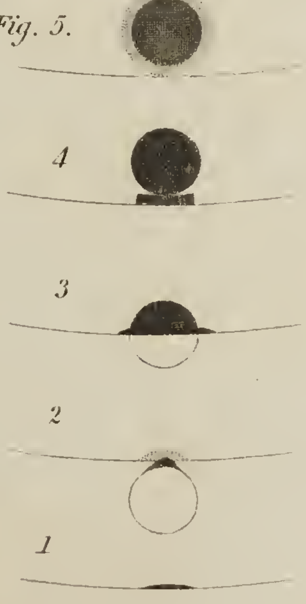

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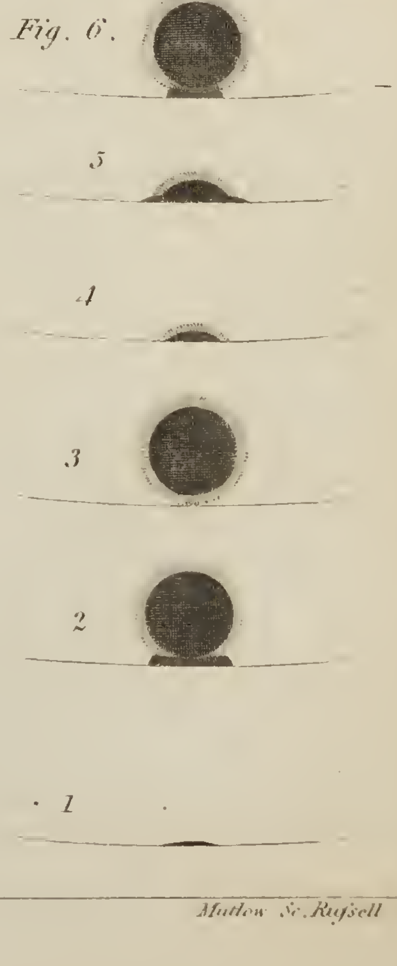

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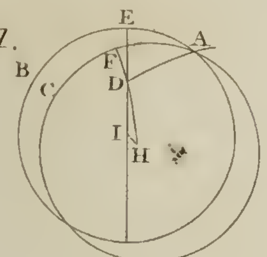
Pemberton on Eclipses. Pa. 184. &c.

Fig. 1.



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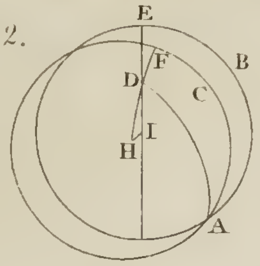

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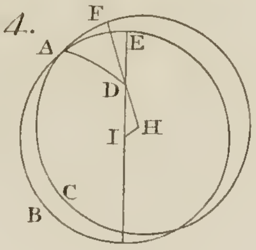
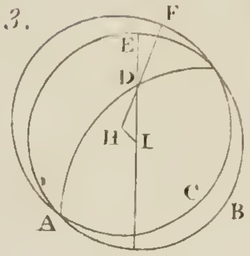


Fig. 3.



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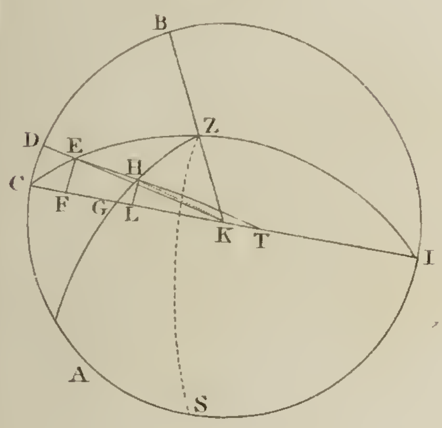

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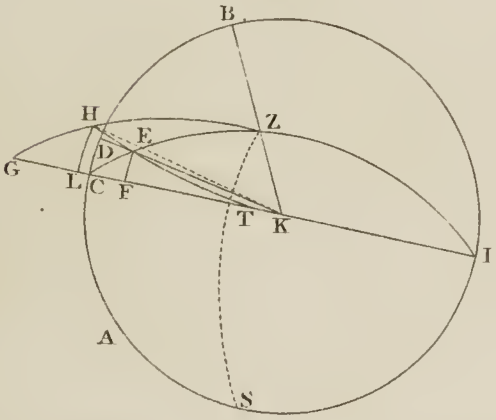

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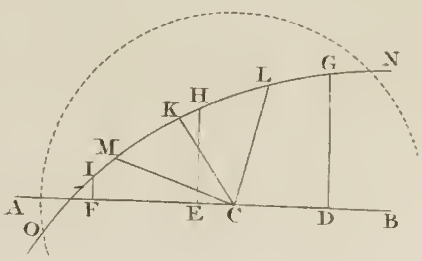

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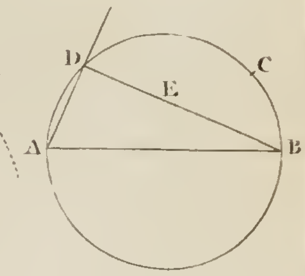

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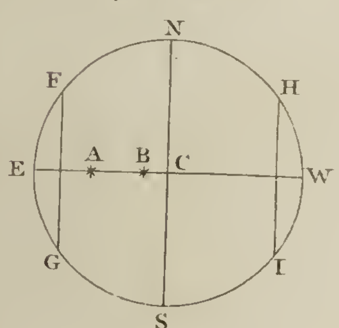

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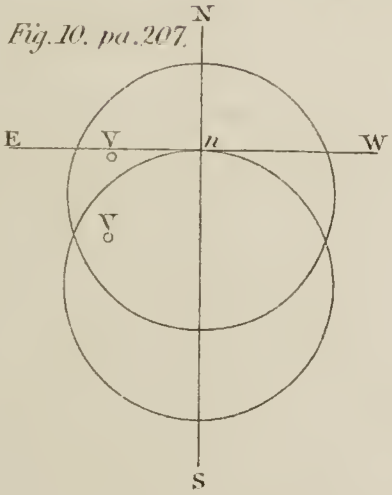

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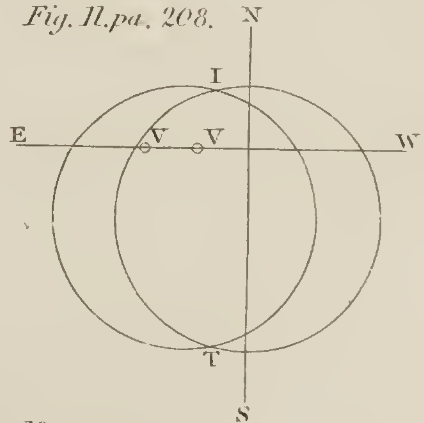

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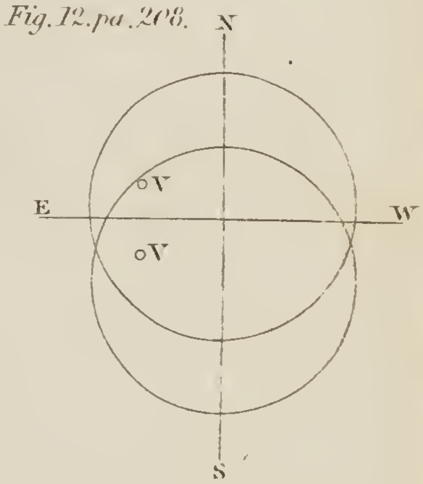

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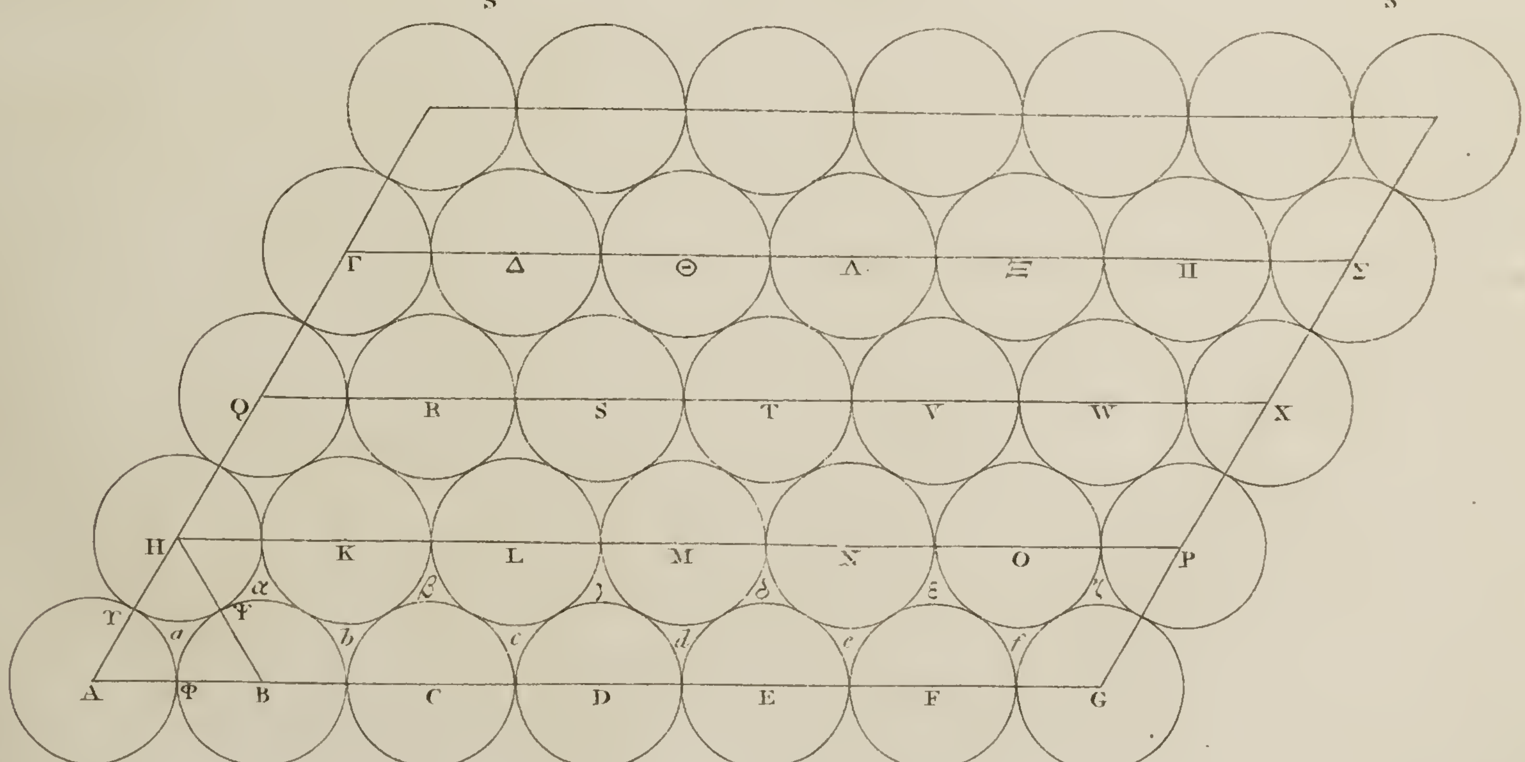

Mutlow Sc. Russell Ct.

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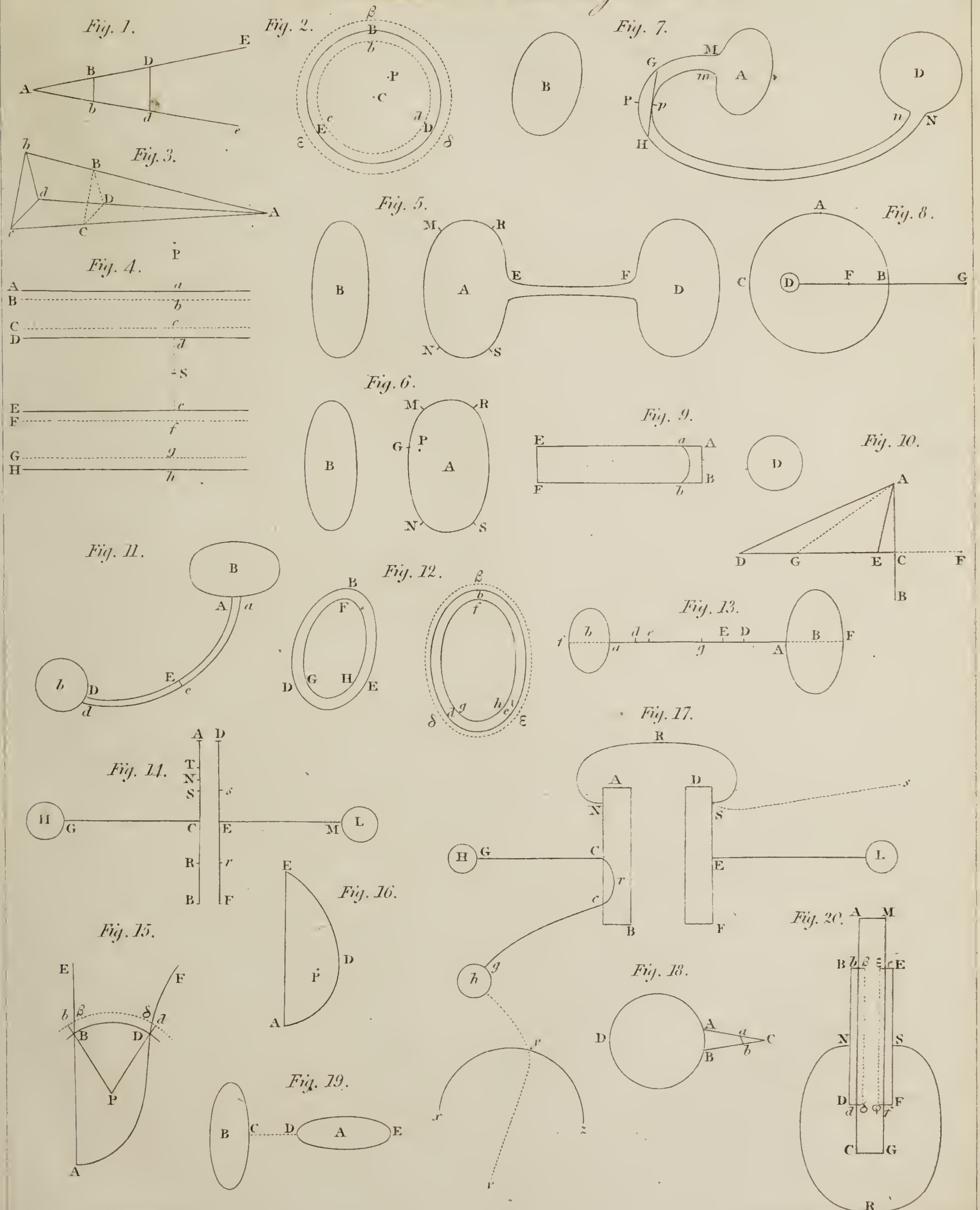
Mr. Cavendish on Electricity. Pa. 225. &c.



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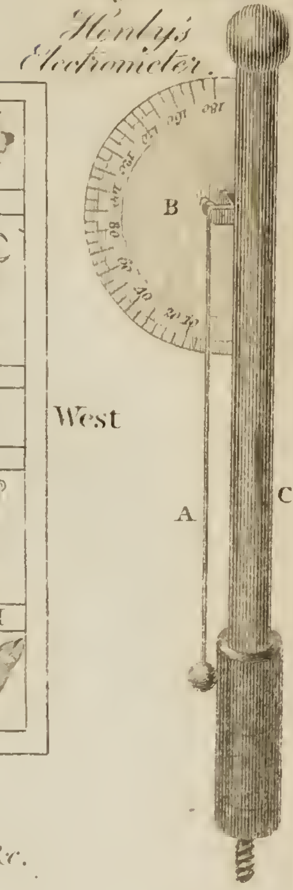
Malacca Male bird.

Fig. 1. p. 267.

Fig. 2. Indian Zodiac. p. 321.

Fig. 3. Pa. 323. Henly's Electrometer.



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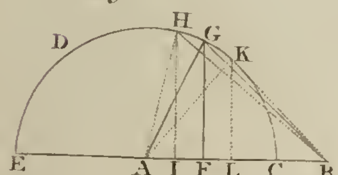

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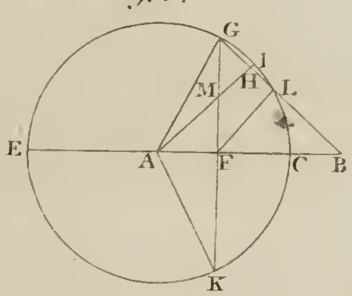

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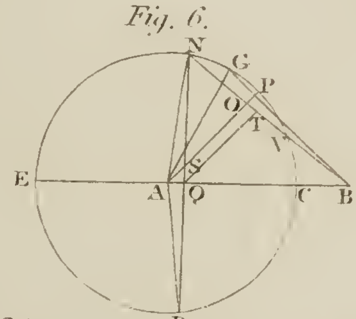

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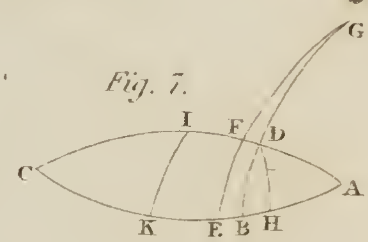

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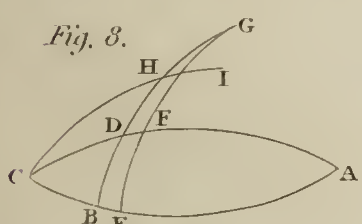

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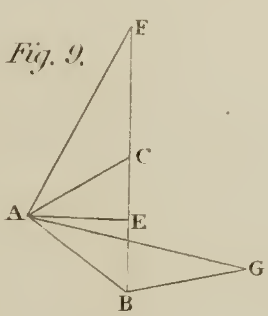

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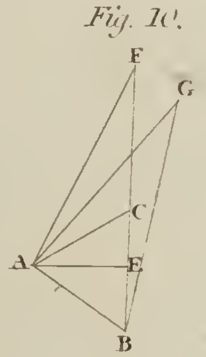

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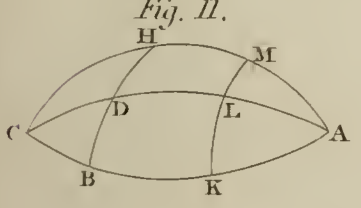

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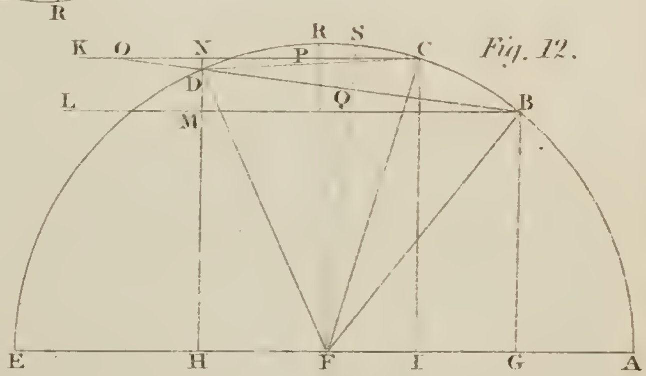

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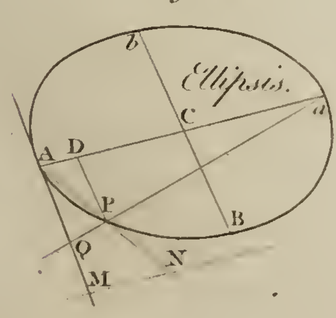

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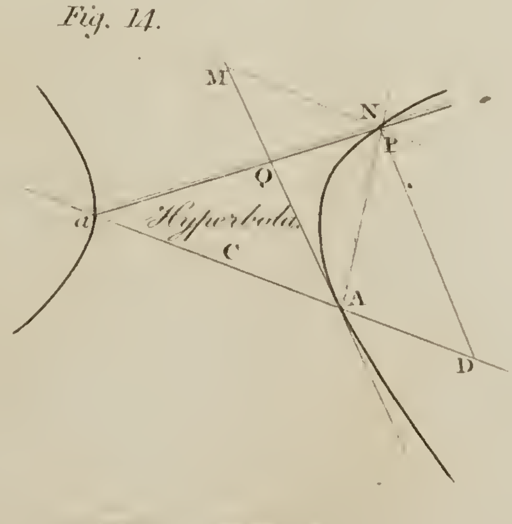

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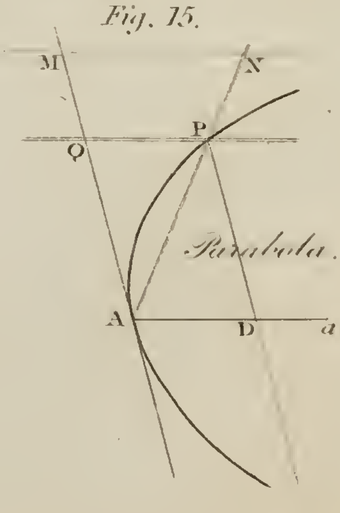

Mutlow Sc. Russell C.t

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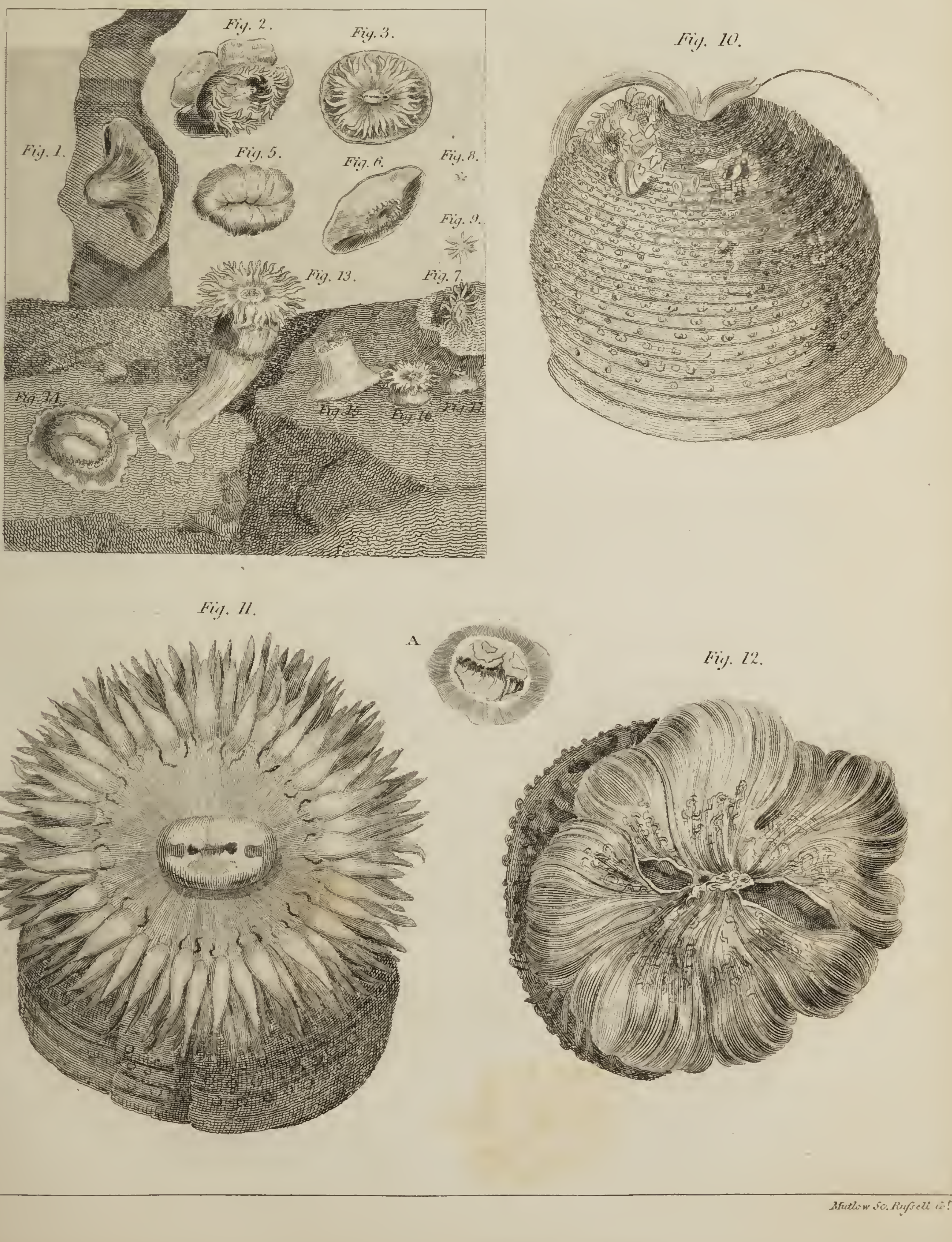
Sea Anemonies. Pa. 460 &c.



Mutlow Sc. Russell ct.

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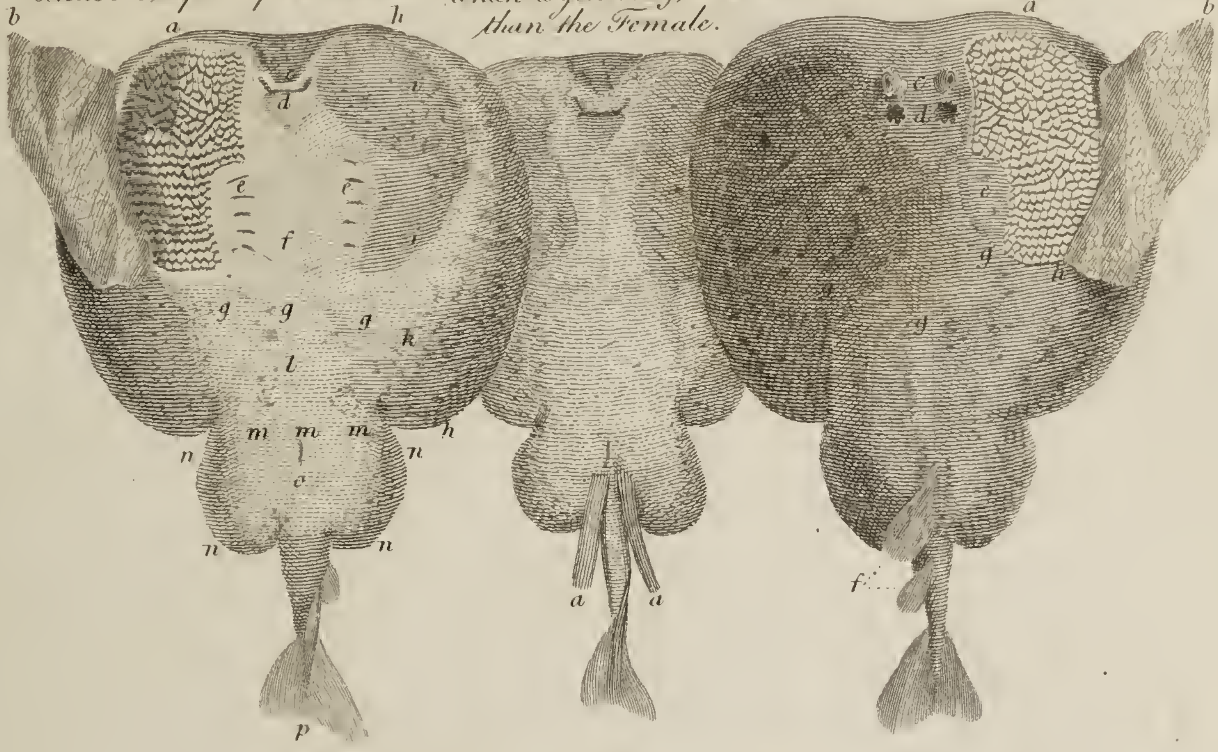
The Male & Female Torpedo, or Electric Ray, frequenting the Sea Shores, in the Neighbourhood of La Rochelle. Pa. 477.

Fig. 1. Under Surface of the Female.

Fig. 3. Under Surface of the Male, which is generally smaller than the Female.

Fig. 2. Upper Surface of the Female.



Torpedo Electric Organs. Pa. 481.

Fig. 4. Fig. 5.

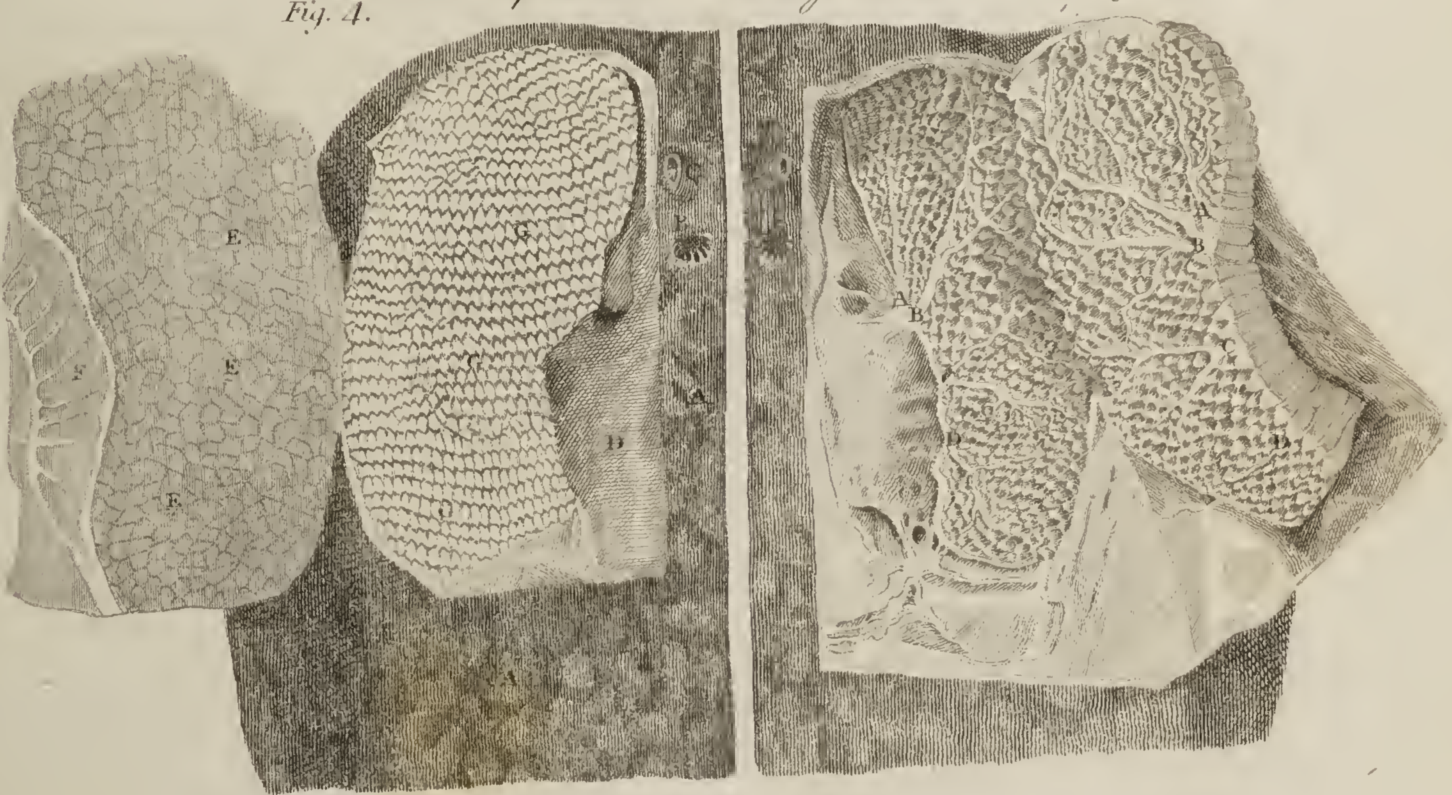


Fig. 6.

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Points and Balls.

Fig. 1. Pa. 501.

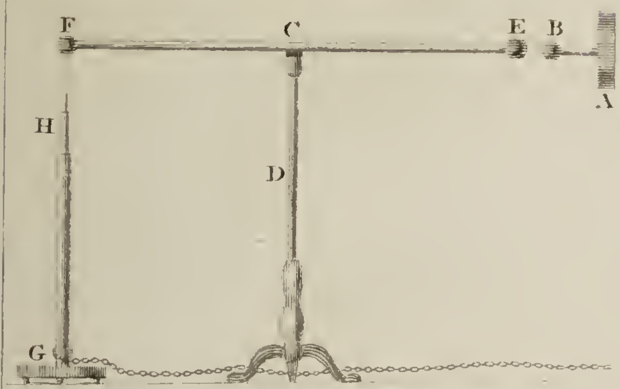
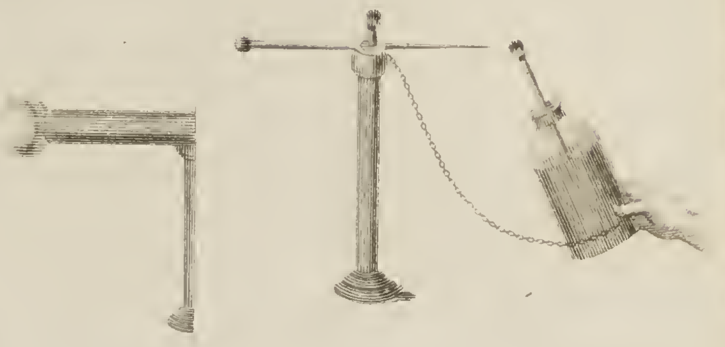


Fig. 2. p. 512.

Fig. 3.



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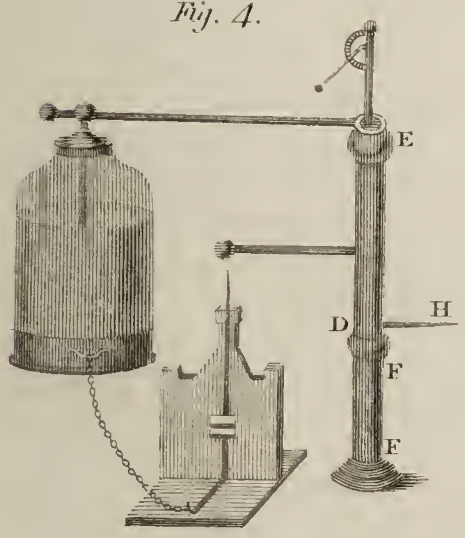

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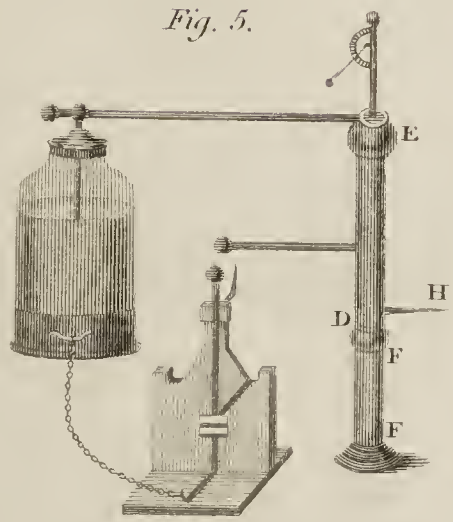
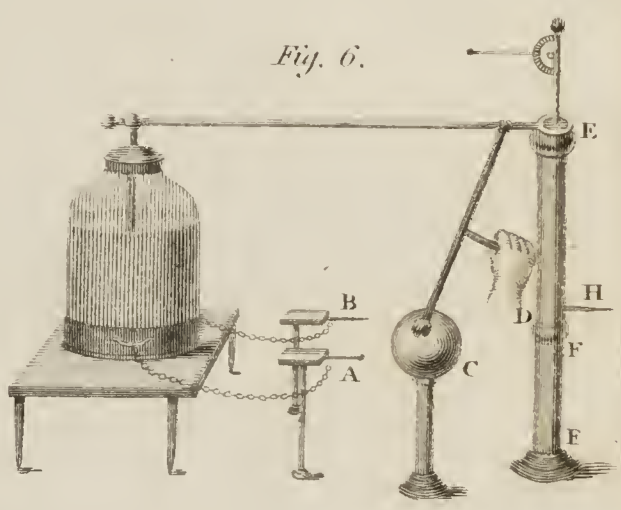


Fig. 6.



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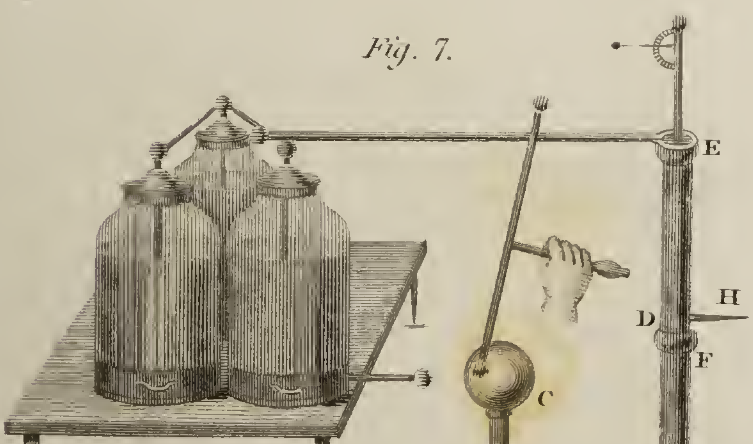

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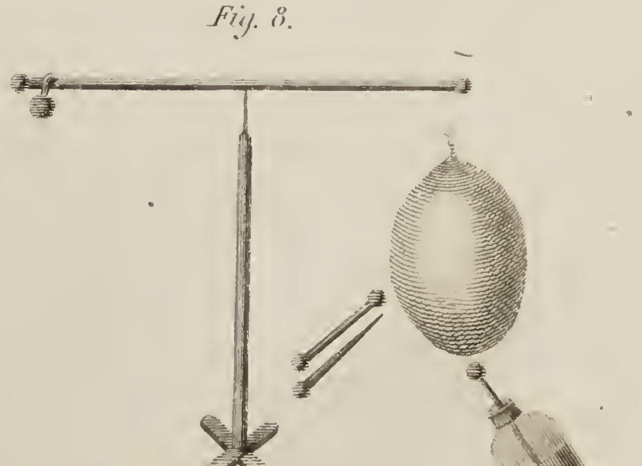

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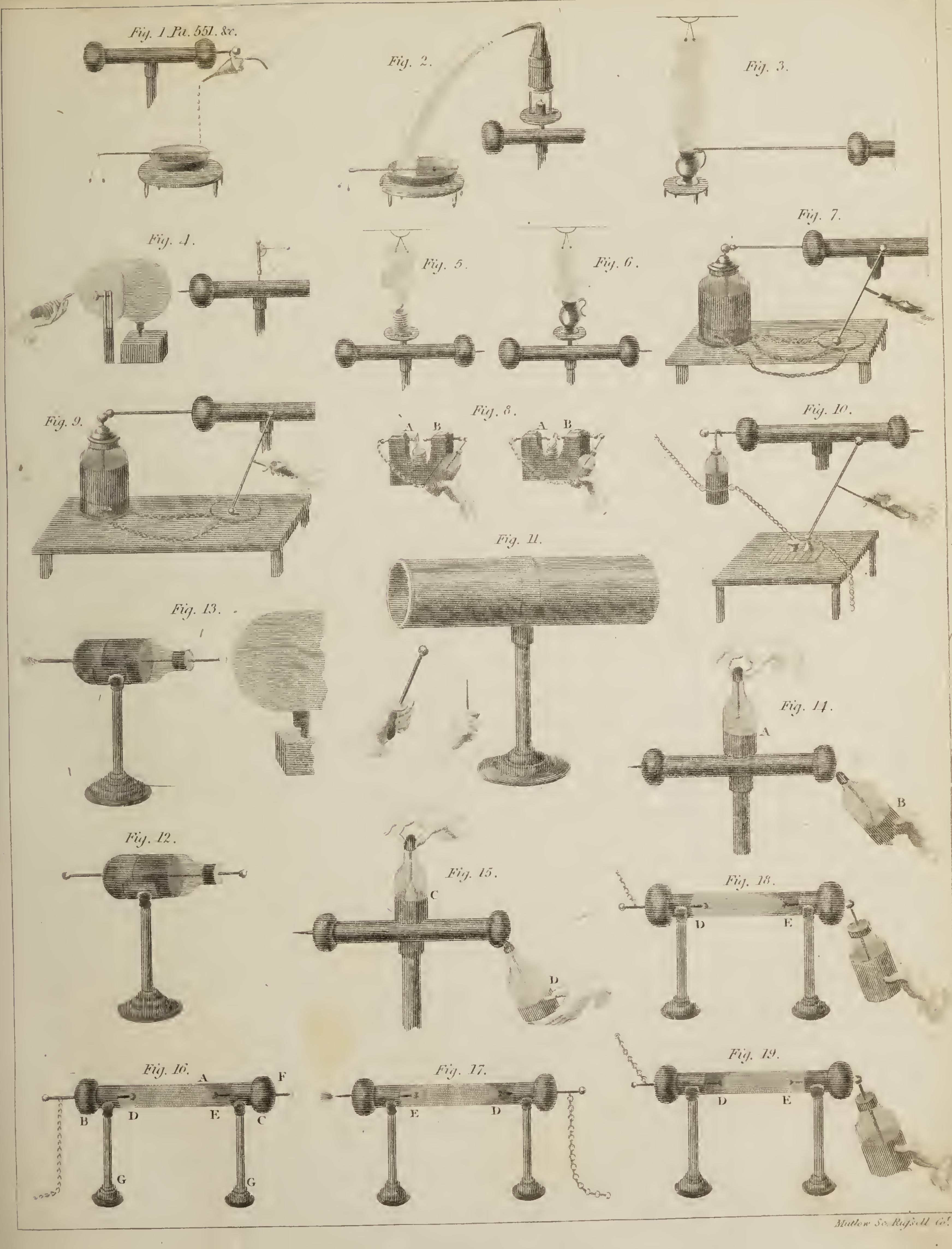

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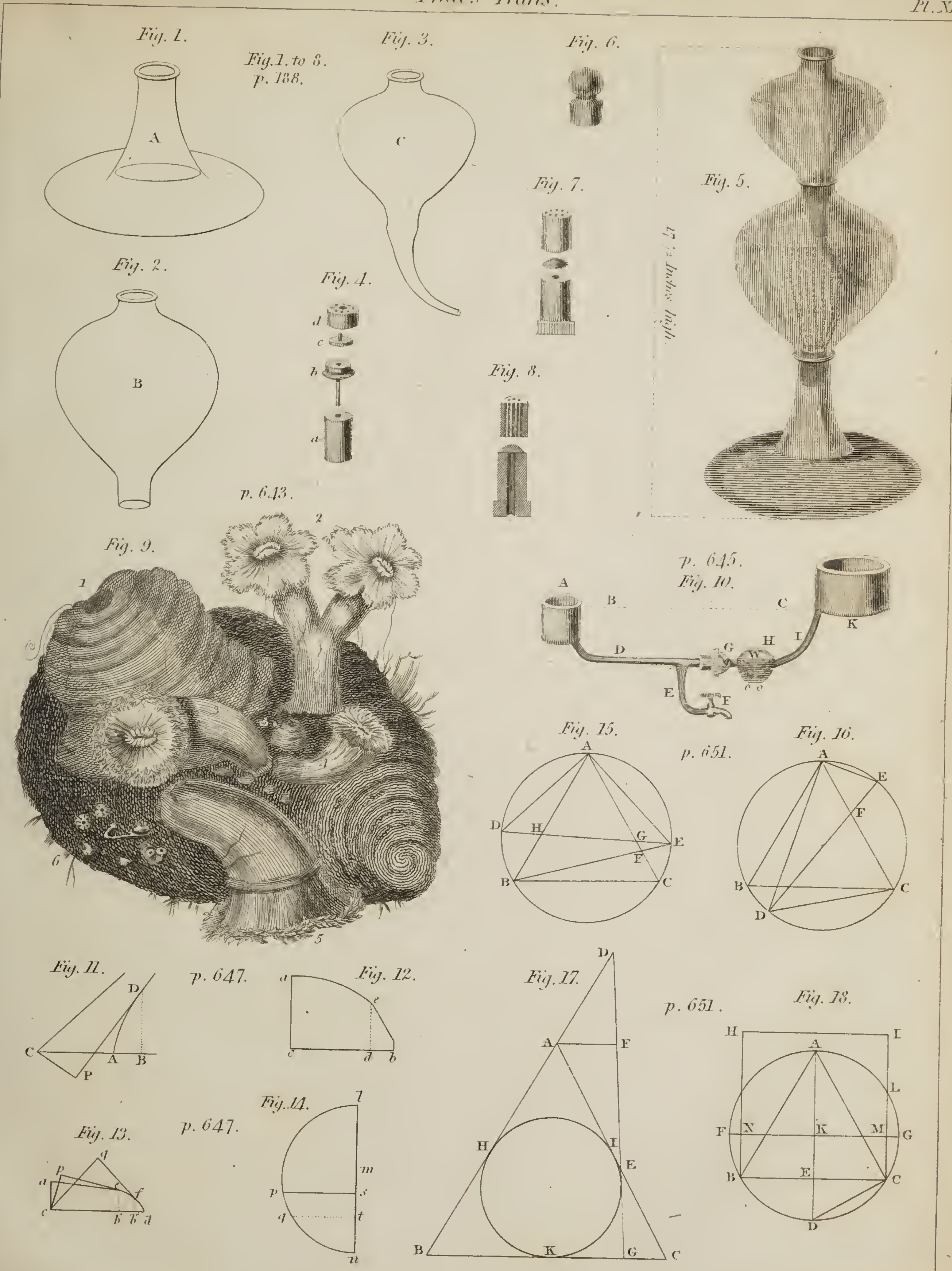

Mutlow Sc. Russell Ct.

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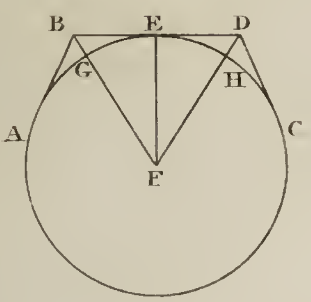
Fig. 1. Pa. 653.



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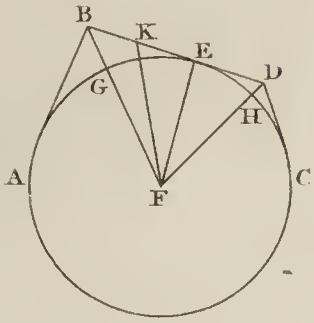

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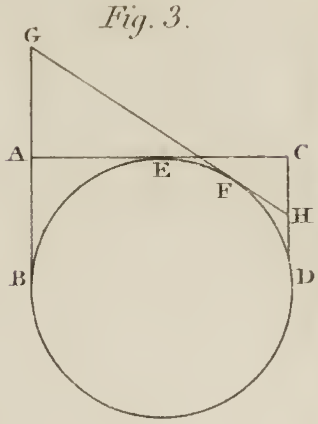
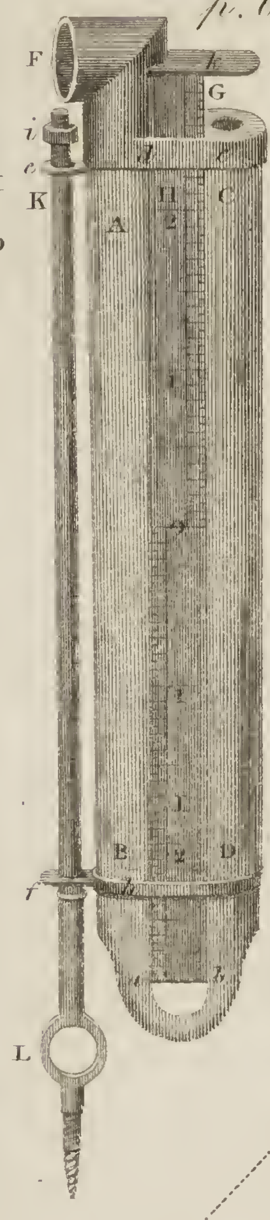


Fig. 4. Wind Gage. p. 661.



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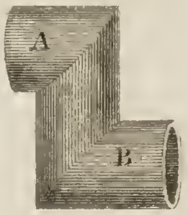

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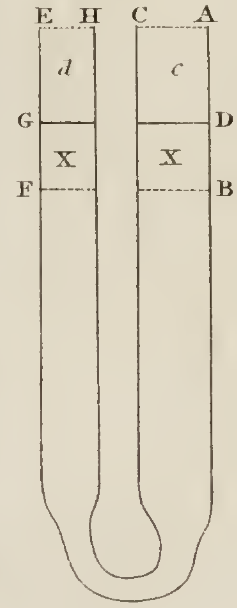
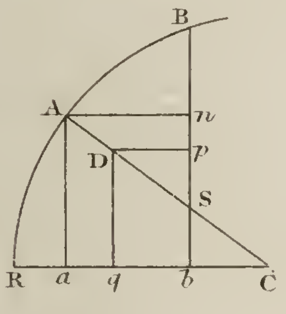


Fig. 7. to 10 p. 690.

Fig. 7.



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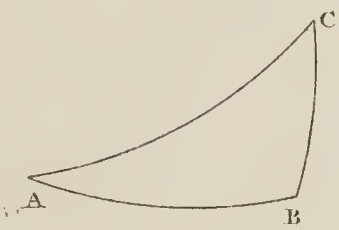

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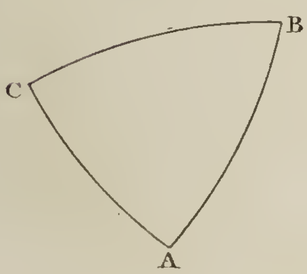
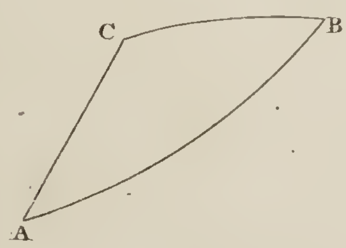


Fig. 10.



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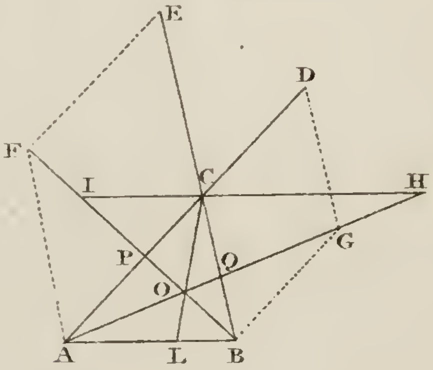


Fig. 11. to 15 p. 729.

Fig. 12.

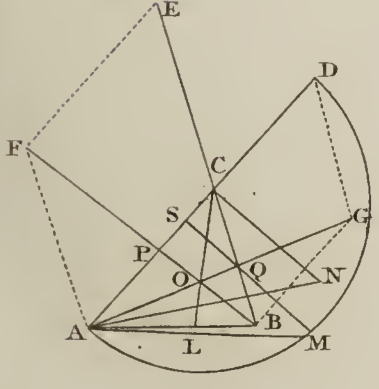


Fig. 13.

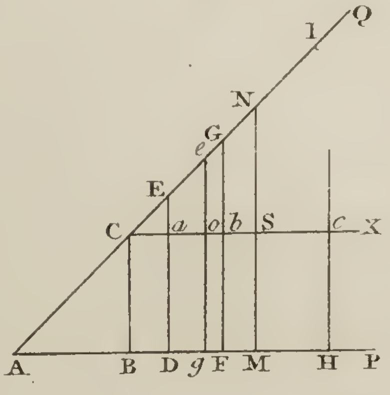


Fig. 14.

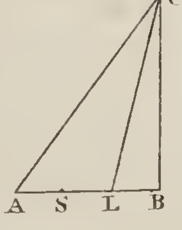
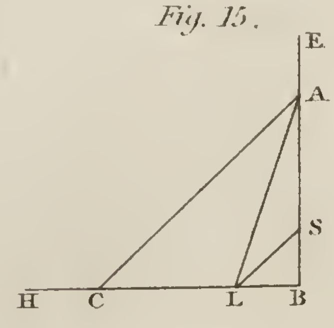


Fig. 15.



Mutlow Sc. Russell Ct.

Published by C & R. Baldwin, of New Bridge Street, London, 1807.

Specimens of Gorgonia. p. 720.

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Gymnotus Electricus. p. 671.

Fig. 1.

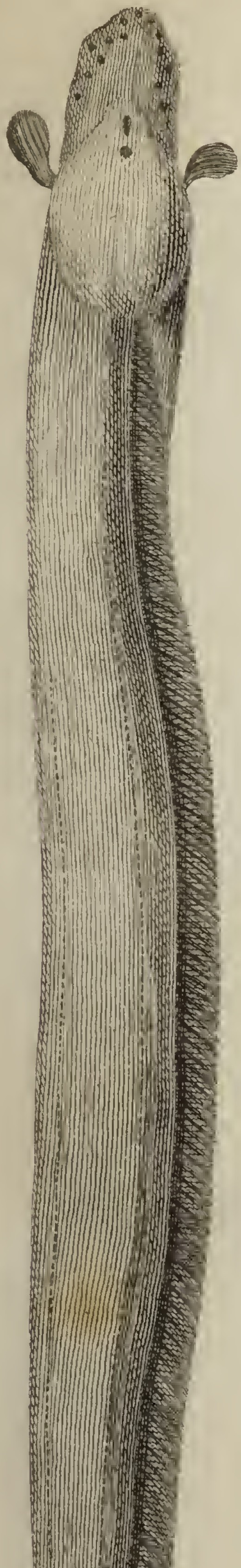

Fig. 2.

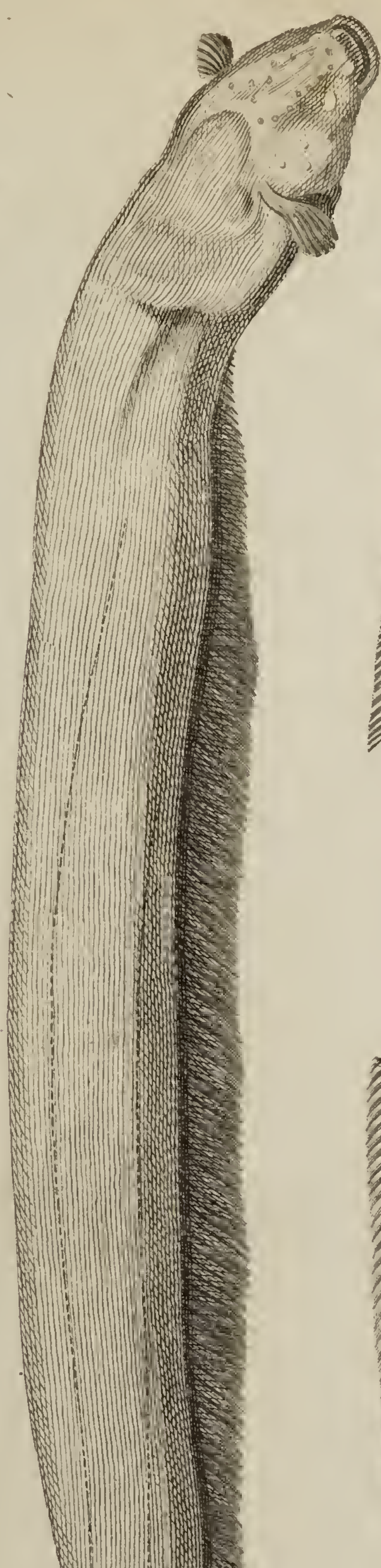

Fig. 3.

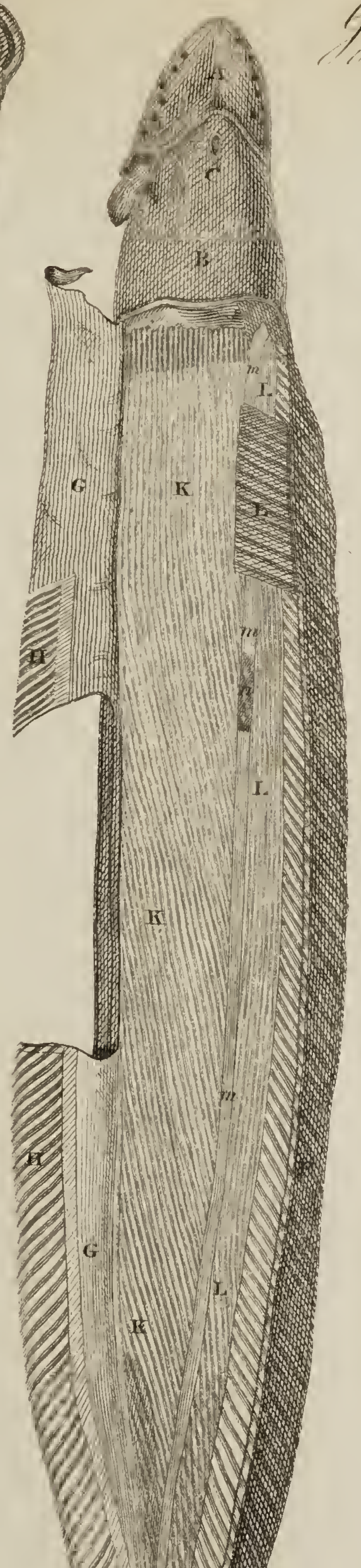

Fig. 4.

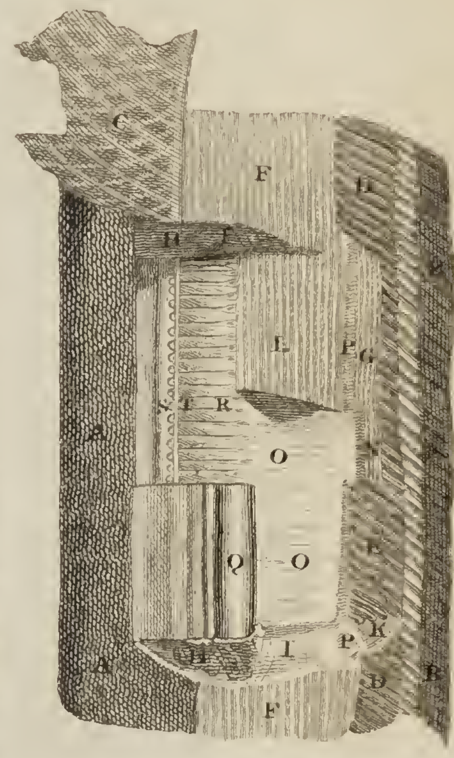

Fig. 5.

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THE

PHILOSOPHICAL TRANSACTIONS

OF THE

ROYAL SOCIETY OF LONDON;

ABRIDGED.

I. An Account of a Journey to Mount Etna. By the Hon. William Hamilton. Dated Naples, Oct. 17, 1769. p. 1. *Vol. LX. Anno* 1770.

After having examined with much attention the operations of mount Vesuvius, during 5 years, and after having carefully remarked the nature of the soil for 15 miles round this capital, Sir W. was well convinced that the whole of it has been formed by explosion. Many of the craters, whence this matter has issued, are still visible; such as the Solfaterra near Puzzole, the lake of Agnano, and near this lake a mountain composed of burnt matter, that has a very large crater surrounded with a wall to inclose the wild boars, and deer, that are kept there for the diversion of his Sicilian majesty; it is called Astruni: the Monte Nuovo thrown up from the bottom of the lucrine lake in the year 1538, which has likewise its crater, and the lake of Averno. The islands of Nisida and Procida are entirely composed of burnt matter; the island of Ischia is likewise composed of lava, pumice, and burnt matter; and there are in that island several visible craters, from one of which, no longer ago than the year 1303, there issued a lava which ran into the sea, and is still in the same barren state as the modern lavas of Vesuvius. After having been accustomed to these observations, he was well prepared to visit the most ancient, and perhaps the most considerable volcano that exists; and he had the satisfaction of being thoroughly convinced there, of the formation of very considerable mountains by mere explosion, having seen many such on the sides of Etna.

On the 24th of June 1760, Sir W. and 2 companions left Catania, a town situated at the foot of mount Etna, and passed through the inferior district of the mountain, called by its inhabitants La Regione Piemontese. It is well watered, exceedingly fertile, and abounding with vines, and other fruit trees, where the lava, or, as it is called there, the Sciara, has had time to soften and gather soil sufficient for vegetation, which he was convinced from many obser-

vations, unless assisted by art, does not come to pass for many ages, perhaps a thousand years or more. The circuit of this lower region, forming the basis of the great volcano, is upwards of 100 Italian miles. The vines of Etna are kept low, quite the reverse of those on the borders of Vesuvius, and they produce a stronger wine, but not in so great abundance. The Piemontese district is covered with towns, villages, monasteries, &c. and is well peopled, notwithstanding the danger of such a situation. Catania, so often destroyed by eruptions of Etna, and totally overthrown by an earthquake towards the end of the last century, has been rebuilt within these 50 years, and is now a considerable town, with at least 35,000 inhabitants.

In about 4 hours of gradual ascent they arrived at a little convent of benedictine monks, called St. Nicolo dell' Arena, about 13 miles from Catania, and within a mile of the volcano whence issued the last very great eruption in the year 1669. They slept in the benedictines' convent the night of the 24th, and passed the next morning in observing the ravage made by the abovementioned terrible eruption, over the rich country of the Piemontese. The lava burst out of a vineyard within a mile of St. Nicolo', and by frequent explosions of stones and ashes, raised there a mountain, which Sir W. thinks is not less than half a mile perpendicular in height, and is certainly at least 3 miles in circumference at its basis. The lava that ran from it, and on which there are as yet no signs of vegetation, is 14 miles in length, and in many parts 6 in breadth; it reached Catania, and destroyed part of its walls, buried an amphitheatre, an aqueduct, and many other monuments of its ancient grandeur, which, till then, had resisted the hand of time; and ran a considerable length into the sea, so as to have once formed a beautiful and safe harbour; but it was soon after filled up by a fresh torrent of the same inflamed matter, a circumstance the Catanians lament to this day, as they are without a port. There has been no such eruption since, though there are signs of many, more terrible, that have preceded it.

For 2 or 3 miles round the mountain raised by this eruption, all is barren, and covered with ashes; this ground, as well as the mountain itself, will in time certainly be as fertile as many other mountains in its neighbourhood, that have been likewise formed by explosion. If the dates of these explosions could be ascertained, it would be very curious, and mark the progress of time with respect to the return of vegetation, as the mountains raised by them are in different states; those which seem to be the most modern are covered with ashes only; others of an older date, with small plants and herbs; and the most ancient, with the largest timber trees he ever saw; but he believes the latter are so very ancient, as to be far out of the reach of history. At the foot of the mountain raised by the eruption of the year 1669, there is a hole, through which, by means of a rope, they descended into several subterraneous caverns,

branching out and extending much farther and deeper than they chose to venture, the cold there being excessive, and a violent wind frequently extinguishing some of their torches. These caverns undoubtedly contained the lava that issued forth and extended quite to Catania. There are many of these subterraneous cavities known, on other parts of Etna. Some of them are made use of as magazines for snow; the whole island of Sicily and Malta being supplied with this essential article from mount Etna; many more would doubtless be found, if searched for, particularly near and under the craters, whence great lavas have issued; as the immense quantities of such matter we see above ground must necessarily suppose very great hollows beneath.

After having passed the morning of the 25th in these observations, they proceeded through the 2d, or middle region of Etna, called La Selvosa, the woody, than which nothing can be more beautiful. On every side are mountains, or fragments of mountains, that have been thrown up by various ancient explosions: some are near as high as mount Vesuvius, one in particular, is little less than one mile in perpendicular height, and 5 in circumference at its basis. They are all more or less covered, even within their craters, as well as the rich valleys between them, with the largest oak, chestnut, and fir trees, he ever saw any where; and indeed it is hence chiefly, that his Sicilian majesty's dock-yards are supplied with timber. As this part of Etna was famous for its timber in the time of the tyrants of Syracusa, and as it requires the great length of time already mentioned before the matter is fit for vegetation, we may conceive the great age of this respectable volcano. The chestnut-trees predominated in the parts through which they passed, and, though of a very great size, are not to be compared to some on another part of the Regione Selvosa, called Carpinetto. It is amazing that trees should flourish in so shallow a soil, for they cannot penetrate deep without meeting with a rock of lava; and indeed great part of the roots of the large trees are above ground, and have acquired, by the impression of the air, a bark like that of their branches. In this part of the mountain are the finest horned cattle in Sicily; in general the horns of the Sicilian cattle are near twice the size of any they had ever seen; the cattle themselves are of the common size. They passed by the lava of the last eruption in the year 1766, which has destroyed above 4 miles square of the beautiful wood abovementioned. The mountain raised by this eruption abounds with sulphur and salts, exactly resembling those of Vesuvius.

In about 5 hours from the time they had left the convent of St. Nicolo dell' Arena, they arrived at the borders of the 3d region, called La Netta, or Scoperta, clean, or uncovered, where they found a very sharp air indeed; so that in the same day, the 4 seasons of the year were sensibly felt on this mountain; excessive summer heats in the Piemontese, spring and autumn temperature in

the middle, and extreme cold of winter in the upper region. As they approached the latter, a gradual decrease of vegetation is perceived, and from large timber trees they came to the smaller shrubs and plants of the northern climates; he observed quantities of juniper and tansey. Night coming on, they here pitched a tent and made a good fire, which was very necessary, for without it, and very warm cloathing, they would surely have perished with cold; and at one o'clock in the morning of the 26th, they pursued their journey towards the great crater. They passed over valleys of snow that never melts, except there is an eruption of lava from the upper crater, which scarcely ever happens; the great eruptions are usually from the middle region, the inflamed matter finding probably its passage through some weak part, long before it can rise to the excessive height of the upper region, the great mouth on the summit only serving as a common chimney to the volcano. In many places the snow is covered with a bed of ashes, thrown out of the crater, and the sun melting it in some parts makes this ground treacherous. They arrived safe at the foot of the little mountain of ashes that crowns Etna, about an hour before sun-rise. This mountain is situated in a gently inclining plain, of about 9 miles in circumference; it is about a quarter of a mile perpendicular in height, very steep, but not quite so steep as Vesuvius; it has been thrown up within these 25 or 30 years. Hitherto the ascent had been so gradual (for the top of Etna is not less than 30 miles from Catania, whence the ascent begins) as not to have been the least fatiguing; and if it had not been for the snow, we might have rode on our mules to the very foot of the little mountain. As Sir W. saw that this little mountain was composed in the same manner as the top of Vesuvius, which, notwithstanding the smoke issuing from every pore, is solid and firm, he made no scruple of going up to the edge of the crater. The steep ascent, the keenness of the air, the vapours of the sulphur, and the violence of the wind, which obliged them several times to throw themselves flat on their faces to avoid being overturned by it, made this latter part of the expedition rather inconvenient and disagreeable.

Soon after they had seated themselves on the highest point of Etna, the sun arose and displayed a scene that indeed passes all description. The horizon lighting up by degrees, they discovered the greatest part of Calabria, and the sea on the other side of it; the Pharo of Messina, the Lipari islands, Stromboli with its smoking top, though at above 70 miles distance, seemed to be just under their feet; they saw the whole island of Sicily, its rivers, towns, harbours, &c. as if they had been looking on a map. In short, they took in at one view, a circle of about 900 English miles. The pyramidal shadow of the mountain reached across the whole island and far into the sea on the other side. They counted from hence 44 little mountains (little in comparison of their mother Etna, though they would appear great any where else) in the middle region on

the Catania side, and many others on the other side of the mountain, all of a conical form, and each having its crater; many with timber trees flourishing both within and without their craters. The points of those mountains, that he imagined to be the most ancient, are blunted, and the craters of course more extensive and less deep than those of the mountains formed by explosions of a later date, and which preserve their pyramidal form entire. Some have been so far mouldered down by time as to have no other appearance of a crater than a sort of dimple or hollow on their rounded tops, others with only half or a third part of their cone standing; the parts that are wanting having mouldered down, or perhaps been detached from them by earthquakes, which are here very frequent. All however have been evidently raised by explosion; and he believes, on examination, many of the whimsical shapes of mountains in other parts of the world would prove to have been occasioned by the same natural operations. Sir W. observed that these mountains were generally in lines or ridges; they have mostly a fracture on one side, the same as in the little mountains raised by explosion on the sides of Vesuvius, of which there are 8 or 9. This fracture is occasioned by the lava's forcing its way out. Whenever he shall meet with a mountain, in any part of the world, whose form is regularly conical, with a hollow crater on its top, and one side broken, he will be apt to decide such a mountain's having been formed by an eruption, as both on Etna and Vesuvius the mountains formed by explosion are without exception according to this description. They looked into the great crater, which is about $2\frac{1}{2}$ miles in circumference; they did not think it safe to go round and measure it, as some parts seemed to be very tender ground. The inside of the crater, which is incrusted with salts and sulphurs like that of Vesuvius, is in the form of an inverted hollow cone, and its depth nearly answers to the height of the little mountain that crowns the great volcano. The smoke issuing abundantly from the sides and bottom, prevented their seeing quite down; but the wind clearing away the smoke from time to time, they saw this inverted cone contracted almost to a point. The air was so very pure and keen in the whole upper region of Etna, and particularly in the most elevated parts of it, that they had a difficulty in respiration, and that independent of the sulphureous vapour. At the foot of Etna, the 24th, the quicksilver stood at 27 degrees 4 lines, and the 26th, at the most elevated point of the volcano, it was at 18 degrees 10 lines. The thermometer, on the first observation at the foot of the mountain was at 84 degrees, and on the second at the crater at 56. The weather had not changed in any respect, and was equally fine and clear, the 24th and 26th. He believes that the perpendicular height of mount Etna is something more than 3 Italian miles.

After having passed at least 3 hours on the crater, they descended and went to a rising ground, about a mile distant from the upper mountain just left, and saw

there some remains of the foundation of an ancient building; it is of brick, and seems to have been ornamented with white marble, many fragments of which are scattered about. It is called the Philosopher's Tower, and is said to have been inhabited by Empedocles. As the ancients used to sacrifice to the celestial gods on the top of Etna, it may very well be the ruin of a temple that served for that purpose. Hence they went a little further over the inclined plain above-mentioned, and saw the evident marks of a dreadful torrent of hot water that came out of the great crater at the time of an eruption of lava in the year 1755. Luckily this torrent did not take its course over the inhabited parts of the mountain, as a like accident on mount Vesuvius in 1631 swept away some towns and villages in its neighbourhood, with thousands of their inhabitants. The common received opinion is, that these eruptions of water proceed from the volcanos having a communication with the sea; but Sir W. rather believes them to proceed merely from depositions of rain water in some of the inward cavities of them. They also saw from hence the whole course of an ancient lava, the most considerable as to its extent of any known here; it ran into the sea near Taormina, which is not less than 30 miles from the crater whence it issued, and is in many parts 15 miles in breadth. As the lavas of Etna are very commonly 15 and 20 miles in length, 6 or 7 in breadth, and 50 feet or more in depth, we may judge of the prodigious quantities of matter emitted in a great eruption of this mountain, and of the vast cavities there must necessarily be within its bowels. The most extensive lavas of Vesuvius do not exceed 7 miles in length; the operations of nature on the one mountain and the other are certainly the same; but on mount Etna, all are on a great scale. As to the nature and quality of their lavas, they are much the same; but he thinks those of Etna rather blacker, and in general more porous, than those of Vesuvius. In the parts of Etna that they went over they saw no stratas of pumice stones, which are frequent near Vesuvius, and cover the ancient city of Pompeia; but their guide said, that there are such in other parts of the mountain. Sir W. saw some stratas of what is called here Tuffa, it is the same that covers Herculaneum, and that composes most of the high grounds about Naples; it is on examination a mixture of small pumice stones, ashes, and fragments of lava, which is by time hardened into a sort of stone. In short he found, with respect to the matter erupted, nothing on mount Etna that Vesuvius does not produce, and there certainly is a much greater variety in the erupted matter and lavas of the latter, than of the former; both abound with pyrites and crystallizations, or rather vitrifications. The sea-shore at the foot of Etna, indeed, abounds with amber, of which there is none found at the foot of Vesuvius. At present there is a much greater quantity of sulphur and salts on the top of Vesuvius than on that of Etna; but this circumstance varies according to the degree of fermentation

within, and the guide assured them he had seen greater quantities on Etna at other times. In their way back to Catania, the guide showed him a little hill covered with vines, which belonged to the jesuits, and, as is well attested, was undermined by the lava in the year 1669, and transported half a mile from the place where it stood, without having damaged the vines. In great eruptions of Etna, the same sort of lightning, as described in his account of the last eruption of Vesuvius, has been frequently seen to issue from the smoke of its great crater. Till the year 252 of Christ, the chronological accounts of the eruptions of Etna are very imperfect; but as the veil of St. Agatha was in that year first opposed to check the violence of the torrents of lava, and has ever since been produced at the time of great eruptions, the miracles attributed to its influence having been carefully recorded by the priests, have at least preserved the dates of such eruptions. The relics of St. Januarius have rendered the same service to the lovers of natural history, by recording the great eruptions of Vesuvius.

On their return from Messina to Naples, they were becalmed 3 days in the midst of the Lipari Islands, by which they had an opportunity of seeing that they have all been evidently formed by explosion; one of them, called Vulcano, is in the same state as the Solfaterra; Stromboli is a volcano, existing in all its force, and, in its form of course, is the most pyramidal of all the Lipari Islands; they saw it throw up red hot stones from its crater frequently, and some small streams of lava issued from its side, and ran into the sea. This volcano differs from Etna and Vesuvius, by its continually emitting fire, and seldom any lava: notwithstanding its continual explosions, this island is inhabited, on one side, by about a hundred families.

II. On the Inhabitants of the Coast of Patagonia. By Philip Carteret, Esq., Captain of the Swallow Sloop. p. 20.

Capt. Carteret sailed in company with Capt. Willis on this expedition. Capt. C. describes these Patagonians as the finest set of men he ever saw; their height in general from 6 feet to 6 feet 5 inches; though some few were 6 feet 7 inches; but none above that. Other particulars of these people may be seen in Dr. Hawksworth's account of the voyages made to Patagonia.

III. On a Camelopardalis found about the Cape of Good Hope. By Captain Carteret. p. 27.

The accompanying drawing is of a camelopardalis, (fig. 1, pl. 1) as it was taken from life, of one near the Cape of Good Hope. From its scarcity, Capt. C. believes none have been seen in Europe since Julius Cæsar's time, when he thinks there were two of them at Rome. The present governor of the Cape of

Good Hope has sent out parties of men on inland discoveries, some of which have been absent from 18 months to 2 years, in which traverse they have discovered many curiosities. One of these parties crossed many mountains and plains, in one of which they found 2 of these creatures, but they only caught the young one, of which the inclosed is the drawing, as it was taken off by them; they endeavoured to bring him alive to the Cape Town, but unfortunately it died. They took off his skin, which they brought as a confirmation of the truth, and it has been sent to Holland.*

Dimensions of a Male Camelopardalis, killed in a Journey made in the year 1761, *through the country of a tribe of Hottentots, called the Mamacquas, viz.*

Length of the head, 1 foot, 8 inches. Height of the fore-leg from the lower to the higher point, 10 feet. From the upper part of the fore-leg to the top of the head, 7 feet. From the upper part of the fore-leg to the upper part of the hind-leg, 5 feet, 6 inches. From the upper part of the hind-leg to the tail, 1 foot 6 inches. Height of the hind-leg from the upper to the lower part, 8 feet, 5 inches.

IV. Experiments in Support of the Uses ascribed to the Ganglions of the Nerves, in the Phil. Trans., vol. 54, *and vol.* 57. *By James Johnstone, M. D.* p. 30.

Reprinted in this author's Medical Essays and Observations, 1795.

V. Of a New Species of the Manis,† *or Scaly Lizard, extracted from the German Relations of the Danish Royal Missionaries in the East Indies, of the Year* 1765, *published at Halle, in Saxony. By Dr. Hampe, F.R.S.* p. 36.

October the 14th, in the evening, a rare and remarkable animal was, in the city of Tranquebar, discovered in the wall of an oil-merchant's house, and with difficulty killed. The Mallabars call it Alungu. It somewhat resembles a large lizard, except the head and tail, which are alike as to shape, being both pointed, the former not unlike a mole's. The whole length is a German ell and $\frac{1}{8}$ long, and its breadth half an ell. The tail is half an ell long, and its broadest part a span wide. The fore feet are a quarter of an ell long, and the extremity of it a thumb in thickness. The whole body, excepting under the belly, where it is smooth for about the length and breadth of a man's hand, and under the feet, is covered with hard, strong, sharp, and bright scales, shaped like a muscle-shell, the largest of which are of the length and breadth of 8 fingers. Under its scale come out 2 or 3 hairs like hog's bristles. On its fore-claws are 5 strong long nails, on the hind-claws but 4. When pursued, it rolls itself so together, that

* The animal described in this letter is now in the cabinet of natural history at Leyden, where I have seen it this year. M. MATY.—Orig.

† This animal is the *Manis pentadactyla* of Linnæus; the Short-Tailed Manis of Pennant; and the Pangolin of Buffon.

nothing but the back and tail are to be seen. It could not be killed, though struck with wooden poles armed with iron, with which rice is stamped; but the blows on the scales brought forth sparks of fire from the iron. It was at last killed by a stroke under the belly with an iron hook. It is remarkable that this little animal is able to kill an elephant, by twisting itself about that large animal's trunk, and squeezing it with its body and tail (on the sides of which are rows of pointed scales) so long, that it kills the elephant. This animal is seldom seen, except in large valleys.

VI. On the Result of Some Attempts made to Ascertain the Temperature of the Sea in Great Depths, near the Coasts of Lapland and Norway. By Charles Douglas, Esq., F.R.S., then Captain of His Majesty's Ship the Emerald, Anno 1769. p. 39.

May the 12th, 1769, between the islands of Surey and Hammerfest, in Lapland, in the latitude of 70° 40′, between the hours of 6 and 9 p. m. the thermometer stood in the open air at 27, in the sea at the surface 36, and in three several depths, from 87 to 78 fathoms at the bottom, as often tried, at 39. May 17th, in nearly the same place, the thermometer having at noon stood in the open air at 44, stood between 7 and 9 p. m. at 38, at the surface 37; and at the bottom in the depths of 86 and 90 fathoms, having been twice tried, at 39. May 22d, in lat. 70° 32′, between the island of Hammerfest and the main land of Finmark, about 7 p. m. the thermometer stood at 40, in the open air; in the sea at the surface at 37; and at the bottom in 80 fathoms depth, at 39. June 29th in the afternoon, between the island of Maggeroe and the main land of Lapland, in lat. 70° 54′, the thermometer stood in the open air at 47; in the sea, at the surface at 44; and in 98 fathoms water, at the ground at 40. July 7th at sea, about 6 leagues distance from the island of Tromsound, in the lat. of 70° 45′, the thermometer in the open air at 46, in the sea at the surface 46, and at the bottom 70 fathoms deep 44. July 8th, in lat. 68° 43′, at the distance of 12 or 14 leagues from the island of Lofoot, in the province of Norland, the thermometer stood in the open air at 46, in the sea at the surface 47, 260 fathoms below the surface, but not at the bottom, at 52: and 100 fathoms below the surface at 46. July 9th, in lat. 65° 25′, the thermometer in the open air at 48, in the sea at the surface 48; 210 fathoms deep on the ground at 48; and 100 fathoms below the surface at 46. July 10th, in lat. 64° 40′, about 30 leagues from the coast of Norway, the thermometer in the open air, and in the sea at the surface at 52, at the grouud in 141 fathoms water, at 46; and 75 fathoms below the surface at 45.

The foregoing thermometrical experiments, made in deep water, were effected

by means of a tin cylinder, containing a large quart, with an apparatus so contrived, as to keep the thermometer standing upright in the middle, without touching its sides: thus inclosed in a case, filled with water from along side, and covered with a cap, so as to be perfectly water tight, he sunk it with the deep-sea sounding lead; letting it hang just clear of the ground for the space of half an hour, and then had it hauled up as briskly as possible, and the case being instantly opened, he inspected the thermometer. He found the inconvenience however of making the experiment in this way, because of the length of time necessary; therefore he made a very small hole in each end of the cylinder, to let the water in and the air out, and sent it down empty, that it might fill as far below the surface as possible, suffering it however always to hang a few minutes, that it might be full before he caused the boat's crew to begin hauling it up. The lead, with this apparatus fastened to the line a little above it, sunk 260 fathoms in $3\frac{1}{2}$ minutes, and was hauled up in $13\frac{1}{2}$.

VII. On the Manner in which White Marble is produced. By R. S. Raspe, F.R.S. An Abstract from the Latin. p. 47.

In this paper Mr. R. gives an account of some observations made by the Abbé Vegni on the hot mineral waters of St. Philip, situated at Radicofani, in Tuscany, on the road from Florence to Rome. 1° The Abbé traced the source of these waters to a small hill (which appeared to be entirely composed of white marble), from which they flowed in several rivulets. 2° He found that these waters abounded in sulphur. 3° He found that they deposited a great quantity of shining white tophus, with which not only the sides of the channels, along which they flowed, became incrusted, but likewise all kinds of hard bodies that were thrown into them; and this in such manner, that when the said tophus was dexterously broken off, it retained exactly the form and shape of the bodies on which it had been deposited. 4° He further observed, that when the old channels became choked up by the accumulation of tophus, or in any other manner, the water still continued to deposit tophus in its new and more elevated channels.

Hence the Abbé was led to infer, 1° That the whole of that hill, from which these hot mineral waters issued, was formed by the successive deposition of this shining white tophus. 2° That this tophaceous precipitate might be rendered subservient to the arts, provided it was caught upon moulds. Accordingly the Abbé set on foot an undertaking of this kind, which succeeded very well. Models of various kinds of sculpture formed of gypsum, well varnished and besmeared with oil or grease, being placed in the currents of these hot springs, became incrusted with tophus to the thickness of 2 lines in the space of 6 days; and in this manner were obtained bas reliefs, medallions, architectural ornaments

for doors, windows, chimneys, &c. which in many instances looked like real sculpture, and seemed to be formed of the purest Carrara marble.

Mr. Raspe adopts the opinion that all white marble is a precipitate from water like the tophus of the hot springs above mentioned.

VIII. *Account of a Very Remarkable Young Musician. By the Hon. Daines Barrington, F.R.S.* p. 54.

Joannes Chrysostomus Wolfgangus Theophilus Mozart, was born at Saltzbourg in Bavaria, on the 17th of Jan. 1756.

Mr. B. was informed, by a most able musician and composer, that he frequently saw him at Vienna, when he was little more than 4 years old. By this time he not only was capable of executing lessons on his favourite instrument the harpsichord, but composed some in an easy style and taste, which were much approved of. His extraordinary musical talents soon reached the ears of the empress dowager, who used to place him on her knees while he played on the harpsichord.

This notice taken of him by so great a personage, together with a certain consciousness of his most singular abilities, had much emboldened the little musician. Being therefore the next year at one of the German courts, where the elector encouraged him, by saying, that he had nothing to fear from his presence; little Mozart immediately sat down with great confidence to his harpsichord, informing his highness, that he had played before the empress. At 7 years of age his father carried him to Paris, where he so distinguished himself by his compositions, that an engraving was made of him.

On leaving Paris, he came over to England, where he continued more than a year. As during this time Mr. B. was witness of his most extraordinary abilities as a musician, both at some public concerts, and by having been alone with him for a considerable time at his father's house, he gives the following account, amazing and incredible almost as it may appear. He carried to him a manuscript duet, which was composed by an English gentleman to some favourite words in Metastasio's opera of Demofoonte. The whole score was in 5 parts, viz. accompaniments for a 1st and 2d violin, the 2 vocal parts, and a base. Mr. B.'s intention in carrying with him this manuscript composition, was to have an irrefragable proof of his abilities, as a player at sight, it being absolutely impossible that he could have ever seen the music before. The score was no sooner put upon his desk, than he began to play the symphony in a most masterly manner, as well as in the time and style which corresponded with the intention of the composer. The symphony ended, he took the upper part, leaving the under one to his father. His voice in the tone of it was thin and infantine, but nothing could exceed the masterly manner in which he sung. His father, who

took the under part in this duet, was once or twice out, though the passages were not more difficult than those in the upper one; on which occasion the son looked back with some anger, pointing out to him his mistakes, and setting him right. He not only however did complete justice to the duet, by singing his own part in the truest taste, and with the greatest precision: he also threw in the accompaniments of the two violins, wherever they were most necessary, and produced the best effects. It is well known that none but the most capital musicians are capable of accompanying in this superior style.

When he had finished the duet, he expressed himself highly in its approbation, asking with some eagerness whether Mr. B. had brought any more such music. Having been informed, however, that he was often visited with musical ideas, to which, even in the midst of the night, he would give utterance on his harpsichord; Mr. B. told his father that he should be glad to hear some of his extemporary compositions. The father shook his head at this, saying, that it depended entirely on his being as it were musically inspired, but that Mr. B. might ask him whether he was in humour for such a composition. Happening to know that little Mozart was much taken notice of by Manzoli, the famous singer, who came over to England in 1764, Mr. B. said to the boy, that he should be glad to hear an extempore love-song, such as his friend Manzoli might choose in an opera. The boy on this, who continued to sit at his harpsichord, looked back with much archness, and immediately began 5 or 6 lines of a jargon recitative proper to introduce a love song. He then played a symphony which might correspond with an air composed to the single word Affetto. It had a 1st and 2d part, which, together with the symphonies, was of the length that opera songs generally last: if this extemporary composition was not amazingly capital, yet it was really above mediocrity, and showed most extraordinary readiness of invention.

Finding that he was in humour, and as it were inspired, Mr. B. then desired him to compose a song of rage, such as might be proper for the opera stage. The boy again looked back with much archness, and began 5 or 6 lines of a jargon recitative proper to precede a song of anger. This lasted also about the same time with the song of love; and in the middle of it, he had worked himself up to such a pitch, that he beat his harpsichord like a person possessed, rising sometimes in his chair. The word he pitched on for this second extemporary composition was, Perfido.

After this he played a difficult lesson, which he had finished a day or two before:* his execution was amazing, considering that his little fingers could

* He published 6 sonatas for the harpsichord, with an accompaniment for the violin, or German flute, sold by R. Bremner, in the Strand, and are intitled, Oeuvre Troisieme. He is said in the title page to have been only 8 years of age when he composed these sonatas. The dedication is to

scarcely reach a 5th on the harpsichord. His astonishing readiness however did not arise merely from great practice; he had a thorough knowledge of the fundamental principles of composition, as, on producing a treble, he immediately wrote a base under it, which, when tried, had a very good effect. He was also a great master of modulation, and his transitions from one key to another were very natural and judicious; he practised in this manner for a considerable time with a handkerchief over the keys of the harpsichord. These facts Mr. B. was an eye-witness of; to which he adds, that he had been informed by two or three able musicians, when Bach the celebrated composer had begun a fugue, and left off abruptly, that little Mozart has immediately taken it up, and worked it after a most masterly manner.

Mr. B. made frequent inquiries with regard to this very extraordinary genius after he left England, and was told in 1769, that he was then at Saltzbourg, where he had composed several oratorios, which were much admired. He was also informed, that the Prince of Saltzbourg, not crediting that such masterly compositions were really those of a child, shut him up for a week, during which he was not permitted to see any one, and was left only with music paper, and the words of an oratorio. During this short time he composed a very capital oratorio, which was most highly approved on being performed.

Having stated these proofs of Mozart's genius, when of almost an infantine age, it may not be improper perhaps to compare them with what has been well attested with regard to other instances of the same sort. Among these, John Barratier has been most particularly distinguished, who is said to have understood Latin when he was but 4 years old, Hebrew when 6, and 3 other languages at the age of 9. This same prodigy of philological learning also translated the travels of Rabbi Benjamin when 11 years old, accompanying his version with notes and dissertations. Before his death, which happened under the age of 20, Barratier seems to have astonished Germany with his amazing extent of learning; and it need not be said, that its increase in such a soil, from year to year, is commonly amazing. Mozart, however, is not now much more than 13 years of age, and it is not therefore necessary to carry the comparison further.

The Rev. Mr. Manwaring, in his Memoirs of Handel, has given us a still more apposite instance, and in the same science. This great musician began to play on the clavichord when he was but 7 years of age, and is said to have composed some church services when he was only 9 years old, as also the opera of Almeria, when he did not exceed 14. Mr. Manwaring likewise mentions that Handel, when very young, was struck sometimes while in bed with musical ideas,

the queen, and is dated at London, Jan. 8, 1765. He subscribes himself, 'tres humble, et tres obeissant *petit* serviteur.' These lessons are composed in a very original style, and some of them are masterly.—Orig.

and that, like Mozart, he used to try their effect immediately on a spinnet, which was in his bedchamber.

IX. A Determination of the exact Moments of Time when the Planet Venus was at External and Internal Contact with the Sun's Limb, in the Transits of June 6, 1761, and June 3, 1769. By Samuel Dunn. p. 65.

The telescope being properly adjusted, and having a most clear and distinct sight of the sun's limb where the external contact was expected to happen; at $11^h\ 56^m\ 32^s$ per clock (which, reduced by the Astronomer Royal to apparent time, is $7^h\ 10^m\ 33^s$) while Mr. D. was moving his eye gently along that part of the sun's limb where the contact was expected, there appeared as though a kind of lucid wave of transparent matter, of the colour of that part of the lucid annulus (which afterwards appeared round Venus), which was nearest to the limb of Venus, and taking up the space of about a 5th part of a minute of a degree along the sun's edge, this lucid wave seemed to strike gently against the sun's limb, and in an instant the little tremulous vibrations on the sun's limb were totally stopped, and that part of the limb was rendered thereby a little obscure. A second and half of time after this, at the same place of the sun's limb, arose, first gently, and then more violently, a ferment, or boiling, very different in colour as well as magnitude, from the tremulous vibrations at other parts of the sun's limb, for it was darker and much more violent, and at $11^h\ 56^m\ 36^s$ per clock, or $7^h\ 10^m\ 37^s$ apparent time, this fermentation was enlarged along the limb of the sun, and the limb of Venus was entering on the sun's limb.

Mr. D.'s attention being at this time engaged in examining the place around the point of contact, he endeavoured to see a kind of brown penumbra precede the limb of Venus, but saw none; instead of it, a kind of whitish light, at first very faint, and afterwards as it advanced on the sun's disk becoming more strong, preceded the limb of the planet; which light gradually diminished nearer to Venus, and formed a narrow margin of lucid matter, by which the limb of the planet became a little ill defined. Almost the same circumstances happened in the exterior contact 1761, but with this difference, the lucid border then following, the limb of Venus was more clear and transparent. Mr. D. was the more particular in these circumstances, to be able to determine what differences might arise from observations made with telescopes and eyes equally good, and concluded from the phenomena, that two such observers, with but little inequality in their judgments, might differ from each other 10 seconds of time. Before this contact and a little after it, he endeavoured to find a faint illumination on the exterior limb of Venus; but could find none, till the time of internal contact drew near. Another circumstance attending the phenomenon was this; the limb of Venus which first entered on the sun's disk appeared to be the arch of a

very small circle, but as the planet advanced onward on the solar disk, that same preceding part of Venus appeared to enlarge and expand itself, and the subsequent part of Venus, which was on the sun's limb, appeared as though it was the portion of a smaller circle; and thus the planet appeared to the time of central ingress, at which time half the planet appeared a semi-ellipsis, the conjugate diameter forming the notch in the sun's limb. After the time of central ingress, while the latter half of the planet was passing over the sun's limb, the like appearances occurred; so that, though that circumference was really concave to Venus's centre, a little after the central ingress, it appeared a little convex to that centre, and so the planet advanced, that part of it which was nearest the sun's limb appearing contracted, but enlarging itself a little farther on the disk.

The planet being considerably past the central ingress, and being at broad black contact with the sun's limb, but of an irregular form on account of the above-mentioned circumstances, and it being hard to judge what kind of contact would appear, Mr. D. perceived a very faint luminous crescent exterior to the limb of the sun; and nearly coinciding with the preceding limb of Venus continued over the sun's limb. This crescent was very faint, but steadily defined at certain fits and returns till 7^h 28^m 30^s apparent time. When this crescent being come near to the limb of the sun it vanished, and seemed to fall in with a kind of confused slight illumination in the limb of the sun itself, where the internal contact was to happen. At the same time a kind of partial and very faint illumination took place, both a little without, and a little within the sun's limb, as well as in the limb itself, where the contact was to be, and a gentle ebullition or boiling arose a little without the sun's limb on the limb of Venus, which continued till the dark body of the planet was wholly within the sun's disk, or 7^h 29^m 28^s apparent time, when Venus's circumference was not passed coinciding with the sun's circumference above 3 or 4 seconds of time. While attentively viewing this, and judging it difficult to determine the exact moment of circular contact, on account of the circumstances described, the ebullition or boiling between the limb of Venus and the sun became more violent, and the partial illumination increased; and at 7^h 29^m 38^s he saw the planet as it were held to the sun's limb by a ligament formed of many black cones, whose bases stood on the limb of Venus and their vertexes pointing to the limb of the sun. These cones put on various positions, and as Venus advanced they alternately contracted themselves towards the limb of Venus, and expanded themselves towards the sun's limb, performing their undulations always regularly and in the same time as the planet advanced on the disk, till 7^h 29^m 48^s apparent time. At the end of this interval, the agitation or fermentation was exceedingly violent, for the whole limb of Venus would sometimes librate towards the limb of the sun, and sometimes the limb of the sun would turn convex in yielding towards Venus; but the

thread of light was not yet formed, for still 3 or 4 broad parts of the ligament never had yet broken from the sun, and therefore the thread of light was not yet formed. He carefully examined the sides of those black cones connected with the limb of the sun, and saw the fissures or spaces between them to be filled with a steady illumination, of the colour of twilight compared with the light of the sun; and while steadily attending to these circumstances, he saw the pure and genuine light of the sun break in between some of those fissures like streaks of lightning, which made the partial light become in 2 or 3 seconds of time, of the same colour as the light of the sun, yet still the undulating ligament, though reduced, was not broken. And now,

In an instant, the northern part of the divided ligament withdraws itself from the sun's limb about half way towards Venus, and instantly but gently it returns and again unites the limbs of the sun and Venus; instantly after, another less northern part of the ligament does the like, and then breaks off again, and so does each part of the divided ligament, till 7^h 29^m 51^s apparent time, when the ends or vertexes of the black cones between Venus and the sun's limb appear to be separated from the sun's limb, retreating to that of Venus, and dissolving or dying away like a drop of tinge thrown into water, and now the thread of light becomes complete.

The internal contact being past, and Venus being wholly on the sun, Mr. D. examined the space surrounding Venus, and saw such a lucid annulus around the planet as appeared in 1761. The part of this annulus next to the limb of Venus appeared a little dusky, but much more clear than in 1761, when it appeared more confused, and as a penumbra; but that part of the annulus farthest off from the circumference of Venus appeared a little tinged with blue. The breadth of the annulus about 5 or 6 seconds. On hearing a gentleman in the lower apartment call out to be showed the atmosphere of Venus, Mr. D. now left his telescope, went down stairs to Mr. Nairne and Mr. Dollond, and desired them to be attentive at their telescopes, and they would see this shining annulus, which they attended to, and after a little while saw it plainly, though for some time they could not perceive any such thing. Then the other gentlemen present also saw it.

These observations being made, Mr. D. states the first external contact at 7^h 10^m 37^s apparent time for Greenwich. Circular contact internally at 7^h 29^m 25^s. Completion of the thread of light at 7^h 29^m 48^s, under the circumstances above described.

Though he would not willingly form any hypothesis from the aforementioned phenomena, there is one of them, namely, the appearance of the well defined streaks of light between the fissures, which seems accountable for thus. The partial light which preceded it, he takes to be rays scattered by refraction and

reflection through that part of the planet's atmosphere where the contact was to happen; and the well-defined streaks of light following it, he takes to have been the sun beams passing between mountains on the surface of Venus's globe.

X. *Of some Improvements made in a New Wheel Barometer, Invented by Keane Fitzgerald, Esq., F. R. S.* p. 74.

Mr. F. gave a former description of a wheel barometer of a new construction, with registers to mark the rise and fall of the mercury, which were published in the 52d vol. of the Philos. Trans. for the year 1761. And he here offers further improvements of the same, which are described.

The exactness and facility with which an account of the variations in the weight of the atmosphere may be kept, with the help of a barometer of this kind, Mr. F. thinks must be very evident. He often found, by extraordinary variations that have happened in the night, when the wind has risen considerably, how little the observations made with common barometers are to be depended on; and several times found by the registers, that the mercury had sunk 50 or 60 divisions; and one night particularly had sunk 117 degrees, and returned within a degree and half of the place he had marked it on going to bed. When a strong gust of wind rises, one may very plainly perceive the index of this latter barometer to sink several divisions, and rise again as it abates. Besides the satisfaction that a barometer of this kind might afford to a curious observer, Mr. F. imagines it might also be usefully applied to the finding the height of the atmosphere; with a much greater degree of exactness, at least, than can well be afforded by any other. It is generally allowed from experiments, that a column of air 72 feet high is equal in weight to 1 inch of water of the same base; so that, if the air were of equal density throughout, the atmosphere could be little more than 5 miles high. But as the density is found to decrease by the difference of pressure, and the air to be more rarefied or expanded in proportion to its distance from the earth, it seems reasonable to conclude, that if by accurate experiments, the ratio of its decrease were found regular in proportion to the distance from the earth, its height might be estimated with a much greater degree of precision than it has been hitherto; though it seems generally allowed that its real height cannot possibly be ascertained. The impossibility of observing the difference of the pressure of the atmosphere at small distances with accuracy by a common barometer, the scale of which is but 3 inches, is very evident; how far this instrument, the scale of which is 90 inches, might be conducive to the purpose, is submitted to the judgment of others. Mr. F. imagines it would not be difficult, with a proper teakle, to raise a barometer of this kind gently, as high as 200 feet; and if it were raised from the ground, and let down again from each distance of 20 feet, the registers would mark very exactly to the 600 part of an

inch at what height the mercury stood at each distance; so that the weight of each column of air of 20 feet, to the height it could be raised, would be found pretty exactly. And if a proper apparatus were fixed for raising the barometer, the experiments might be repeated, as often as requisite, with very little trouble.

XI. Observations on an Inedited Greek Coin of Philistis, Queen of Syracuse, Malta, and Gozo, who has been passed over in Silence by all the ancient Writers. By the Rev. John Swinton, B.D., F.R.S. p. 80.

The ancient piece Mr. S. proposes to consider here, has a place in the very valuable collection of the Rev. Mr. Godwyn, Fellow of Balliol College, Oxford, who has been possessed of it several years. It exhibits on one side the same veiled head of a woman that occurs on a coin of Gozo, before described; and on the other the figures forming the type, or symbol, on the reverse of that coin. Before the face of the veiled head, on Mr. Godwyn's piece, is the Greek word ΒΑΣΙΛΙΣΣΑΣ; and on the reverse the name ΦΙΛΙΣΤΙΔΟΣ, PHILISTIDIS, in the exergue. The medal is of nearly the size of the middle Roman brass, or rather of some of the Syracusian brass coins of the middle form. The head on the anterior part is tolerably well preserved, but the type on the other is in much the same shattered condition as that exhibited by a coin of Gozo, considered in a former paper, both of them having suffered not a little from the injuries of time. In fine, were it not for the legends, in different languages and characters, these two ancient pieces would agree in all respects, and might be considered as duplicates of the same medal. However, the Greek word on the anterior part, and the Greek name on the reverse, will sufficiently, he apprehends, announce the piece in question an inedited coin.

From this medal, in conjunction with those of Gozo, published by the Marquis Scipio Maffei, Sig. Abate Venuti, and the R. S. and that of Malta to be met with in M. Spon, it will most evidently follow, that they are all coins of Philistis; and that this princess was queen of Malta and Gozo, when these islands were under the domination of the Greeks, and occupied by them and the Phœnicians. Which if we admit, it will further follow, that all those pieces were struck before the Carthaginians were possessed of Malta and Gozo. For the settlement of the Phœnicians in those islands was undoubtedly prior to that of the Greeks, and the Carthaginians succeeded the latter in the occupation of them. The medals therefore of Gozo by Mr. S. formerly named Punic, since the discovery of Mr. Godwyn's coin, he would rather denominate Phœnician, as being struck when the Phœnicians remained in that island. This seems to have been suspected by Sig. Abate Venuti, when he affirms the piece so perfectly similar to Mr. Godwyn's to be a Phœnician medal, or at least one of the most ancient Carthaginian coins; but, by Mr. Godwyn's piece, it is rendered abso-

lutely incontestable. We may therefore conclude, that the princess whose head appears on all these medals was queen of Malta and Gozo, before the Carthaginians were settled in either of those islands; though the time when she swayed the sceptre there cannot, for want of sufficient light from ancient history, with any precision, be so easily ascertained. But, from various circumstances, we may, he apprehends, safely enough place queen Philistis in the interval between Dyonysius I. and Gelo, kings of Syracuse, and even somewhere near the earlier of those princes, as her silver medals so much resemble Gelo's silver coins. If therefore it should be supposed probable, that the pieces of Gozo, adorned with Phœnician letters, were struck in that island, about 450 years before the commencement of the Christian æra; the learned would not, Mr. S. flatters himself, refuse their assent to such a supposition.

XII. A Letter from Mr. Tho. Woolcomb, Surgeon, on the Case of a Boy, who died of a Gun-shot Wound. p. 94.

Dec. 17, 1763, in the forenoon, Mr. W. was sent for to the assistance of John Kitt, a lad of about 15 years of age, who had just received a considerable wound by the unexpected going off of a gun loaded with small shot, held near his arm. He found the shot, by being so near, had acted altogether as a slug, had lacerated much, and made a pretty large perforation through the biceps and brachiæus internus muscles, had bared the os humeri, and in fine penetrated quite through the arm from below upwards.

By the time Mr. W. arrived, which was almost immediately after the accident, he found little or no hæmorrhage, which made him hope the humeral artery had not been divided. On examining the wound, and finding no extraneous substances lodged, but the passage quite pervious to the probe; he dressed up with dry lint, digestive, &c. ordering the whole limb to be wrapped up in a warm poultice made with oatmeal, stale beer, and a good deal of oil. Returning in the evening, he found the patient tolerably easy, but applying his fingers to the artery of the wrist of the same hand, was not a little alarmed to find he could not perceive the least pulsation. It was but too easy to apprehend the cause of it; that in all probability the artery was divided, and if so, the limb perhaps would not be saved.

Mr. W. made his report to the friends accordingly. However, as no threatening symptoms attended, he was willing to see whether, if the artery was divided, the blood might not, as after the operation of the aneurism, find a passage by the collateral branches, and thereby the circulation be kept up. He was apt also to think, as there had been no hæmorrhage of the wound, that it might not be divided, but the course of the circulation be impeded only by some spasmodic constriction, which possibly by the morning might relax and give way; at

all events, he judged it most prudent to wait. He dressed up therefore with a little warm digestive, after properly fomenting the limb, and ordered the cataplasm to be renewed as before. Little or no tension had yet taken place; yet, in order to obviate that, and the symptomatic fever that might be expected, and finding the pulse began to rise, he ordered him to be bled about ℥xvj, and left him with a tourniquet put loosely round the arm, with proper directions to the attendants, for fear of any sudden rupture of the blood-vessel in the night.

The next morning he found him tolerably easy, but the pulse very quick and strong, and still no pulsation in the wrist of the wounded arm. The aspect of the wound very good, no tension round. However, as it was so nice a point to determine, whether the artery was or was not divided, and of consequence whether it would be more prudent, on the supposition it was, to proceed to amputation, or any longer run the risk of a mortification's ensuing; he judged it proper to have other opinions, and for that purpose, called in 3 surgeons of credit in the town.

They were all of opinion, as there were no imminent symptoms, it was best still to wait; judging rightly, that if a mortification took place only through defect of the blood's circulation in the lower limb, it might easily be remedied by amputation above, time enough when it first made its appearance. He accordingly dressed up in the same manner, but had the patient bled again to ℥x or ℥xii, and gave a gentle lenitive, which procured a few stools. In the evening symptoms were much the same; pulse still strong and quick; bleeding was therefore repeated. The next day every thing seemed to take a favourable turn, the pulse grew much more calm, a good digestion came on, no tension at all was observed on the limb, and in this kindly manner they went on for 3 or 4 days. Though all this time not the least pulsation could be felt on the wounded limb, there was always a kindly natural warmth on it, and the patient made no other complaint than of a numbness and deadness of his little and ring-finger.

By all these favourable circumstances Mr. W. was induced to hope all danger had now been over, when about the 5th or 6th day from the accident, the appearance of the wound began to alter, and to look of a pale leucophlegmatic hue; the discharge became much more thin and serous, and very considerable fungi grew out from the surface of each wound; the whole limb both above and below the wound became greatly enlarged, the hand and fore-arm perfectly œdematous; the pulse quick and small, the countenance, from a fresh florid hue, sunk, pale and sallow. These alarming symptoms coming on, gave him the greatest reason to be apprehensive of the event. To obviate them as much as possible, he ordered the cortex both in decoction and substance to be administered every hour or two, and had fresh consultations with the other surgeons. It was not now practicable to amputate, as the distension of the limb extended quite to the

axilla. He therefore continued the use of the fotus, warm dressings, &c. as before; strewing over the fungi well with the pulv. angel. Yet they continued to sprout to a great height, and, though he pared away at every dressing all the dead surface with the knife, they baffled all endeavours to suppress them.

In this manner it continued to go on till the first of January, in the afternoon of which the patient began to complain greatly of being cold; and, though the warmest and most invigorating medicines were given, he grew more and more so, till about 11 or 12, when he expired. Neither before nor after death was there the least appearance of a mortification having taken place. However, in order, if possible, to investigate the true cause of his death, and to satisfy themselves whether the artery was or was not divided, in the presence of the other surgeons, Mr. W. laid open the wounded parts, and passing a probe through the artery at a transverse incision made above the wound, carefully dissected away the surrounding integuments, and thereby discovered a perforation (about the size of a small pea) made through the coats of one side of the artery. They were all at a loss to account, why there never ensued any hæmorrhage from so considerable a vessel's being opened, as no eschar could well have formed, nor yet appeared there any constriction or compression; and yet it appeared as plain, that the course of the blood was thoroughly intercepted in that vessel, by there never being the least pulsation at the wrist after the accident.

The cause of his death too at last seems to be pretty unaccountable, as no mortification ensued, which one would have expected to have been the natural consequence of the blood's being so intercepted. If, owing to the shock given the constitution, or remora to the circulation, should not one have expected the ill consequences would have been felt sooner? whereas, for nearly the first week, no patient with so considerable a wound could go on better, no wound could have a better aspect, or digest better. By the repeated bleeding, lenient cathartics, and proper topical relaxing applications, all degree of tension was happily kept off, little or no symptomatic fever attended, and seemingly every ill symptom was obviated. In what manner then shall we conclude death at last to have been brought about so long after, since he neither sunk under discharge from the wound, had no fever or convulsion, and no mortification ever appeared? and what shall we assign to be the true reason of no hæmorrhage ensuing, since there was so manifest an aperture through the coats of the artery? This he confesses himself wholly at a loss to account for.

XIII. Journal of a Voyage, made by Order of the Royal Society, to Churchill River, on the North-west Coast of Hudson's Bay; of 13 Months Residence

in that Country; and of the Voyage back to England; in 1768 *and* 1769. *By Wm. Wales,* p. 100.

It must be observed, that the astronomical, and not the nautieal day, is every where to be understood in the following Journal.

This sea-journal is now very uninteresting. The party sailed from the river May 31, 1768; and July 23 arrived at the island of Resolution, which forms the north-shore at the entrance of Hudson's Straits, where the variation of the needle was found 39° 48′ west.

One day as Mr. W. was observing the sun's meridional altitude, there eame along side 3 Eskimaux in their canoes, or, as they term them, Kiacks, but who had very little to trade, except toys. None of these had along with them any weapon, except a kind of dart, evidently constructed for sea purposes, as it had a buoy fixed to it, made of a large bladder blown up. The men had on their legs a pair of boots, made of seal-skin, and soled with that of a sea-horse; these came barely up to their knees; and above these they had breeches made of seal, or deer-skin, much in the form of our seamen's short trowsers. The remaining part of their cloathing was in one piece, much in the form of an English shift; only coming just below the waist-band of their breeches, and had a hood which serves instead of a cap. Over these they wore a kind of foul-weather jacket, made of the same leather with the legs of their boots, and fastened tightly about their necks and wrists; and when they are in their kiacks, are likewise fastened in such a manner round the circular hole which admits the man's body, that not the least drop of water can get into it, either from rain or the spray of the sea. The dress of the women differed not from that of the men, excepting that they had long tails to their waistcoats behind, which reached down to their heels; and their boots eame up to their hips, which are there very wide, and made to stand off from their hips with a strong bow of whalebone, for the convenience of putting their children in. He saw one woman with a child in each boot top.

As to their persons, they seem to be low; but pretty broad built, and inclined to be fat: their hands remarkably small; their faees very broad and flat; very little mouths, and their lips not remarkably thiek; their noses small, and inclined to what is generally termed bottled; their eyes are black as jet, and their eye-lids so encumbered with fat, that they seem as if they opened them with difficulty; their hair is black, long, and straight; and though they seem encumbered with a superfluity of flesh, they are remarkably brisk and active; more especially in the management of their kiacks, which exceeds every thing of the kind that he ever saw. All he can say with regard to their disposition is, that if they really deserve the character which authors have given of them, they are the most complete hypocrites that nature ever formed. Mr. W. then observes

as follows: "I have had, whilst at Churchill, a good opportunity of learning the disposition of those people; as several of them came almost every year, by their own free will, to reside at the factory; and can with truth aver, that never people less deserved the epithets of "treacherous, cruel, fawning, and suspicious;" the contrary of which is remarkably true in every particular. They are open, generous, and unsuspecting; addicted too much, it must be owned, to passion, and too apt to revenge what they think an injury, if an opportunity offers at that moment; but are almost instantly cool, without requiring any acknowledgement on your part, which they account shameful, and I verily believe, never remember the circumstance afterwards. Mr. Ellis observes, "That they are apt to pilfer from strangers, easily encouraged to a degree of boldness; but as easily frightened." Now I cannot help thinking that he would have conveyed a much better idea of them if he had expressed himself thus: They are bold and enterprising even to enthusiasm, while there is a probability of success crowning their endeavours; but wise enough to desist, when inevitable destruction stares them in the face. Perhaps few people have a greater genius for arts, which shows itself in every one of their implements, but particularly in their boats, harpoons, darts, bows and snow-eyes, which last are most excellently contrived for preserving the eyes from the effect of the snow in the spring. But a volume might be written on these subjects, and perhaps not unentertaining.

I beg leave to mention, says Mr. W., what I apprehend to be a mistake in Crantz's history of Greenland, where he says that those pieces of ice which are of a vitriol colour are salt, and consist of salt water frozen to ice; but I can, from my own experience, assert, that when the salt water, which they catch by the sea washing over them, is wiped clean off, they are entirely fresh. I will not take upon me to say that they are not made from salt water; but if they are, it must have deposited all its salts before it was frozen to ice.

July 27, Mr. W. counted 58 islands of ice, all going directly across the Straits from the mouth of the above-mentioned inlet, at the rate of several miles per hour. From this one circumstance, says Mr. W. we have an irrefragable argument to prove the impossibility of Capt. Middleton's hypothesis, relating to the very slow progressive motion of these islands, and the long time which they take up in dissolving. For, admitting his hypothesis to be true, and that there were no other islands of ice but what came out of this bay; not only Hudson's Straits, but even all the adjacent sea would in a very few years be so entirely choaked up with them, that it would be impossible to force a ship among them, could a master of one be found so imprudent as to venture; which must be inevitable destruction. The truth is, their motion and dissolution are apparently so very quick, that I am of opinion it must be a pretty large island which is not dissolved in one summer. How Capt. Middleton could drop into such a pal-

pable mistake, is very difficult to say: he most certainly had as great an opportunity of informing himself of the truth of what he wrote on this subject, as any person whatever; and in this case had not the least inducement, whatever he might be thought to have in others, to speak contrary to his knowledge.

July 29 they hauled the wind to the southward, the ice being quite thick a-head. At 19^h hauled the wind to N. W. and stood through the ledge of ice, as it appeared to reach quite to Cape Walsingham, which now bore S. W. It consisted of large pieces close jambed together: in the place where they attempted to pass through, it was not quite so close. It is really very curious to see a ship working among ice. Every man on board has his place assigned him; and the captain takes his in the most convenient one for seeing when the ship approaches very near the piece of ice which is directly a-head of her, which he has no sooner announced, than the ship is moving in a quite contrary direction to what it was before, by which it avoids striking the piece of ice, or at least, striking it with that force which it would otherwise have done. In this manner they turned the ship several times in a minute; the wind blowing a strong gale all the time.

August 7, about 5, saw the low land of Cape Churchill, bearing from the S. to S. W. b. S. but the haziness of the horizon made the land put on a different appearance every 4 or 5^m. I cannot help taking notice of one circumstance, says Mr. W., as it appears to me a very remarkable one. Though we saw the land extremely plain from off the quarter deck, and as it were lifted up in the haze, in the same manner as the ice had always done; yet the man at the mast-head declared he could see nothing of it. This appeared so extraordinary to me, that I went to the main-top-mast-head myself to be satisfied of the truth; and though I could see it very plainly both before I went up, and after I came down, yet could I see nothing like the appearance of land when I was there. I had often admired the singular appearance of the ice in these parts, which I have seen lifted up 2^0 or 3^0 at a distance of 8 or 10 miles, though when we have come to it, we have found it scarcely higher than the surface of the water. On the 8th of Aug. they arrived at the Factory in Churchill River, their desired station. After breakfast, on the 10th, the surgeon of the factory was so kind as to walk with them several miles, to show them the country. The soil, as far as they went, consisted entirely of high bare rocks, or loose gravel: among the latter, there shoots up, in the lower places, many dwarf willows, and birch; in the higher ones some small gooseberry bushes; but these do not grow upright as in England, but creep along the gravel like the bramble brier. They saw besides these some strawberries, many cranberries, and a few bilberries; but none of these were yet ripe, except a few of the last. They also saw some few plants creeping among the moss; but none that they knew, except the dandelion and small yarrow.

They saw some wild ducks and curlews, but could handle none of them: they shot a few birds, much about the size, colour, and make of a woodcock: these they call here stone-plover. They saw another bird, not much unlike a quail, which they call here the whale-bird, from its feeding on the offal of those fish after the oil is boiled out of it. Besides those, they saw many, and great variety, of the gull, or sea-mew kind; and also of small birds, like our linnets, larks, &c. But the most extraordinary bird yet met with is, Mr. W. knows not for what reason, called a man-of-war, and feeds on the excrements of other birds; its way of coming at its food is also a little extraordinary; he pursues the bird which he pitches on for his supply, until fear makes it void what he wants, and so soon as this happens, he catches the morsel in his mouth; after which he leaves that bird and pursues another.

Mr. W. found here 3 very troublesome insects. The first is the moschetto, too common in all parts of America, and too well known, to need describing here. The second is a very small fly, called (he supposes on account of its smallness) the sand-fly. These in a hot calm day are intolerably troublesome: there are continually millions of them about one's face and eyes, so that it is impossible either to speak, breathe, or look, without having one's mouth, nose, or eyes full of them. One comfortable circumstance is, that the least breath of wind disperses them in an instant. The third insect is much like the large flesh-fly in England; but, at least three times as large: these, from what part ever they fix their teeth, are sure to carry a piece away with them, an instance of which he had frequently seen and experienced.

August 11th, 12th, 13th, 15th, 16th, 17th, and 18th, they got on shore the observatory and instruments; but the people were all so busy unloading the ship, and repairing the quay, craft, &c. that they could not begin to put any part of the observatory up.

♂ The 16th, Mr. W. went with Mr. Fowler about ten miles up the country, which, as far as they went, was nothing but banks of loose gravel, bare rocks, or marshes, which are over-flowed by the spring tides, and do not get dry before they return, and overflow them again. Their errand was, to see if they could not find some sand likely to produce corn; and in all that extent they did not find one acre, which was likely to do it. In some of the marshes the grass is very long, and with much labour they cut and dry as much hay as keeps three horses, two cows, a bull, and two or three goats, the whole winter. He saw many acres of land covered with fir-trees, some of which might be perhaps about 20 feet high: these grow chiefly on the borders of the marsh-lands, or, which is the same thing, round the skirts of the rocky parts. He saw no other wood, of any kind, that would bear the name of trees; but, except where the rocks are entirely bare, or where the ground is covered with water every tide, it

is entirely covered with low bush-wood, after they get a few miles from the factory. These shrubs consist of willows of many kinds, birch, juniper, gooseberry, and black currants. He saw several plants, very different from any which he ever saw in England.

Mr. W. gives the following short abstract of the circumstances of their residence at Churchill in Hudson's Bay. They arrived at Churchill just in the height of what is called the small bird season, which consists of young geese, ducks, curlews, plovers, &c. This begins about the latter end of July, and lasts till the beginning of September, when the greater part of these birds leave that part of the country. The geese then begin to go fast to the southward, and continue to do so until the beginning of October. This is called the autumnal goose-season, in which every person, both native and European, that can be spared, is employed; but they seldom kill more geese at this time than they can consume fresh. By the middle of October the ground is generally covered with snow. The partridges then begin to be very plentiful; and as soon as that happens, the hunters repair to such places as they think most probable to meet with plenty of game in. The English generally go out in parties of 3 or 4, taking with them their guns, a kettle, a few blankets, a buffalo, or beaver skin coverlid, and a covering for their tent; which is made of deers skins, dressed by the natives, and sewed together, so as to make it of a proper form and size. In pitching their tents, they have an eye also to their own convenience with respect to shelter from the winds, and getting of fire-wood; which, it will easily be imagined, makes a considerable article here in the necessaries of life, at this season of the year.

Much about this time, those who stayed at the factory began to put on their winter rigging; the principal part of which was their toggy, made of beaver skins: in making of which, the person's shape, who is to wear it, is no further consulted, than that it may be wide enough, and so long that it may reach nearly to his feet. A pair of mittens and a cap, of the same, are all the extraordinary dress that are worn by those who stay at the factory, unless we add a pair of spatter-dashes, made of broad cloth, which are worn over the common stockings, and 2 or 3 pair of woollen socks, for the feet. Those who go out add to the fur part of their dress a beaver-skin cap, which comes down, so as to cover their neck and shoulders, and also a neckcloth, or cravat made of a white fox's skin, or, which is much more complete, the tails of two of these animals sewed together at the stump-ends, which are full as long and thick as those of the Lincolnshire wethers before they are shorn. Beside these, they have shoes of soft-tanned moose skin, and a pair of snow-shoes about 4 feet, or $4\frac{1}{2}$ feet long. Most of these articles of dress, says Mr. W. I was furnished with by the Hudson's Bay company; but my chest was broken open, after the ship came

up the river, and every article, except the snow shoes, taken away by the officers of the customs. And though there was not one thing which was not an article of dress; and though a petition was preferred to the commissioners, in favour of Mr. Dymond and myself, yet, for some reason or other, they could not be restored.

But, to return to Hudson's Bay. November the 6th, the river, which is very rapid, and about a mile over at its mouth, was frozen fast over from side to side, so that the people walked across it to their tents: also the same morning, a half pint glass of British brandy was frozen solid in the observatory. Not a bird of any kind was now to be seen at the factory, except now and then a solitary crow, or a very small bird about the size of a wren; but our hunters brought us home every week plenty of partridges and rabbits, and some hares; all of which are white in the winter season; and the legs and claws of the partridges are covered with feathers, in the same manner as the other parts of their bodies. They now killed two or three hogs which Captain Richards had been so kind to leave with the governor, which before they were well opened, and cut into joints, were frozen like a piece of ice, so that they had nothing to do but hang them up in a place where they would remain in that state, and use them when they thought proper. They used some of these in the month of May, which were as sweet as they were the moment they were killed, and much more tender and delicate. One thing however must be observed, that if you roast them on a spit, or cut them in any manner while roasting, all the gravy will run out immediately.

In the fore part of December, Mr. W. went to one of the hunter's tents, where he stayed near a week. When he was there, he was told by one of the people, that they had a spring very near them, which was not yet frozen over, though the sea was frozen up as far as could be seen, and the ice in the river was 4 or 5 feet thick. He went to see it; but that morning the frost had been so very intense; that it was frozen over about an inch thick; when they broke the ice, the water was so shallow, that they raised all the mud from the bottom; and yet other springs, that were at least 6 times its depth, had been frozen quite dry several weeks.

In January 1769, the cold began to be extremely intense: even in their little cabbin, which was scarcely 3 yards square, and in which they constantly kept a very large fire; it had such an effect, that the little alarm clock would not go without an additional weight, and often not with that. The head of Mr. W.'s bed-place, for want of knowing better, went against one of the outside walls of the house; and though they were of stone, near 3 feet thick, and lined with inch boards, supported at least, 3 inches from the walls, the bedding was frozen to the boards every morning; and before the end of February, these boards were

covered with ice almost half as thick as themselves. Towards the latter end of January, when the cold was so very intense, he carried a half-pint of brandy, perfectly fluid, into the open air, and in less than 2 minutes it was as thick as treacle; in about 5, it had a very strong ice on the top; and he believes that in an hour's time it would have been nearly solid. About the beginning of December they began to use spirits of wine for the plumb-line of the quadrant, which would have been evaporated to about half the quantity in a fortnight's time, the spirituous part shooting up the plumb-line and sides of the glass, like coral; but perfectly white. What remained would then freeze, but not before. At the beginning of the winter Mr. W. hung a small vial with about a tea-spoonful of proof spirits of wine by the thermometer, on the outside of the observatory, and when he had well corked it up, dropped some water on the cork, which was instantly frozen to ice, and thus sealed the vial, in a manner hermetically. This, though it hung all the winter, never froze; nor, that he could perceive, altered its fluidity in the least.

It was now almost impossible to sleep an hour together, more especially on very cold nights, without being awakened by the cracking of the beams in the house, which were rent by the prodigious expansive power of the frost. It was very easy to mistake them for the guns on the top of the house, which are 3 pounders. But those are nothing to what we frequently hear from the rocks up the country, and along the coast; these often bursting with a report equal to that of many heavy artillery fired together, and the splinters are thrown to an amazing distance.

March 19th, it thawed in the sun, for the first time, and on the 26th it thawed in reality. The yard of the factory was that day almost covered with water. After this, it continued to thaw every day about noon when the sun was out; and by the 23d of April, the ground was in many places bare. On the 26th it rained very fast, almost the whole night, which was the first rain we had after October the 3d, 1768. It was really surprizing next morning to see what an alteration it had made in the appearance of the country. We had now alternately snow and rain, frosts and thaws, as in England; the grass began to spring up very fast in the bare places, and the gooseberry bushes to put out buds; in short, they began to have some appearance of spring.

The latter end of April, the hunters began to come home from the partridge tents, in order to prepare for the spring goose season, which is always expected to begin about that time; and is, in truth, the harvest to this part of the world. They not only kill, so as to keep the whole factory in fresh geese for near a month, but to salt as many as afterwards make no inconsiderable part of the year's provision. There are various sorts of the geese, as the grey-goose, the way-way, the brant, the dunter, and several more. The gander of the dunter

kind is one of the most beautiful feathered birds ever seen, its colours being more bright and vivid than those of the parrot, and far more various.

Toward the latter end of May, the country began to be really agreeable; the weather being neither too hot, nor so cold, but that one might walk any where without being troubled with any disagreeable sensation: and the dandelion, having grown pretty luxuriant, made most excellent sallad to our roast geese. On June 16th, the ice of the river broke up, and went to sea; we now set our nets, and caught great plenty of fine salmon; Mr. W. has known upwards of 90 caught in one tide. They had besides, fishermen up the river, who brought down plenty of pyke, mathoy, and tittymeg; these last two being fish peculiar to this country, and both very good. But, in enumerating the fish, he must not omit the kepling, which comes about the middle of July. This fish is nearly of the size of a smelt, and has exactly the same smell; but its back is much darker, and it is not quite so thick as a smelt in proportion to its length, more especially toward the head: according to his opinion, it exceeds in point of delicacy every other fish whatever, and is in such plenty, that they are thrown up, and left on the shore by the surf of the sea; but then it must be owned that this rarity can never be had above a fortnight in a year, and sometimes not so long. This fish is well known on the banks of Newfoundland. About the beginning of July they also got plenty of very fine radishes; and the tops of the turnips began to grow large enough to boil for greens to their beef and salt geese. Also, towards the middle, they had very fine lettuce, so that if the muschettos had not paid them a visit about the beginning of the month likewise, the last 2 or 3 months would have been extremely agreeable; but taking altogether, he thinks that the winter is the more agreeable part of the year.

Mr. W. then adds such remarks as he had been able to make, relative to the natural history of the country; its inhabitants, soil, air, produce, &c. And first with respect to the inhabitants: they are of a middle size, but rather tall than otherwise; very spare and thin; he never saw one, either man or woman, inclined to be fleshy; they are of a copper colour, with wide mouths, thick lips, and long, straight, black hair; of which they are immoderately fond, and would not have it cut, except on the death of a friend, for any thing that you can give them; their eyes are black, and the most beautiful ever seen. The rest of their features vary as those of Europeans do. Their disposition seems to be of the melancholic kind; good-natured, friendly, and hospitable to each other, and to the Europeans; and he believes the most honest creatures that are any where to be met with. They do not readily forget an injury; but will never revenge it when they are sober. They have no laws to regulate their conduct, except that of reason; which, in their sober moments, they are seldom known to transgress. They converse extremely well on subjects which they understand, and are re-

markably cleaver in repartees; but seem to have very little genius for arts or science. They lead an erratic life, living in tents, as all people must do whose subsistence depends entirely on hunting.

They are not without some notion of religion, but it is a very limited one. They acknowledge two Beings; one the author of all good, the other of all evil. The former they call Ukkemah, which appellation they give also to their chiefs; and the latter they call Wittikah. They pay some sort of adoration to both, though it is difficult to say what. Their opinion of the origin of mankind is, that Ukkemah made the first men and women out of the earth, 3 in number of each; that those, whom we Europeans sprang from, were made from a whiter earth than what their progenitors were, and that there was one pair of still blacker earth than they. They have likewise an imperfect traditional account of the deluge; only they substitute a beaver for the dove.

With respect to the soil and its produce of the vegetable kind, Mr. W. can add very little to what was said on his first coming on shore. As to corn, he is well convinced, that about Churchill it will produce none, except oats: those, from a trial which he had seen, he believes might be brought to some tolerable degree of perfection in time, and with proper culture. Its internal contents are, he believes, chiefly rocks; they are, however, many of them marble, and some very fine. He had also specimens of copper, copper ore, mundic, spars, talc, (different from the Muscovite) and several pyrites.

The air in this country is very seldom, if ever, clear for 24 hours together; but they were not so much troubled with fogs as he expected from the accounts he had read of the country, and from what was experienced in the voyage out.

There is a haze continually found near the horizon here. This he apprehends is the cause why the sun's rising is always preceded by two long streams of red light, one on each side of him, and about 20° distant from him. These rise as the sun rises; and as they grow longer, begin to be inflected towards each other, till they meet directly over the sun, just as he rises, forming there a kind of parhelion, or mock-sun. These two streams of light seem to have their source in two other parhelia, which rise with the true sun; and in the winter season, when the sun never rises out of the above-mentioned haze, all three accompany him the whole day, and set with him in the same manner that they rose. Mr. W. had once or twice seen a 4th parhelion directly under the true sun; but this is not common.

The aurora borealis, which has been represented as very extraordinary in those parts, bears, in his opinion, no comparison to what he had seen in the north parts of England. It is always of the same form here, and consists of a narrow, steady stream of a pale straw-coloured light, which rises out of the horizon,

about E.S.E., and extends itself through the zenith, and vanishes near the horizon, about the W.N.W. It has very seldom any motion at all; and when it has, it is only a small tremulous one on the two borders. On the 7th of August they took their departure from the Factory, and sailed on their return homewards. The latitude of the factory Mr. W. makes 58° 55′½.

The prodigious difference between the latitude of Churchill factory, as laid down from observations made by Hadley's quadrant, and that deduced from the observations made with the astronomical quadrant on shore, has often employed Mr. W.'s most serious attention; but he cannot think on any probable cause for such difference, unless it lie in the very great refractive power of the air in these parts. He has mentioned how the ice and land appear to be lifted up, when persons stand on the ship's deck: and if the visible horizon be lifted up in like manner, it must make its apparent distance from the sun, or, which is the same thing, the sun's apparent altitude, less than it otherwise would be; and consequently, the latitude greater than the truth; and also greater than it will be shown by a land quadrant, which depends not on the horizon, agreeable to what is found in the case before us.*

Before quitting this part of the world, Mr. W. observed that he had abundant reason, in his voyage home through Hudson's Straits, and the adjacent seas, to rest satisfied with having ventured his opinion in respect to the quick motion, or swift dissolution, of the ice islands. For after they left the Straits they had not seen one; and though they were becalmed, and much troubled with contrary winds, so that they lay beating from side to side about 9 days in the Straits, yet they did not see 20 islands the whole time, and these none of them very large. Whereas, was Capt. Middleton's hypothesis true, and they were some hundreds of years dissolving, and travelling into the latitude of 50°, they could not have got by this time quite out of Hudson's Straits, much less out of the Straits of Davis.

Oct. 11, at noon, the Lizard light-houses bore N. E. by N. dist. by estimation

* Having mentioned this circumstance to the Rev. Mr. Maskelyne, it immediately occurred to him, that the longitude deduced from observations of the ☽'s distance from the sun or a star, would be considerably affected by this cause, as not only the altitudes of the ⊙, from whence the time at the ship is found; but also the latitude of the ship, found by an observation of the sun's meridional altitude, or otherwise, will conspire to increase the sun's distance from the meridian, or angle at the pole. Mr. W. therefore recomputed the longitude from his observation of the moon's distance from the sun, taken August 5th, 1768, on a supposition that the mean error in any altitude taken by Hadley's quadrant, arising from this cause, is 10 minutes; and found that on such a supposition, which it must be allowed appears to be extremely well founded, the longitude will be 11′¼ less than what he found it at the time when he made the observation, and therefore the longitude of Churchill will in this case be only 94° 50′¾ W. And by making a similar correction of 15′ to Mr. Dymond's observation of the 6th, it will give the longitude of Churchill 95° 18′ W.—Orig.

about 8 miles; and from hence Mr. W. infers that its longitude west from Greenwich is by account 4° 27′. By his first observation 5° 8′, by the second 4° 29′½, and by the last 4° 48′. The true longitude of this place as determined by Mr. Bradley's observations made there (vide preface to the Nautical Almanac of 1771) is 5° 15′ w. and therefore the greatest error that he had committed in these, is 45′½, and the mean of the three differs no more than 26′½ from the truth; but he apprehends the greatest error will be thought of very little consequence in the practice of navigation.

XIV. Observations on the State of the Air, Winds, Weather, &c. made at Prince of Wales's Fort, on the North-West Coast of Hudson's Bay, in the Years 1768 *and* 1769. *By Joseph Dymond and William Wales.* p. 137.

These are tables of the daily state of the barometer, thermometers, winds and weather, during their stay at the fort. During most of the winter months, Nov., Dec., Jan., Feb., March, the thermometer without was considerably below the cypher, the lowest of all being — 45, that is, 45 below 0, which was on the 22d of January. And the highest state was + 80, viz. on the 3d of July.

XV. Of some very Perfect and Uncommon Specimens of Sponges from the Coast of Italy. By John Strange, Esq., F.R.S. p. 179.

Mr. S. having had frequent opportunities, during his stay in Italy, of visiting the sea coasts, he was encouraged, among other researches after the antiquities and natural history of that country, to collect some specimens of submarine productions. On examining the south-west coast of Italy in particular, he happened to meet with some very perfect and curious specimens of sponges, the descriptions of which he here gives. One of these has never been described before; and since only fragments of the other two specimens have been delineated in the works of the authors who mention them, he adds their respective descriptions and figures. These descriptions may perhaps appear imperfect, being confined merely to the figure and substance of the bodies, without any mention of the polypes that inhabited them. To account for this omission, it is necessary to observe that they were drawn up a few years ago, with others of the like kind, at the request of Dr. Targioni Tozzetti of Florence, who designed them for an appendix to a posthumous work of Micheli's, entitled De Plantis Marinis. As the plan of this work was botanical, Mr. S. thought it necessary to accommodate his descriptions accordingly, though he was not inclined to his opinion about the origin of these bodies. On inquiry it appears that the publication of Micheli's work is very uncertain; for which reason Mr. S. gave the following descriptions.

Fig. 2, pl. 1. Stupose, tuberous, tubular sponge, with cylindrico-conoid

tubes, growing together into a mass, all assurgent, and of which some of the larger appear pyxidated like a calyx, with a bifid and trifid base, and furnished with two or three columns; the smaller tubes are shaped like a mouse's tail, ending in a rounded point. The body or substance consists of a tough or but slightly compressible substance, of a deep dusky colour. It is found on the Etrurian coast, from Populonia, in a place called Porto Baratto, and is not very common. It admits of a great many varieties, some with the tubes completely conoid as it were, upright, oblique, here and there inverse, with an entire base, sometimes perforated or pervious with holes.

Fig. 3. Stupose, tuberculated sponge, with simple and branched tubercles, but with the ramifications imperfect, and commonly obtruncate at the roots: the form of the mass is subconoid, flattish at the top, the whole surface unequal, every where rough with branches and tubercles, hollowed and bifid at the base. The body consists of a tough substance, considerably anfractuous within, with some of the cavities wider or more excavated than the rest: its colour is dusky. It is found on the Etrurian coast, not far from the mouth of the river called la Cornia. This sponge, rising from a wide and hollowed base, terminates in a subconoid, flattened head, not ill resembling the shape of a mitre. It is called the Pope's Mitre by the Neapolitan fishermen, being common about the coast of Naples. *An Alcyonium durum, presbyterorum pileolum prorsus effingens. Cupani Hort. Cathol. Suppl.* 1.?

Fig. 4. A very small sponge, of an inversely conoideal shape, and twisted like a worm. It consists of a very dense and tough substance, with the fibres closely cohering together as in the Spongia hircina: its colour is dull brown. These sponges are found closely adhering in groupes to stones, shells, and other marine bodies. This is a very rare species of sponge, and had not, so far as he knew, been either figured or described. Count Marsigli long ago described something analogous to it under the name of Eperon de Coq.* It differs however from the present species both in colour, and in having a pyxidated head, as appears from the figure. He had observed some solitary specimens of the present species of sponge on the Etrurian coast, between Populonia and the mouth of the river Cecina. One very perfect specimen he observed in the Museum of Signior Philip Fabrini, near Pisa, and which was fished out of the Tuscan sea.

Fig. 4 and 5 show a congeries of this sponge growing on a calcarious stone, and also a detached single specimen.

It is well known, from the observations of Mercati, Boccone, Donati, and others, that the coasts of Italy in general afford a remarkable variety of zoophytes. Pallas likewise particularly mentions the many species of gorgoniæ

* Hist. Phys. de la Mer, part 4, p. 63, pl. 5, n. 22.

found on that coast, and justly laments the indolence of the Italians in not regarding them. Mathioli, Mercati, Ferrante Imperato, and the other early writers in natural history, made few, if any, new observations of this kind. They did little more than copy the ancients, or one another; and thought it sufficient to ascertain the identity of the species described by Aristotle, Dioscorides, and Pliny, and to illustrate them by figures, which were wanted in the works of these old masters. Thus for instance, Matthioli and Ferrante Imperato describe only the Alcyonia of Dioscorides. The more modern Italian naturalists have made as little progress in this subject, from the influence of the opinion established among them by Micheli and Marsigli; for if we except Donati, scarcely any of the rest have embraced the present received system about the origin of zoophytes, though the discoveries of the French academists, added to those of Ellis, Pallas, and other ingenious writers, seem to have put this matter beyond a doubt.

*XVI. On a Method of Preparing Birds for Preservation. By Capt. Thomas Davies.** p. 184.

Let a bird, beast, or any such like production of nature, be procured, that has been well preserved in its death, either naturally or by shot, as those that intend making any tolerable collection must do. He would not recommend shooting them (birds in particular) with shot smaller than common partridge shot, or N° 5, and that at a considerable distance, to prevent their being torn with too great a number. Having procured a bird as aforesaid, let it be opened from the upper part of the breast, to the vent, with a sharp knife or pair of scissars, the feathers of the breast and belly being first carefully laid aside by the fingers, so as not to hinder the skin being easily come at. The skin must then be carefully loosened from all the fleshy parts of the breast, body, thighs, and wings; then cut off all the flesh from those parts, and take out also the entrails and all the inside: then, having got a composition of burnt alum, camphor, and cinnamon, of each an equal quantity, well powdered and mixed together, strew some of this powder lightly over the whole carcase; but salt is by no means to be used with this composition, as it always will drop and nasty the plumage in moist weather; pour also into the body a small quantity of camphor dissolved in rectified spirits of wine; after that, fill up the cavity with fine cotton, or any soft woolly substance, pouring some of the aforesaid spirits into the cotton, or stuffing. Open next the mouth, and with a pair of scissars take away the tongue, the roof of the mouth, eyes, brains, and inside of the head; fill that also with the same composition; and having procured eyes as near the na-

* The present General Davies of the Royal Artillery, F.R.S., whose museum contains a large collection of the most curious specimens of Natural History.

tural ones as possible, put them into the sockets by means of a small pair of nippers introduced at the mouth. The eyes will be best made by dropping drops of black sealing wax on a card of the size of the natural ones; the card must be cut something larger than the wax to prevent their falling out of the head. Fill the head quite full with cotton, pouring some of the spirits down the throat, with some of the powder; a small piece of brass wire, that has been heated in the fire to make it pliable, may be put down the throat, being passed through one of the nostrils, and fastened to the breast bone, to place the head in any attitude you choose; next fill up the body where the flesh has been taken away, with cotton and the composition; and having a fine needle and silk, sew up the skin, beginning at the breast, observing, as you approach towards the vent, to stuff the skin as tight as it will bear. This will be easiest accomplished by means of a small piece of stick or ivory, like a skewer, till the whole is done: then lay the feathers of the breast and belly in their proper order, and the bird will be completed. If you would chuse to put it into an attitude, by introducing a small piece of the wire above mentioned through the sole of each foot up the leg, and into the pinion of each wing, it may be disposed of as you please. A composition of sublimate mercury, tempered with some water, and rubbed gently over the feathers, will prevent insects, and other vermin, from destroying the plumage.

XVII. On the Appearance of Lightning on a Conductor fixed from the Summit of the Mainmast of a Ship, down to the Water. By Capt. J. L. Winn. p. 188.

Capt. Winn says he was never without a conductor in his ship. He had them of various constructions: that which he last used was a chain of copper wire, down by the outside of the ship into the water. That such a chain, so disposed, may conduct the lightning, and prevent a stroke that might destroy a ship, has often been demonstrated; but a circumstance that occurred in his last voyage, may perhaps have greater weight with some seamen, than all the reasonings of the electricians. If it should be a means of persuading them to make use of conductors, his intention will be answered.

In April last, as they approached the coast of America, they met with strong south-westerly gales: they had continued several days, when exceedingly dark heavy clouds arose in the opposite quarter, forced against the wind till they had covered all the north-eastern half of the hemisphere: the struggle then between the two winds was very extraordinary; sometimes one prevailing, sometimes the other. Capt. W. was apprehensive they should have much lightning, and got his conductor in order; when, in hauling up the mainsail, the sheet block struck violently against the back-stays, to which the chain was fastened, and, as he found afterwards, broke the latter, which occasioned the phenomenon described

below. It was near midnight and very dark, when he first observed a pale bluish light a few feet above the quarter rail: at first he thought it proceeded from the light in the binnacle; but finding that it frequently disappeared and returned again precisely in the same place, and that it sometimes emitted sparks not unlike those of a small squib, he began to suspect that it proceeded from the conductor. To be certain, he ordered all the lights to be put out below, and that no rays of light might issue from the binnacle, he covered it entirely with his cloak. He was presently confirmed in his conjecture, that the light and sparks proceeded from the chain; for, placing himself near it, during the space of 2 hours and a half, he saw it frequently emit continued streams of rays or sparks; sometimes single drops as it were slowly succeeding to each other, and sometimes only a pale feeble light. On examining next morning, he found the chain broken a little above the ship's gunwale, half the eye of each link being quite gone, and the points of the remaining halves about three fourths of an inch asunder; luckily the chain was fastened to a smaller rope above and below the eye of each link, which prevented that part of the chain below from falling into the water, or of being separated from the part above, beyond the striking or attracting distance.

XVIII. An Investigation of the Lateral Exposition, and of the Electricity communicated to the Electrical Circuit, in a Discharge. By Joseph Priestley, LL.D., F.R.S. p. 192.

Several years before Dr. P. made any experiments in electricity, except with a view to amuse himself and his friends; he had observed, that in discharging jars, and particularly such as were filled with water, without any coating on the outside, he felt a slight shock; though it was plain that the hand in which he held the discharging rod, made no part of the circuit. Mr. Wilson also in his first experiments on the Leyden phial, observed, that bodies placed without the electric circuit would be affected with the shock, if they were only in contact with any part of it, or very near it. Analogous to this was his observation, that if the circuit was not made of metals, or other very good conductors; the person who laid hold of them, in order to perform the experiment, felt a considerable shock in that arm which was in contact with the circuit. See History of Electricity, p. 95. Lastly, in the course of his experiments with large electrical batteries, he found the force of this lateral explosion, as he calls it, to be very considerable: for he several times observed, that a chain which communicated with the outside of the battery, but which made no part of the circuit, made a black stain on a piece of white paper, on which it accidentally lay, almost as deep as the chain that formed the circuit. (History, p. 644.) And when, in order to judge, by his feeling, of the lateral force of electrical explosions; he made it pass over a part of his naked arm, the hairs of the skin were all singed, and the papillæ py-

ramidales raised, not only along the path of the explosion, but also wherever any part of the chain had touched it, though not in the circuit. Ib. p. 686.

It was to ascertain the nature and effects of this lateral explosion, that the following experiments were made. Not having the least doubt, but that if any electric spark passed between a body that was insulated, and another, the insulated body would appear, either to have received or to have lost electricity; he imagined that nothing more was to be done than to insulate bodies placed within the influence of the electric circuit, with pith balls hanging from them; and on their diverging with the electric spark, immediately to observe, of what kind the electricity they had contracted was; and previous to the experiment, he conjectured it would be negative; supposing that the discharge from the inside coating, in an interrupted circuit, was not able to supply the outside fast enough. And since, the larger the insulated body was, the greater quantity of the electric fluid it was capable of receiving, or parting with, and consequently the more sensible the effort would be; he began with suspending on silken strings, a pasteboard tube, covered with tinfoil, 7 feet long and 4 inches thick, with large knots at each end; and a brass ball (at the end of an iron rod, which communicated with the outside of the jar) was placed within about a quarter of an inch of it; while the discharge was made through an insulated interrupted circuit, no part of which was less than 2 feet from the insulated tube. On making the explosion, the spark appeared as he expected; but to his great surprize, he could not find that either positive or negative electricity was communicated to the insulated tube. Neither the pith-balls, nor the finest threads diverged, or moved in the least, at or after the discharge; though, every thing else remaining in the same state, the least sensible electricity communicated to this tube (a quantity so small as hardly to be visible, in the form of a spark, at the time of communication) made the balls and the threads separate to a great distance, and would have kept them in a state of divergency more than an hour. Lest a small degree of motion or divergency should escape notice, while he was intent on making the discharge, he had an assistant along with him, whose eye was on the threads all the time the Dr. was making the experiment.

This experiment, as will easily be imagined, shook his whole hypothesis, and confounded all his ideas. He could not question the fact, having repeated the experiment, with precisely the same event, above 50 times, on account of having been hardly able to believe his own senses, or those of others. There was an evident electric spark, sometimes near half an inch in length, between the bodies composing the electric circuit and the insulated tube, in such a state of the air, as he knew, by frequent trials, would have kept it electrified a long time, and yet there was no communication of electricity. He did not remember that he was ever more puzzled with any appearance in nature than he was with this; and

in the night following these experiments, endless were the schemes that occurred to him of accounting for them, and the methods with which he proposed to diversify them the next morning, in order to find out the cause of this strange phenomenon. Accordingly, he was no sooner at liberty to attend to this experiment; but repeating it with some difference in the disposition of the apparatus, he observed that on every discharge a slight motion was given to the threads that hung from the insulated tube. On this the impossibility of an electric spark, neither giving nor taking any thing from an insulated body, contrary to his most attentive observation, and that of his assistants, he concluded that some motion must have been given to the threads the day before; especially when he found that in these later experiments the communicated electricity was always positive, the same with that of the inside of the jar; but the quantity of it was so small, that the most exquisite contrivance was necessary to ascertain the nature of it; for though on this occasion the lateral spark was near a quarter of an inch in length, the threads on the insulated tube could only be made, by the explosion, to change their position, from leaning a little one way, to leaning as much the other, in the neighbourhood of an insulated brass rod, loaded with a small quantity of positive or negative electricity.

Dr. P. could not help however being surprized, that so large a spark should give no more electricity to the insulated tube than it appeared to have done; when, in other circumstances, a spark ten times less than this would have made a great and permanent alteration: yet improbable as these circumstances were, he entertained no doubt at that time, but that these insulated bodies were electrified, either positively or negatively, according as the inside of the jar was positive or negative, by this lateral explosion, though the degree was exceedingly small; and he continued in this persuasion the longer, as it happened to be a considerable time before he got another spark that communicated no sensible electricity. Dr. P. cannot help taking notice, that if it had happened, that in his first experiment the insulated tube had always acquired or lost the least sensible electricity (and as he afterwards found, there were many chances against the first result), he should have formed, and have acquiesced in, some sort of hypothesis, to account for the giving or receiving of electricity in those circumstances, and there the business would have ended; but the seeming contrariety of these appearances obliged him to pursue them further.

Not being able completely to satisfy himself with his last conclusion, attended with the difficulties above mentioned, he kept diversifying the experiments, and introducing every circumstance that he could imagine might possibly affect the result of them; and among the rest, he made the following experiments, which quite unhinged him again, and left him as much at a loss as ever he had been before. Having suspended a fine thread on an insulated brass rod, placed about

$\frac{1}{8}$ of an inch from another rod, which was likewise insulated, and one end of which was in contact with the coating of the jar; and having electrified the rod that supported the pith balls, and placed a rod loaded with the same electricity near them: he observed that on every discharge, the balls, which before were repelled, were instantly attracted by the electrical rod; and that the result was invariably the same, whether they and the rod were loaded with positive or negative electricity: and also whether the jar was charged positively or negatively. He repeated the experiment for several hours, without the least variation in the event: which clearly proved, that in these circumstances the electricity of the rod that received the lateral explosion was discharged by it.

Afterwards, Dr. P. repeated this experiment with some little variety, and found the electricity of the rod only lessened by the lateral explosion. These experiments however by no means favoured the supposition of the uniform communication of electricity, either that of the inside or that of the outside of the jar: and together with the former experiments, convinced him that this lateral spark by no means produced the effect that might have been expected in communicating electricity. But with the next set of experiments, the difficulty began a little to clear up, and continued to do so gradually, till he gained all the satisfaction he could wish for, with respect to this puzzling phenomenon. The first time that he was able to vary the electricity of the insulated body placed near the electric circuit, or of the bodies that formed the circuit (which he now began to attend to), by any different adjustment of the apparatus, was on the following occasion.

Near to an iron rod, that touched the bottom of a jar charged positively, he placed another insulated rod, with a pair of pith balls hanging to it; and observed, that when he attempted to make the discharge through an imperfectly conducting circuit, (bringing for instance, part of the table into it), a strong spark passed between the insulated rod and the other that touched the jar, and immediately the balls separated as far as they possibly could; and, continuing in a repulsive state, appeared to be electrified negatively. But immediately completing the circuit with good conductors, and making the remainder of the explosion in a full spark; another spark passed between the two rods, and immediately the balls fell close together again; and sometimes would separate with the opposite, or positive, electricity.

Dr. P. could not, on this occasion, make the lateral spark, in the full explosion, so great as in the imperfect discharge. He also observed, that the more interrupted the circuit was, the farther would the lateral explosion reach; and that the electricity, which the full explosion communicated, was always positive when the jar was charged positively, and negative when it was charged negatively. The result of the imperfect discharge was always the reverse. Insulating

several bodies, and the jar too, charged positively, they all equally contracted positive electricity by the discharge.

In this state of the experiments, he had no idea of the possibility of the lateral spark not communicating electricity to the insulated body ; but he imagined that the kind of electricity communicated depended on some circumstance in the disposition of the apparatus, that he was not sufficiently aware of. At length recollecting, that this last experiment resembled, in some respects, that curious one of Professor Richman, mentioned in the History of Electricity, p. 272; in which it appeared, that when the coating of either side of a plate of glass communicated with the ground, the opposite electricity of the other side was more vigorous; he was satisfied that the negative electricity of the bodies that formed the circuit in the imperfect discharge, was produced by the greater difficulty with which the outside of the jar was supplied, than the inside was discharged; so that the outside was comparatively in a state of insulation, and therefore would communicate negative electricity to all bodies within its reach. And from this he was led to conclude, that, provided the jar was insulated, and the inside was made to part with its electricity with more difficulty than the outside received it, the bodies that formed the circuit would contract positive electricity ; and the result answered exactly his expectations. He also concluded, that, making the interruption in the middle of the circuit, since, in this case, the inside would give, and the outside receive, with equal difficulty, the bodies in the circuit, placed between the place of interruption and the inside of the jar, would be charged positively ; and those placed between the place of interruption and the outside, would be charged negatively ; and this also was verified by experiment.

In this state of things, Dr. P. found that he could give the insulated circuit what kind of electricity he pleased, provided there was any kind of interruption in some part of the circuit ; and conjecturing that the electricity of bodies placed near the circuit would be the same with that of the bodies that composed it, he sometimes placed the rod that supported the pith balls near the circuit, and sometimes introduced it into the circuit; and found, that, in both cases, it contracted the same electricity. This tended to confirm him in the supposition, that the lateral explosion was always attended with a giving or receiving of electricity, according to the nature of the circuit, and the place where it was situated; and he again overlooked the disproportion between the cause and the effect.

Presently after this, it occurred to him, that what may be called the redundant electricity of the outside or inside of the jar, separates from that which is in the glass, and constitutes the charge, must have some concern in this event; and the supposition was verified by fact. For insulating a jar, charged positively, he observed, that when he touched the outside coating last (as is com-

monly done in setting it down) and made the discharge through good conductors, they were all electrified positively; and bodies placed near the circuit were the same. On the contrary, if, after placing the jar on the stand, he touched the knob of the wire, communicating with the inside, so as to take off all its redundant electricity; both the circuit and the neighbouring bodies contracted negative electricity.

Dr. P. had at this time quite forgot that Æpinus had made the same observation, on discharging a plate of air, mentioned in the History of Electricity, p. 273; but considering what he says on that subject, he found he was mistaken with respect to the reason of this experiment not succeeding with Dr. Franklin and others, who had always asserted, that the electric circuit contracts no electricity at all by a discharge. For he says, that the surfaces with which the doctor tried the experiment, were not large enough to make the effect sensible; and that the distance of the metal plates was likewise too small, as, he says, it necessarily must be in the charging of glass: whereas he could give the insulated circuit as sensible electricity with a common jar, as he could with his plate of air; and much more depends on the height of the charge, which must have been inconsiderable in the plate of air, than the quantity of surface; which, however, may be increased at pleasure, by multiplying jars in batteries.

He found, however, afterwards, that much depended on the quantity of surface in the coating, and the bodies connected with them, as containing more of that redundant electricity, the effect of which was seen in the last mentioned experiment. For when he discharged the jar, standing in contact with the prime conductor, the tendency to the communication of positive electricity was so great, that, in that situation, the insulated circuit contracted strong positive electricity, when, every thing else remaining the same, except removing it from the conductor, and then making the discharge, it contracted no electricity at all.

Being now perfectly master of the electricity of the circuit in electrical explosions, and being able, in two different methods, to give which of the two electricities he pleased; he imagined that, if he could so balance them, as to communicate neither, there would be no lateral spark, as in the abovementioned experiments; but in this he was absolutely mistaken. For, in the first place, when, after setting the charged jar upon the stand, he took off, as near as he could guess, one half of the redundant electricity of the inside, and left both sides equally electrified (as appeared by the equal attraction of the pith balls to them both), the discharge of the jar through a circuit of good conductors did not, indeed, communicate the least sensible electricity to the circuit, but the lateral explosion was almost as manifest as before. The pith balls, hung upon the rod that received it, never separated. In the next place, he repeated this experiment by balancing the two different methods of communicating electricity to the cir-

cuit, one against the other. For, not insulating the jar, but setting it on the table, which gave the circuit and the bodies contiguous to it an advantage for contracting positive electricity by the discharge; but, at the same time, making an interruption in the circuit (by introducing part of the table into it, which tended to give them negative electricity); he could easily manage it so, that the circuit contracted neither the one nor the other; and yet, as in the former case, the lateral explosion was as considerable as ever. The balls never separated.

To vary the experiment, he placed an insulated brass ball, 2 inches in diameter, round and smooth, so as not easily to part with any electricity it had got, in the place of the rod that supported the pith balls; and having found a situation in which no electricity was communicated to the circuit, he observed that none was communicated to it, though, to all appearance, it received a spark of about $\frac{1}{4}$ of an inch in length. At least, if it had contracted any, it was so little, as to make it very problematical; whether a pith ball, or a fine thread, was moved by it, or not: whereas, when he gave it the smallest sensible spark in any other manner, it would attract those light bodies for a long time together. The interruption of the circuit made use of in this experiment, was not by means of any part of the table, but only about a yard of brass chain introduced into it, and disposed between the inside of the jar and that part of the circuit, near which the insulated ball was placed. N. B. The ball must not be placed near the jar itself; for, in that situation, he found, that, though it was very smooth, and perfectly spherical, yet it could not be placed very near the outside of the jar standing on the table, without contracting negative electricity, in a very small space of time.

These experiments threw him back into his former state of perplexity, with respect to the lateral spark; since, when the two electricities of the circuit were exactly balanced, it was very little diminished, and yet the body that received it was not in the least sensibly electrified. But, on reflection, he concluded, that this lateral spark must be of the nature of an explosion, and consequently, that an electric spark must enter, and pass out again, within so short a space of time, as not to be distinguished, and leave no sensible effect whatever: for though, in this case, part of the electric matter natural to the body must be repelled, to make room for the foreign electricity, its restoration to its natural state was so quick, that no other motion could correspond to it. This hypothesis is favoured by the observation, that it is the very same thing, whether a body be introduced into a circuit, or placed near it, with respect to contracting electricity; that is, whether the electric charge enter the body at one place, and go out at another, or whether it be received and emitted at the same place. This lateral explosion is an effect similar to a partial circuit, in which, part of the electric matter that forms the charge in an explosion, goes one way, while the rest of the

charge goes another; the only difference is, that this detached part of the charge leaves the common track, and returns to it again, in the very same place.

Several remarkable partial circuits occurred in the course of his experiments before, particularly one, mentioned in the History of Electricity, p. 692, in which, part only of the explosion passed in the shortest way, while another part of it took a circuit, consisting of the same materials, 30 times as long; and another, mentioned, p. 691, where one circuit was made through a thick rod of metal, and another, at the same time, through the open air.

That there is an admission and an explosion of the electric matter, in this lateral explosion, seems evident, from this circumstance, that it is far more considerable when the body that receives it is large, than when it is small. In the former case, there is room for the electric matter, natural to the body, to retire, on the admission of the foreign electricity belonging to the charge; whereas, in the latter case, there is not room for it. When he placed a small brass ball, of about a quarter of an inch in diameter, near the circuit, he could not perceive that it was at all affected by any lateral explosion; and the spark was very inconsiderable, when he placed a needle, about 2 inches in length, to receive it; but when he connected the large tube above mentioned, by means of a pretty thick iron wire, to any body whatever, placed in the neighbourhood of the circuit, he had (with a jar of only half a square foot of coating glass) made the lateral explosion, an inch or more in length, consisting of a very full and bright spark of electric fire. Insulated bodies, of about 8 or 9 feet in length, seem to admit as large a lateral explosion as any body whatever is capable of: for, connecting them with the ground, by means of the best conductors (which gave the electric matter in the bodies, the freest recess possible) he could never make this explosion much more considerable, using the same jar, and all other circumstances the same.

It is a manifest advantage in these experiments, that the lateral explosion be not taken from the coating of the jar itself, or from any part of the circuit, very near to it. He found that, cæteris paribus, it is the most considerable when taken at the extremity of a brass rod, of one foot, or a foot and a half long, the other end of which is contiguous to the jar. It is analogous to this, that the longest spark is taken, not from the body of the prime conductor itself, but at the extremity of a long rod inserted into it. The electric matter seems to acquire a kind of impetus by the length of the medium through which it passes. But he found that the maximum, in this case, did not exceed, or rather, that it did not quite reach, 3 feet; for, making use of a thick iron rod, 8 or 9 feet long, the lateral explosion, taken at the extremity of it, was about the same, as when it was taken at the end of a rod 4 inches from the jar; and not half so considerable as when taken at the extremity of a rod one foot long. This, he

imagined, might be owing to the obstruction which the electric fluid meets with in passing even through metals; which appears, by his former experiments, to be much more considerable than was generally imagined.

On the whole, this remarkable experiment seems to be made to the most advantage in the following circumstances. Let the jar stand upon the table; let a thick brass rod, insulated, stand contiguous to the coating; and, near the extremity of this rod, place the body that is to receive the explosion. This body must be 6 or 7 feet in length, and, perhaps, some inches in thickness, or be connected with a body of those dimensions. Lastly, let the explosion be made with the discharging rod resting on the table, close to a chain, the extremity of which reaches within about an inch and a half of the coating of the jar. In this case, the operator will hardly fail of getting a lateral explosion of an inch in length; which shall enter and leave the insulated body, without making any sensible alteration in the electricity natural to it.

With large jars, containing 3 or 4 square feet of coated glass, bearing a very high charge, Dr. P. makes no doubt but that this experiment might be made to much more advantage; but, at the time that he was engaged in this investigation, he happened not to have any such jar, and therefore only used one that contained half a square foot of coated glass. If the interruption in the circuit, which is almost necessary in these experiments, be made by introducing a length of chain into it, rather than by making part of the explosion pass along the tube, there is a medium in the length of chain, that answers better than either a longer or a shorter circuit. In a long interrupted circuit, the electric matter seems to lose the impetus which it discovers in a short one. In all these cases, the electric charge seems to remain for a moment in the parts of the interrupted circuit; and therefore instantaneously rushes, in all directions as well towards bodies that are not placed along its passage to the jar, as those that are; but, when the same charge occupies a larger circuit, it has more room to expand itself, and is not so strongly impelled to desert it. He found, however, by repeated trials, that when he made use of 3 yards of brass chain in the circuit, there was a distance to which the lateral explosion would not reach. The same distance it also would not reach, when the circuit consisted of only one brass rod; but it reached it with great ease, when only half a yard of chain was used, even without any other interruption in the circuit. But it reached to a much greater distance, when the chain was very short, and the interruption was greater in other respects.

Dr. P. had imagined, that, since the body which had received the lateral explosion, contained, for a moment, more than its natural quantity; that if it were acutely pointed, some would escape, and that, on the return of the explosion, the body would be exhausted; but he found no such effect, though he

affixed fine needles to the bodies he made use of. The lightest pith balls placed near the extremities of these needles, were not in the least affected by the explosion. When he placed a number of brass balls, one behind another, the lateral explosion passed through them all, being visible in the intervals between each of them, and returned the same way, leaving them all in the same state in which it found them; and a great number of lateral explosions might be taken at the same time, in different parts of the circuit, some of them very near one another. It made no difference, whether the lateral explosion was received on a flat smooth surface, or the points of fine needles. In both cases the spark was equally long and vivid.

Dr. P. had no sooner completed these experiments on the lateral explosion, but he had a curiosity to see what kind of an appearance it would make in vacuo; since no other phenomenon in electricity resembles it. In all other cases, the electric matter rushes in one single direction; whereas, in this, it goes and returns in the same path, and, as far as can be distinguished, at the same instant of time; so that all the difference of the two electricities, which are so conspicuous in vacuo, must here be confounded. Accordingly he found, though the pump was not in good order, that he could perceive this explosion in vacuo, at the end of rods, placed several inches asunder; and when they were brought within about 2 inches, they seemed to be joined by a thin blue or purple light, quite uniform in its appearance. As these rods were made to approach, this light grew denser; but still exhibited no such variety, as is observed between the bodies that give and receive electricity, in the common experiments in vacuo.

Dr. P. was pretty soon convinced, that uncoated jars could not be used to any more advantage in these experiments, than those that were coated; since the want of coating only operated as an interruption in the circuit, occasioning a difficulty in the admission of the charge on the outside of the jar. And, in all cases, the greater this difficulty of passage was made, provided the discharge was made at once, the more considerable was the lateral explosion, and the greater shock was given to the hand that held the discharging rod; which shock was nothing more than one of these lateral explosions, issuing from the rod as part of the circuit. He concludes the account of these experiments with observing, that they may possibly beof some use in measuring the conducting power of different substances; since, the greater is the interruption in the electric circuit, occasioned by the badness of its conducting power, the more considerable, cæteris paribus, is the lateral explosion.

XIX. Experiments and Observations on Charcoal. By Joseph Priestley, LL.D., F. R. S. p. 211.

May be consulted in the author's collected works on different kinds of air.

XX. and XXI. Meteorological Observations for 1769, *made at Bridgewater, Somersetshire; and at Mount's Bay, Cornwall, by Wm. Borlase, D D, F. R. S. Communicated by Dr. Jeremiah Milles, Dean of Exeter, and F. R. S.* p. 228.

These observations, are of the barometer, thermometer, winds, and weather, and quantity of rain, which last for the whole year, is 23.66 inches, at the former place, and 42.73 at the latter.

XXII. On the Manna Tree, and on the Tarantula. By Dominico Cirillo, M. D. Prof. Nat. Hist. at Naples. p. 233.

The manna tree, commonly called ornus by the botanists, is a kind of ash tree, and is to be found under the name of fraximus ornus, in Linnæus' Sp. Plant. This kind of fraxinus is very easily distinguished from the common fraxinus sive fraxinus excelsior, by the leaves, which are round at the top, subrotunda, integerrima. This tree very seldom grows to a considerable height, nor does it acquire a considerable bulk; in general it is from 10 to 20 feet high, the trunk is commonly of 5 or 6 inches in diameter, and the branches are pretty numerous, and irregularly spread: these dimensions however vary, if these trees are not crowded together, and have more liberty of growth. The manna tree is common, not only in Calabria and Sicily, but also on the famous mountain Garganus, situated near the old town of Sypontum on the Adriatic; and is mentioned even by Horace as an inhabitant of that mountain. In all the woods near Naples the manna tree is to be found very often; but, for want of cultivation, it never produces any manna, and is rather a shrub than a tree. The manner, in which the manna is obtained from the ornus, though very simple, has been yet much misunderstood by all those who travelled in the kingdom of Naples; and among other things they seem to agree, that the best and purest manna is obtained from the leaves of the tree; but this seems to be an opinion taken from the doctrine of the ancients, and received as an incontestible observation, without consulting nature. The manna is generally of two kinds; not on account of their intrinsic quality being different, but only because they are gotten in a different manner. In order to have the manna, those who have the management of the woods of the orni, in the month of July and August, when the weather is very dry and warm, make an oblong incision, and take off from the bark of the tree about 3 inches in length, and 2 in breadth; they leave the wound open, and by degrees the manna runs out, and is almost suddenly thickened to its proper consistence, and is found adhering to the bark of the tree. This manna, which is collected in baskets, and goes under the name of manna grassa, is put in a dry place, because moist and wet places will soon dissolve it again. This first kind is often in large irregular pieces of a brownish colour, and frequently is full of dust and other impurities. But when the people want to have a very fine manna, they apply to the incision of the bark, thin straw, or small bits of shrubs, so that the manna, in coming

out, runs upon those bodies, and is collected in a sort of regular tubes, which give it the name of manna in cannoli, that is, manna in tubes: this second kind is more esteemed, and always preferred to the other, because it is free and clear. There is indeed a third kind of manna, which is not commonly to be met with; it is very white, like sugar; but it is rather for curiosity than use. The two sorts of manna above-mentioned undergo no kind of preparation whatever, before they are exported; sometimes they are finer, particularly the manna grassa, and sometimes very dirty and full of impurities; but the Neapolitans have no interest in adulterating the manna, because they always have a great deal more than what they generally export; and if manna is kept in the magazines, it receives often very great hurt by the southern winds, so common in this part of the world. The changes of the weather produce a sudden alteration in the time that the manna is to be gathered; and therefore when the summer is rainy, the manna is always very scarce and very bad.

After this short account of the manna, I shall, says Dr. C. give you a little of the history of the Tarantula, because I have had an opportunity of examining the effects of this animal, in the province of Taranto, where it is found in great abundance: but I am afraid I shall have nothing more to say, than that the surprizing cure of the bite of the Tarantula, by music, has not the least truth in it; and that it is only an invention of the people, who want to get a little money, by dancing when they say the tarantism begins. Probably sometimes the heat of the climate contributes very much to warm their imagination, and to throw them into a delirium, which may be in some measure cured by music: but several experiments have been tried with the Tarantula; and neither men nor animals, after the bite, have had any other complaint, but a very trifling inflammation on the part, like those produced by the bite of a scorpion, which go off by themselves without any danger at all. In Sicily, where the summer is still warmer than in any part of the kingdom of Naples, the Tarantula is never dangerous, and music is never employed for the cure of the pretended tarantism. Every year this surprizing disorder loses ground, and doubtless in a very little while it will entirely lose its credit.

XXIII. Observations made at Dinapoor, on the Planet Venus, when passing over the Sun's Disk, June 4, 1769, *with three different Quadrants, and a Two-Foot reflecting Telescope. By Luis Degloss, Captain of Engineers, with the Assistance of J. Lang and A. Stoker.* p. 239.

At Sun-rise cloudy

At 5^h 20^m 32^s A. M. ...... The sun disengaged from the clouds, when Venus appeared on the ☉'s disk.

At 7 5 22. .... The beginning of the emersion.

At 7 23 36. .... The end of the emersion.

The latitude of the place where the observation was made, is 25° 27′ N.

The time is exactly corrected, and all the allowances made.

XXIV. Directions for making a Machine for finding the Roots of Equations Universally, with the Manner of Using it. By the Rev. Mr. Rowning. p. 240.

Perusing a discourse in the memoirs of the Royal Academy at Petersburg, tome 7, page 211, by the learned John Andrew de Segner, containing a universal method of discovering the roots of equations, Mr. R. found, that the author's method consisted in finding several ordinates of a parabolic curve, such, that its abscissas being taken equal to any assumed values of the unknown quantity in the equation, the ordinates corresponding to those abscissas, should be equal to the values of all the terms in the equation, when brought to one side; that is, in other words, in finding several ordinates of a parabolic curve defined by the equation proposed: in which case, as is well known, if a curve be drawn through the extremities of the said ordinates, the points on the axis, where the curve shall cut it, will necessarily give the several values of the real roots of the equation; and the several points, where the curve shall approach the base, but shall return without reaching it, will show the impossible ones.

This is a method Mr. R. himself fell upon 10 or 12 years before, and had constantly used for finding the roots of such equations as he had had occasion to consider. But Mr. S.'s method is preferable in one respect, viz. that whereas Mr. R. always computed the value of the ordinates in numbers, Mr. S. finds them by drawing certain right lines; however, when there are both possible and impossible roots in an equation, as generally there are, these methods are both of them extremely embarrassing: the learned author therefore wishes, that some method might be thought of, whereby such curves, as now spoken of, might in all cases be described by local motion; but this, he tells us, he looked upon as so very difficult a task, that he never attempted it. This hint, however, convinced Mr. R. that the thing was possible; he therefore determined to endeavour to discover it.

He soon found, that if rulers were properly centred, and so combined together, that they should always continue representatives of the several right lines, by which he discovers the above-mentioned ordinates, on moving the first, a point or pencil, so fixed as to be carried along perpetually by the intersection of the first and last rulers, would describe the required curve, let the number of dimensions in the equation be what it will; only the greater that number, the greater must be the number of the rulers made use of. And this appeared to Mr. R. so obvious, that he wondered that neither the learned author, who seems to have the thing much at heart, nor any body else since its publication saw it.

But as this is a matter of curiosity rather than any use, and as the method was afterwards published separately, in 1771, it is unnecessary to enter any further into it at this time.

XXIII. On the late Transit of Venus. By Nathanael Pigott, Esq. Dated Caen, Lower Normandy, Feb. 9, 1770. p. 257.*

To his own observations Mr. P. has added those of other observers, which were sent to him from different places, and reduced them to the observatory of Paris, keeping an account only of the difference of meridians, as inserted in the Connoissance de tems, and omitting the small correction of the parallax, suitable to the different situations of these places, because the longitude of some of them is not known with sufficient precision, to admit here of this very small equation.

Table of Observations reduced to the Meridian of the R. Observatory, at Paris.

Places of observations.	Observers.	External contact.	Internal contact.	Focus of instruments.	Mag. powers.	Exter. contact reduc. to Paris.	Inter. contact reduc. to Paris.
Greenwich	Maskelyne	$7^h\ 10^m 55^s$	$7^h\ 29^m 23^s$	telesc. 2 ft.	140	$7^h\ 20^m 11^s$	$7^h\ 38^m 39^s$
	Hitchens	7 10 54	7 28 57	telesc. 6 ft.	90	7 20 10	7 38 13
	Hirst	7 11 11	7 29 18	telesc. 2 ft.	55	7 20 27	7 38 34
	Horseley	7 10 44	7 29 28	achro. 10 ft.	50	7 20 0	7 38 44
	Dun	7 10 37	7 29 48	achro. 3½ft.	140	7 19 53	7 39 4
	Dollond	7 11 19	7 29 20	achro. 3½ft.	150	7 20 35	7 38 36
	Nairne	7 11 30	7 29 20	telesc. 2 ft.	120	7 20 46	7 38 36
Kew	Bevis	7 10 2	7 28 17	telesc. 3½ft.	120	7 20 27	7 38 42
Caen		7 9 20	7 27 43	telesc. 12 in.		7 20 7	7 38 30
Obs. of Paris							7 38 11.5
Ditto	D. of Chaulnes						7 38 58
Paris	Messier						7 38 45
	Baudouin						7 38 51
	Turgot						7 38 50
	Zanoni						7 38 41
St. Hubert	Le Monier		7 34 56	achro. 10½ft.			7 36 52.5
Ditto	Chabert		7 35 32	refrac. 18 ft.			7 37 28.5
Mission	Rochefort		7 27 7.5	achro. 3 ft.			7 37 54.5
	My Son		7 26 55.5	telesc. 18 in.	55		7 37 42.5
	Self	7 9 38.5	7 26 24.5	achro. 6 ft.	80	7 20 25.5	7 37 11.5

The place at Kew, where Dr. Bevis observed, being $1^m\ 9^s$ to the west of the observatory at Greenwich, is of course $10^m\ 25^s$ west of that at Paris. The observations in the table, joined by a brace, were made together in the same place. In comparing the observations of the internal contact, it is seen how little the last five agree with the others. Mr. P. was wholly ignorant in what light the able astronomers, who observed at St. Hubert, consider their observations. M. le Monnier, in communicating them, adds no remarks on them. As to those made at the house called La Mission, situated in the neighbourhood of Allemagne, a village near this town; this house is about 500 toises south-east of Mr. P.'s observatory at Caen, and their difference of meridians about 200 toises.

At $7^h\ 4^m\ 58^s.5$ of the clock, or $7^h\ 9^m\ 38^s.5$ apparent time, Mr. P. perceived the external contacts of the sun's and Venus's limbs. As the impression on the sun's limb seemed considerable, he concluded this observation too late, which he

judged to be occasioned by a motion of undulation, with which the sun was strongly affected; for this reason, he does not hesitate to declare this observation insufficient; yet it agrees very well with Dr. Bevis's made at Kew, and is nearly a mean between those of Greenwich, as may be seen by the table. However, Mr. P. prepared himself with all possible care, for the observation of the internal contacts; and though the sun's limb moved continually up and down with a quick motion, he judged the internal contacts at $7^h\ 21^m\ 44^s.5$ by the clock, or $7^h\ 26^m\ 24^s.5$ apparent time, and 3^s or 4^s later, he saw a thread of light separate the planet from the sun. Mr. P. perceived that Venus, before she separated from the sun, was considerably stretched out towards his limb, which gave the planet nearly the form of a pear; and even after the separation of the limbs, Venus was 12 or more seconds before she resumed her rotundity.

	Clock.	Apparent time.	
At	$7^h\ 30^m\ 27^s.0$	$7^h\ 35^m\ 7^s.0$	Venus quite round.
	7 38 25 .0	7 43 5 .0	Venus's limb indented.
	7 45 8 .0	7 49 48 .0	Venus of a very irregular form, and strongly affected by an odd twisting motion.

XXIV. Observations on the proper Method of calculating the Values of Reversions depending on Survivorships. By Richard Price, D. D., F. R. S.* p. 268.

See Dr. Price's treatise on reversionary payments.

XXV. Of Electrical Atmospheres. By J. B. Beccaria, F. R. S. p. 277.

For these theorems, see the author's treatise on electricity, published in English by Mr. Nourse in the Strand, in 1776.

XXVI. On the Preservation of dead Birds. By Mr. T. S. Kuckahn. p. 302.

Mr. K. thinks he has tried most, if not all, the methods that have been published or practised for many years past, with all the care and attention he could; and it was not till after the loss of much time and many fine subjects, birds in particular, that he set himself to find out such methods, drugs, and liquors, as would effectually penetrate and perfectly cure all the parts, so as to keep them plump and full. With regard to the present ways and materials, he first remarks on that in which raw alum, common salt, and black pepper, are applied, that he never could find those materials sufficient for a perfect preservation. They never fail to become humid in moist air and long continued wet weather; they suffer the flesh to rot, and even corrode the wires made use of to confine the birds in their natural attitudes, till the whole drops to pieces on the least touch or motion. Salt naturally degenerates to a pickle; if the bird has been killed by shot, it will ooze through the shot holes. If it has been killed by hand, an

incision must be made, in order to extract the entrails and put in the materials that are to effect the preservation. Now it is impossible to close that incision so tight, as to confine the pickle from creeping out, and whenever it does get out, it will infallibly spoil the plumage; or if, to prevent that, we hang up the birds by the feet, then the pickle will descend to the neck and head, before the upper parts in that situation are sufficiently cured; the certain consequence of which, (in summer here, and at all seasons in hot climates) will be, that maggots will be generated in such uncured parts, and of course the birds destroyed. Supposing however for a moment (what will scarcely be found to happen once in a thousand trials) that the pickle should penetrate and cure every part, we have then, what?—a bird preserved in its natural shape, dimensions, attitude, and colours. No, but we have a poor shrivelled up dried carcase of a bird, in which neither the natural shape, dimensions, nor colours, are preserved, and which continually excites the disagreeable idea of its having been starved to death on purpose. It is true the eyes look lively and in full preservation; and no wonder, for they are glass; they serve, however, by the contrast, to show more strikingly the miserable condition of the rest of the body. One would have imagined that so palpable an absurdity, as the placing a fine full glistening eye in the head of a body, not only manifestly dead, but appearing to have perished by sickness or famine, would have been obvious to every body; to have kindly suffered the languid eyelids to close, would have at least avoided so ridiculous a contradiction. Lastly, experience shows that birds thus treated are seldom or never so cured, but that the flesh grows rank; that rankness invites the insects, and of course the bird is soon destroyed.

A second method of preserving birds is, by immerging them in spirits; and if the barely keeping the carcase of birds from putrefaction be all that is required, this method is effectual. Another method is that of skinning birds; they had no other way in Germany and Holland, and it was generally practised in France till very lately, when the method of preserving by alum, salt, and pepper, was published and recommended. Skinning, compared with the other methods, is no bad way, but yet it is subject to many objections; 1st, there is a great difficulty in skinning, especially small delicate birds, killed perhaps by large shot; 2d, most people will find it hardly possible to reduce the skins to their natural proportions and attitudes, particularly the necks, which are often twice as long when separated from the vertebræ, as before; 3dly, the flesh and bones of the wings and rump must, after all, be left with the skin, and are as difficult to preserve as any other parts of the body. However, those who chuse to continue this method, will find their interest in making use of the materials recommended below.

Those who shoot birds for the purpose of preserving, should always be pro-

vided with a quantity of cotton or fine tow, with which to stop the shot holes, and also the throat of the bird, to prevent the blood from fouling the feathers, which infallibly spoils them. If the birds are not quite killed by the shot, they should be immediately dispatched by pressing the thumb nail hard on the wind-pipe; and care should always be taken to confine the wings as soon as possible, to prevent their fluttering. The birds when dead are to be carried by the legs, and not be crammed into nets or held by the neck, in which last position the weight of the body would stretch it beyond the natural proportion. When they are brought home, they should be hung up by the legs, and the stop of cotton taken carefully out of the throat, and a small stick put across between the bill, to keep it open, that the blood and slime may be discharged by the mouth without damaging the plumage. It is also necessary to observe the proper seasons when the birds are in the best condition for preservation, and when not: during the time of incubation, the breasts and bellies are without feathers, and the skin of those parts is extremely tender; again, while birds are moulting or casting their feathers, they are not fit for preservation, the quills are full of blood, and the plumage not of its proper colours. The best seasons are in the Spring and Autumn: but, if we meet with rare birds, we must not lose the opportunity at any season, but do as well as we can. Young birds are not proper for preservation till the 2d year, because they do not, till then, acquire their proportions and colours, which may occasion their being mistaken for other species; neither is it always possible in the first year, to distinguish the sex of birds, which is very easy afterwards when they arrive at maturity; however, by grouping young birds in their nests, we may preserve them at any time, and when managed in that way they certainly add greatly to a collection.

This naturally leads Mr. K. to what, in his opinion, is by much the most ingenious and entertaining part of this kind of study, viz. the attitudes and actions of birds; all the rest is merely mechanical, but this admits of fancy, taste, and judgment. Without a proper attention to this, however sound your preservation, however vivid the plumage may be, birds are still nothing but mere dead birds; but by a skilful management of attitudes and actions, you, as it were, animate them, they seem alive, moving and acting. Though this part certainly depends in a great degree on taste and judgment, yet an accurate observer of nature will derive much information from noticing the appearance of living birds, in the attitudes and actions which he wishes to express in his preservations; the most picturesque attitude should be fixed on, and propriety observed in chusing such as are most expressive of the particular qualities of each bird, as strength and courage in eagles and hawks, &c. In grouping birds of these kinds with their prey, regard should be had to the particular part at which they begin to devour it; some begin at the breast, some at the head, some at the back, and

others extract the entrails first; the feeble scarce-resisting efforts and extreme terror of the prostrate bird, the exulting audacity and triumph of the victorious one, if properly managed, create a fine contrast. Picking, stretching, feeding, fear, surprise, and fighting, afford peculiar and striking attitudes. Mr. K. fears the word attitude does not sufficiently express his idea; he means the particular positions of the legs, wings, head, body, the manner of the feathers, and in general whatever contributes to express and mark a particular action and passion of the bird. Thus, in surprise attended with fear, the legs are extended, the body leans forward out of the equilibrium, supported almost on the toes, the wings are half expanded, the bill turned to one side, the top, if crested, spread, and the feathers, particularly those of the neck, standing perpendicular to the skin. When any part is not made to co-operate in the expression, we not only lose the additional strength which the proper action of that part would have given to the general expression, but, what is worse, the position of such deficient parts may convey an idea directly contrary to that general expression, and so make the whole unnatural, contradictory, and ridiculous. It is not unfrequent to see this absurdity in a degree that at once surprises and offends a judicious observer. Birds put in such positions as are intended to express the strongest emotions and passions, with their feathers perfectly smooth and unaffected,

"Rage with unruffled plumes and fear with folded wings:"

and this absurdity is the more striking, and therefore the less excuseable, as the action of the wings and feathers are more intelligible and expressive than those of any other parts in birds. Great attention should always be had to the poise of the body; in such positions as a live bird may be supposed to continue some time in, we must take care that the body appears in equilibrium; on the contrary, in fighting and other violent actions, where a forceable motion is to be given, the appearance of equilibrium must be as carefully avoided, for it always conveys the idea of stillness, as do the legs when placed by each other, and in the same straight direction, which they should seldom if ever be in. Bending, advancing, or retiring, one leg a little more than the other, not only gives a more graceful but a more lively and active appearance; and it is observable that living birds, standing on a plain surface, almost always turn the foot of the leg on that side to which they are looking in the same lateral direction with the head. Mr. K. cannot help observing here one fault very common with most preservers; that is, the stretching the legs of their birds down so as to bring the thigh almost perpendicular, which not only gives the bird an ungraceful but an unnatural appearance; for we seldom or never observe this in living birds, except in some particular species.

Birds appear to great advantage when picking their feathers; the tail is then expanded, the wing on that side to which the bill is turned lifted up, the other

drooping down, and somewhat extended from the side, in order to balance the body. Birds when fighting afford endless variety of attitude and expression; but certainly never any so affecting as when grouped with, and feeding their young, whose clamorous hunger, expressed by their gaping mouths and extended pinions, occasion that anxious perplexity and tender joy of the mother bird, so strongly marked by the spreading tail, the drooping wings, and peculiar position of the head.

With regard to the materials: for the liquid varnish, take raw turpentine 2 lb. camphor 1 lb. spirit of turpentine 1 quart. Break the camphor into very small pieces, and put the whole into a glass vessel open at top, place it on a sand heat till thoroughly warmed, then increase the fire gradually till the ingredients are perfectly dissolved and mixed, which will be done in about half an hour. Great care must be taken that the materials do not catch fire: to prevent accidents, it would be better, especially where the process is made in the house, to place the glass vessel in another of any metal, two thirds filled with cold water, which place over a gradual fire till it boils, keep it so, till the ingredients are dissolved and incorporated; then take the glass off and let it stand to cool, and the liquor will be fit for use. This varnish is the only liquid which he uses in making preparations.

For the dry compound, take, viz. corrosive sublimate $\frac{1}{4}$ lb. saltpetre prepared $\frac{1}{4}$ lb. alum prepared $\frac{1}{4}$ lb. flowers of sulphur $\frac{1}{2}$ lb. musk $\frac{1}{4}$ lb. black pepper 1 lb. tobacco ground coarse 1 lb. Mix the whole well together, and keep it in a glass vessel, stopped close and in a dry place. To prepare the alum, place it on an iron plate over a fire, till it ceases boiling and becomes dry and hard; then take it off, and when cool pulverize it. This method evaporates the aqueous parts of the alum, and also renders it much less corrosive. The method of preparing saltpetre is the same as that of alum; only the plate on which it is done must have an upright rim all round it, to prevent the salt running off into the fire.

With regard to dissecting the bird, &c. lay the bird on its back upon a table, covered with several folds of some soft cloth; separate the feathers of the breast and belly very carefully, so that you may come at the skin, in which, about the middle of the breast, make an incision just large enough to introduce the end of a quill-barrel, which enter and blow strongly through, until the skin is entirely detached from the flesh. Continue the incision down along the belly to the anus, and contrarywise up to the craw; double back the skin on both sides, carefully guarding the plumage with cotton to prevent its being soiled during the operation, and take out the craw. This done, run a sharp smooth skewer cross-ways through the breast, and lifting the bird up by it with the left hand, introduce with the other, one point of a sharp strong pair of scissars close by the edge of the breast-bone, and clip along by it, till the breast, together with the

fleshy parts of the belly, are entirely separated, taking great care not to cut the intestines. These must be next extracted, and all the blood and other moisture dried up with cotton, sponge, or tow, with which the cavity of the body is to be filled. Then draw down the neck within the skin until you can come at the back of the skull, out of which cut a small piece, and extract the brains; and having dried the cavity well with cotton dip a hair pencil in the liquid varnish, and wash it well with it, and over it strew some of the dry compound, and fill it up with cotton. Next apply the liquid to the outside of the skull close down to the root of the bill, and over that also strew some of the powder; proceed in the same manner with the neck, and then draw the skin back to its proper place, having first moistened it on the inside with the liquid.

We now proceed to the wings, the bony parts of which must be drawn so far on the inside of the skin as that we may come at the whole length. Cut out the most fleshy parts, or only make some longitudinal incisions into them, and apply the liquor and powder as before; then connect the two wings by small wire or strong thread well waxed; then (having removed the cotton that was put into the cavity of the body to imbibe the moisture) proceed in the same manner with the thighs, observing, if you cut away the flesh, to supply its place with cotton moistened with the varnish. In order to cure the rump, make as many incisions in it as may be, without weakening it too much, and having applied the materials as in the other parts, a sharp wire must be run into it, and continued along the under side of the back bone, to about two-thirds of the length of the body, in order to support the tail; then, with a pencil, varnish over the back and inside of the skin, and apply the powder. Stuff afterwards the cavities of the craw and body with the following herbs, viz. tansy, wormwood, hops, and tobacco, of each an equal quantity, well dried, and cut small.

The next thing is to take particular notice of the breast.—Out of any soft free wood, cut an artificial one as near the shape of it as possible; which being fitted to its proper place, and moistened with the varnish, must be overlaid with cotton; and the skin be drawn over it, being first varnished on the inside. In sewing up the incision, observe to stick the needle always outwards; as you proceed, moisten the seam with some of the liquid; and when finished, dispose the feathers into their natural order. The eyes, must be extracted, as no art can preserve them, so as to look full and lively, for the aqueous humour will dry up, and of consequence the outward tunica become shrivelled and without lustre. In extracting them, great care must be taken that none of the humour drop on the plumage, as it would spoil wherever it touched; the best way is, to stick a sharp pointed awl through each of them, and pluck them out together. They must be laid aside in order to finish the artificial eyes by. Chuse for that purpose beads of as large size as you can conveniently introduce into the orbits; take a long

slender needle threaded with strong silk waxed over, and run it through the hole in the upper part of the mouth, and out at one of the eyes, leaving three or four inches of the silk hanging out at the bill. This done, put one bead on the thread, and run the needle out at the other eye; draw the bead into the orbit, at the same time lifting up the eye-lid with a sharp needle, and place it over the edge of the artificial eye in a natural position; then, with a pencil introduced from the other side, varnish all the cavity with the liquid, and fill the space between the eyes with cotton, so as to keep the bead, already placed, in its proper place. Put on afterwards the other bead, and returning the needle through the orifice in the upper part of the mouth, draw in the other eye, to its proper orbit, lifting up the lid as before. If the eyes are not sufficiently protuberant, you may introduce more cotton by the orifice, through which the threads lead; and when you have by this means fixed the eyes properly, tie the ends of the silk, and cut them off. There is another method of setting the eyes, which is by introducing the beads by the orifice in the roof of the mouth, and when they are placed, stuffing cotton through the same passage to keep them firmly in their places. The stop of cotton must now be taken out of the throat, and some of the same material thrust down very carefully by a little at a time, with a quill, to support the neck in its plumpness when it becomes dry.

We now come to the methods of placing and retaining the birds in the attitudes we would have them; and first, we must provide the legs with wires sufficient to support the weight of the body; which is done in this manner: Take a brass or iron wire of a proper thickness, and made sharp at the point; which run through the foot, up the leg and thigh, through the cavity of the body, on the inside of the wooden breast, and so up the neck, and out at the upper part of the head, just above the bill. The point being then made very slender, and turned back like a hook, take hold of the other end of the wire below the foot, and draw it back till the hooked point has fixed in the head, and by it you may adjust the length and position of the neck and head. The wire which is put through the other foot and leg, &c. need not extend to the head, half way along the body will be sufficient. Next prepare a piece of wire for supporting the tail; this must be about two-thirds the length of the whole body; sharpen it at one end, and bend the other like a hook, run in the sharp end just below the rump, and push it along under the back-bone till the hook is firmly fixed over the rump, among the large feathers of the tail. The next thing is, to fix the bird on the perch or branch, on which you would have it stand; in this you will make two holes at the distance you propose the feet to be, and after having inserted the wires which are run through the feet and legs, bend the legs and every other part into the attitude you would have them. The wings must also have a wire to themselves, in order to keep them in the designed position; this is done by

sharpening the wire at one end, and running it first through one wing and through the body, out at the other wing, both being in their proper places: then the feathers must be disposed in the manner most proper to the position of each part, and the expression intended to be conveyed. The feet and bill may be varnished over with the same sort of varnish that is used for the preservation. The bird must then stand for a day or two in an airy place for the varnish to penetrate and fix; and lastly, the bird must be baked in an oven; it is not absolutely necessary, but it makes them dry, and finishes the preservation immediately. If the bird has been some time dead and has any disagreeable smell, this method makes it perfectly sweet; but care must be taken not to put them into the oven while it is too hot, as it would blister the bill and nails. The best rule to know when the oven has a proper degree of heat, is this; while the oven is cooling, throw in now and then a tail feather taken from any fowl, which must be placed about the middle of the oven. If it is too hot, the feather will have a motion and be bent: we must therefore wait a while, and put in another feather, till we observe there is no motion or bending; then on taking it out, and bending it with the finger, if it breaks, the oven is still too hot, and we must wait till feathers that have been in for a few minutes will bend without breaking. When the oven is thus fit, the birds must be put in, and the door of the oven closed, till it is quite cooled.

The birds in this manner will be perfectly preserved; but as there still remains some oily matter in the feathers, the moths and other insects will deposit their eggs and generate their young in the plumage, if the birds are not carefully cased up. The cases must be first well washed on the inside with the following camphorated spirits, viz. Take one pound of camphor and boil it in half a gallon of spirit of turpentine till well dissolved; and while hot, wash all the inside of the cases by means of a brush, and, as soon as dry, place the birds in, and close it up, and guard the joints of the doors or seams with paper or putty. Though the room, in which the cases of birds, &c. are kept, cannot be too dry, the sun should not be permitted to shine in, as it will certainly discharge the finer colours of the plumage. Baking is not only useful in fresh preservations, but will also be of very great service to old ones, destroying the eggs of insects; and it should be a constant practice once in 2 or 3 years, to bake them over again, and to have the cases fresh washed, as above, which would not only preserve collections from decay much longer, but also keep them sweet.

XXVII. Description of the Blunt-headed Cachalot. By James Robertson, Esq. of Edinburgh. p. 321.

A physeter cætodon Linnæi, or blunt-headed cachalot, ran ashore on Cramond Island, and was there killed, December 22, 1769. Cramond Island is in the

Firth of Forth, 4 miles above Leith. The fish measured 54 feet in length; its greatest circumference, which was a little behind the eyes, thirty. The head was nearly half the whole fish, fig. 6, pl. 1, of an oblong form, and rounded, except within 6 feet of the extremity, where it had inequalities. The body was rounded, and gradually tapered to the tail, except about the middle of the back opposite to the penis, where there was a bump or protuberance, but no fin. The tail, as in all the whale tribe, was placed horizontal, a little forked; the blades were of a wedge shape, and 14 feet from tip to tip. In the lower jaw, which was 11 feet long, were placed 23 teeth on each side, each 2 inches long, and all pointing a little outwards. The upper jaw, projecting 5 feet over the lower, was quite blunt or truncated, 9 feet high; and the spout-hole, placed at its upper part, appeared to be provided with a sphincter. In the upper jaw, were 23 sockets on each side, for lodging the teeth of the lower, when the mouth was shut; but no teeth. The eyes were remarkably small in proportion to the size of the animal, and placed in the most prominent part of the head. The pectoral fins were placed 5 feet behind the corners of the mouth, and measured 3 feet in length and 18 inches in breadth. The penis was 7 feet and a half long and placed 19 feet behind the corners of the mouth, inclosed in a strong sheath, the mouth of which was shut with a sphincter: 5 feet behind it was placed the anus, likewise furnished with a sphincter, and the distance, from the anus to the division in the blades of the tail, was 14 feet.

The cuticula, or scarf-skin, was extremely thin; on the upper part of the head and whole body, of a bright grey colour, and on the under part of the head of a dirty white; it was smooth and slippery to the touch, easily torn off, and when viewed between the light it appeared scaly. The true skin was of a black colour, about $\frac{1}{4}$ of an inch thick, adhering firmly to the fat, or blubber, which was from 4 to 9 inches thick. Below the fat every where were tendinous cords, of a bright straw colour, very elastic, strong, and covered with a loose thin membraneous coat. On the abdomen were 3 layers of those tendons, which crossed each other obliquely, and, in their direction and situation, greatly resembled the obliquus ascendens and the transversalis of the human body, and they became fleshy where the linea alba is in the human body, and at the lumbal vertebræ. The tendons which appear to arise from the upper ribs, the dorsal vertebræ, and the vertebræ of the neck, arose fleshy, were both flatter and stronger than in any other part of the body, and running along the head with little obliquity, seemed to be inserted tendinous into the cranium, &c. Considering the tail as the os sacrum or a continuation of the spine, the tendinous muscles belonging to it arose towards the process of one vertebra, and running almost round, was inserted into the process of another, and have much the same effect on the tail as the supinators and pronators have in turning the hand; which circumstance,

if true, must be of great utility in performing the several motions necessary in the progression of this animal.

The substance, improperly called spermaceti, and erroneously said to be prepared from the fat of the brain, was every where contained in a fluid state in the cavity of the head along with the brain, but quite distinct from it. Was this substance in a state of fluidity when the animal was in life? Very probably not; but it turned into that form by means of a heat occasioned by a fermentation of the different fluids, which soon began after the death of the fish, and increased to such a degree as at length to cause many cracks in the skin, to burst the body in the back, and to throw out the abdominal viscera at that aperture. After this eruption the spermaceti was found every where around the fish, floating on the water in a congealed state. From which circumstance, it seemed to be contained throughout the whole body, and to have run out at these cracks, &c. but on examination, it was found to have run out at the mouth only. How found it a passage from the head there? To come at that fluid, the workmen made a hole into the cavity of the head at (a) and took it out with a skimmer from among the substance of the brain, as it flowed to the hole, which it did like water springing up into a well. When it was taken out, it was hot, and of a clear oily colour; but being exposed to the cold air, it immediately congealed into a white mass.

XXVIII. Experiments and Observations on various Phenomena attending the Solution of Salts. By R. Watson, A.M., F.R.S.* p. 325.

Reprinted in the 5th vol. of this learned prelate's Chemical Essays.

XXIX. An Account of an Occultation of the Star ζ Tauri by the Moon, observed at Leicester. By the Rev. Mr. Ludlam, in a Letter to the Rev. Mr. Maskelyne, Astron. Royal. p. 355.

This occultation of the star ζ Tauri by the moon, was observed at Leicester, April 28, 1770. The immersion was noted at $9^h\ 41^m\ 7^s$ by the clock. It might be 2 seconds sooner, because the clock being of necessity at a distance from the telescope, the instant of the immersion was signified by striking on a bell. The emersion was about $10^h\ 31^m$, but with some uncertainty, the star being hid by a cloud at its first coming out.

By the observed transits of the sun and stars, the clock lost 3 seconds between the 25th and 28th. On the 25th, by corresponding altitudes, the clock was $1^m\ 46^s.8$ too slow; whence on the 28th it was $1^m\ 49^s.8$ too slow. This confirms the observation made by corresponding altitudes on the 28th, by which it was $1^m\ 50^s$ too slow at noon: the clock was then losing at the rate of 4 seconds a

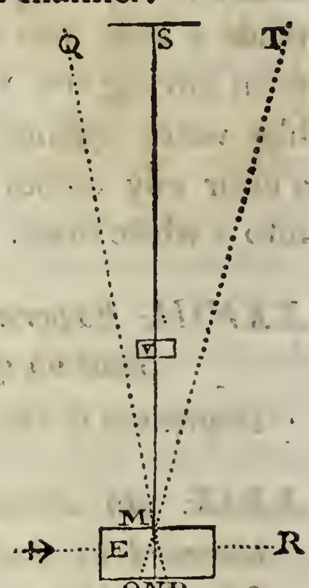
* The present Bishop of Llandaff.

day; whence, on the 28th, at 9^h, it was $1^m\ 51^s.5$ slower than mean time. The equation of time on the 28th at 9^h was $2^m\ 47^s.5$, whence the immersion was at $9^h\ 45^m\ 44^s$ apparent time.

The telescope made use of was one of Dollond's, with a triple object glass of $33\frac{1}{3}$ inches focal distance, and which magnifies 52 times.

XXX. On the Transit of Venus. By John Winthrop, Esq., F. R. S., at Cambridge, New England. p. 358.

I find, says Mr. W. that Mr. Bliss and Mr. Hornsby, in their calculations in the Philos. Trans., suppose the phases of the transit of Venus, to be accelerated by the equation for the aberration of light, which amounts to 55^s of time. According to my idea of aberration, I should think the transit would be retarded by it. I can very easily suppose that I am in an error; and that I may more readily be led out of it, I beg leave to lay before you the several steps by which I have been led into it. And I think it will be best to take some similar instance, rather than to consider the thing in a general abstract manner.



1. Let the parallelogram E represent a vessel sailing in the line LR, from left hand to right, and S, a fixed station, e. g. a castle, discharging balls in the right line SM, perpendicular to the route of the vessel. If the vessel had been at rest, a ball arriving at the middle of it, M, would have gone right across it, to N. But as it is supposed to be sailing, the ball will not go right over from M to N, but will cross the deck obliquely, in another right line, as MO, and so will be left behind towards the stern as much as the vessel had gone forward, while the ball was crossing it; and MN will be to NO as the velocity of the ball to the velocity of the vessel. Thus, to the people on board, the ball would seem to move obliquely across the deck, as if it came from some point T in the line OM, produced, instead of coming from S. And a tube capable of receiving the ball, would allow the ball to pass through it without striking its sides, if it were inclined forward in the direction OM; which it would not do in any other situation. The angle OMN or SMT answers to the aberration; and supposing S to be the sun, and E, the earth, this angle is 20″; and the general effect is, to make the sun, or any fixed star, to appear further that way towards which the earth is moving.

2. Let us suppose another vessel V, between S and E, sailing the same way as E, in a parallel direction. If both the vessels sailed with the same velocity, a ball from V coming to M, would go right across to N, just as if both of them had been at rest; because the ball, while crossing the vessel E, would be carried just as far to the right hand as the points M and N are. And a tube to receive it

must be held in the direction MN. So here would be no aberration of the vessel V.

3. Suppose V to move the same way, but slower. A ball from V would now be really carried forward, that is, to the right hand, though not so far as in the second supposition; and therefore would be left behind in respect of the vessel E; and so, would come to the side of the vessel somewhere between O and N; but the greater its velocity towards the right, the nearer to N. So that if the velocity of V were to be continually increasing from nothing till it became equal to that of E, a tube to receive the ball must be held first in the direction OM, looking forward, and afterwards, more and more inclined till it came into the perpendicular direction MN. Hence it is natural to conclude,

4. That if V move the same way, but swifter, a tube to receive the ball must be reclined backward. For the ball would now be carried to the right hand further than in the 2d supposition; and therefore would come to the other side of the vessel at some point P on the right hand of N, as if it proceeded from some point Q on the left hand of S.

This last seems to be the case of the transit, by supposing S to be the sun, E the earth, and V the planet Venus passing between them from left to right, and with a greater velocity than the earth, greater nearly as 24 to 20. And it should seem that the aberration must make Venus appear farther to the left hand, or to the east from the sun, and consequently retard the transit, and make it happen later than it would otherwise do.

XXXI. On the Transit of Venus, the Lengths of Pendulums, also the Inclination and Declination of the Magnetic Needle. By Mr. Mallett, of Geneva, in a Letter to Dr. Bevis, F. R. S., dated April 13, 1770. p. 363.

I had the pleasure of writing you a few lines in August, last year, when I sent you my observations relative to the transit of Venus, which the Petersburg Academy has printed without my knowledge, while I was yet in Lapland. I left Russia soon afterwards, and have been 5 or 6 months in my own country. Part of this time I have employed in reducing and computing my observations made in the north, to get what useful results I could from them, which I have just now sent to Petersburg, to be printed in the Commentaries of the Academy. As it may be some time before that volume will be published, I thought, Sir, you might be willing to be informed of some of the principal consequences resulting from my observations.

1°. To determine the latitude of Ponoi, where I observed the transit, a great number of meridian altitudes of the stars and sun, taken with a quadrant of 2 feet radius, made at London by Mr. Sisson, gave the elevation of the pole 67° 4′ 30″. I was not able to make any other observation but that of the sun's

eclipse, on the 4th of June, for determining the longitude. I observed with a 12 feet achromatic telescope of Dollond, the end at $0^h\ 7^m\ 55^s$ apparent time. The celebrated M. L. Euler, who has computed several observations of this eclipse made in different places, finds, by my observation, the difference of meridians between Paris and Ponoi $2^h\ 25^m\ 33^s$, that is, $38^\circ\ 51'$ East of Paris.

2°. On the observation of the transit of Venus.—I observed, with the same telescope, the interior contact at the entry at $10^h\ 15^m\ 4^s$ apparent time. I have computed very scrupulously the effect of parallax on the moment of this contact; I made use of the same elements that M. de la Lande gives in his memoir, printed in 1764, excepting that I assume the nearest distance of the centres of Venus and the sun, seen from the centre of the earth, $10'\ 27''$; which quantity I deduced from the whole duration, observed at Hudson's Bay, by Messrs. Dymond and Wales, as given in the newspapers. I find the effect of parallax $7^m\ 3^s$ of time, by which I must have seen the contact sooner than at the earth's centre. The computation of my observation gives the moment of the conjunction at $12^h\ 46^m\ 21\frac{1}{2}^s$ apparent time at Ponoi, and the geocentric latitude of Venus for that moment $10'\ 33''.9$.

If the nearest distance of the centres be taken $5''$ less, I find the effect of parallax $7^m\ 11^s$ of time, that is, 8^s greater, the latitude becomes $5''$ less, and the moment of the conjunction $1^m\ 28^s$ later.

3°. I have made a great number of observations for determining the force of gravity and the length of the simple pendulum swinging seconds. I used an invariable pendulum which M. de la Condamine got constructed at Quito, when the French academicians went thither to measure a degree of the meridian, which he was pleased to send me to Petersburg; this pendulum, which is no other than a simple steel rod fixed to a lentille, made at Para 98740 oscillations in 24 hours of mean time, and at Paris 98891 in the same time. I made experiments with this same pendulum at Petersburg, before my departure for Lapland, and have repeated them since my return thither. They give the number of oscillations in 24 hours of mean time 98941, having been careful to preserve constantly the same temperature, and to cause the pendulum to swing very small arcs. At Ponoi, I found the number of oscillations 98946. Hence it follows, that the simple pendulum, which beats seconds at Petersburg, will be 441.02 lines, Paris measure, that is $\frac{45}{100}$ lin. longer than the pendulum which beats seconds at Paris; and the pendulum at Ponoi will be 441.22 lin. that is $\frac{65}{100}$ lines longer than that of Paris.

The excess of the Paris pendulum, above that at the equator, has been determined by the academicians 1.50 lin. and admitting Sir Isaac Newton's principle, and Huygens's, that the increase of gravity, in approaching the pole, follows the ratio of the square of the sine of latitude, we should find 1.98 lin. for the excess of the Petersburg pendulum above that at the equator, instead

of 1.95, which I find by my experiments; the same calculus would give 2.24 lin. for the excess of the Ponoi pendulum, instead of 2.15 lin. which results from my experiments. Hence it would follow that the increments of gravity follow a ratio somewhat greater than that of the squares of the sines of latitudes; and this result is confirmed by the experiments made at Pello in Lapland, by the French academicians.

4°. I observed several times very exactly at Ponoi, the declination of the magnetic needle 1° 10′ east.

5°. Exact observations of the inclination of the needle made in different places of our globe, combined with those made long ago on the declination, would be very interesting and proper for the advancement of our knowledge, as to the theory of magnetism, which hitherto is but little understood. It is the difficulty of making such observations, and obtaining accurate results, which hitherto is but little understood. It is the difficulty of making such observations, and obtaining accurate results, which has discouraged philosophers and travellers; but it is surprizing that so little has been done in this matter, since Dr. Daniel Bernoulli furnished us with new ideas for constructing a machine fit for determining the true magnetic inclination, in a memoir, which gained the prize proposed for this subject by the Paris Academy, in 1743. He got an inclinatory needle constructed at Basle, on new principles, and the experiments he made assured him of success; he found the inclination at Basle 71½ degrees.

Mr. Euler, the son, made use of the same compass at Berlin, but by employing a method entirely different from that of Dr. Bernoulli. He gives the particulars in the Memoirs of the Academy of Berlin, 1755. After a great number of observations, he found the inclination to be then at Berlin between 72½ and 73 degrees. At Petersburg I got constructed a like machine, and used it for determining the inclination at Petersburg, Kola, and Ponoi; I employed both the methods of Messrs. Bernoulli and Euler, and found a wonderful agreement in the results drawn from a great number of experiments. Two needles made by different artists, one at Basle, the other at Petersburg, consequently susceptible of a different magnetic force, produced but very minute differences, inevitable in such delicate experiments; the several particulars are recited in the papers I have sent to the Petersburg Academy, whence it may be concluded that it is possible to determine with this instrument the true inclination of the magnetic needle, without being any way liable to an error of half a degree in the result, which in my experiments is as follows: viz. in 1769, the inclination,

At Petersburg, lat. 59° 55′, long. 48°, ... was 73°⅔
At Ponoi. . . . . lat. 67 4, long. 58° 51′. 76½
At Kola . . . . . . lat. 68 54, long. 49 45. . . . . 77¾

4

6°. I have also subjoined the several particulars of my meteorological observations, during my 5 months stay in Lapland. Suffice it to give you the mean height of the barometer, which I found 27 in. 6.2 lin. Paris, in March; 27 in. 5.5 lin. in April; 27 in. 7.6 lin. in May; and 27 in. 5.8 lin. in June; the mean height for 4 months being 27 in. $6\frac{1}{7}$ lin. I could not well measure exactly my elevation above the level of the sea, but I take it not to exceed 40 or 50 toises.

XXXII. Experiments on the Blood, with some Remarks on its Morbid Appearances. By William Hewson, F. R. S., p. 368.

Reprinted in the 1st vol., of this author's collected works.

XXXIII. and XXXIV. On the Degree of Heat which Coagulates the Lymph, and the Serum of the Blood; with an Enquiry into the Causes of the Inflammatory Crust, or Size, as it is called. By the same. p. 384, and p. 398.

May be consulted in the vol. of Mr. Hewson's works before referred to.

XXXV. Account of some Bones found in the Rock of Gibraltar, in a Letter from John Boddington, Esq., to Dr. Wm. Hunter, F. R. S., with some Remarks by Dr. Hunter. p. 414.

I beg your acceptance of a piece of the rock of Gibraltar, which my friend Colonel Green, chief engineer of that garrison, has brought from thence, and given to me as a natural curiosity: it appears to be a very extraordinary one indeed: therefore, I shall attempt to explain to you the manner of discovering it, and leave the rest to your better judgment. You must know then, Sir, that Gibraltar is always attended to with great circumspection. The city, town, and fortification are all upon a rock, and sand; of which the whole peninsula is composed: as nature changes the face of the rock, the engineers have a watchful eye to apply art in forming the defences where nature fails; a particular instance of which happened in the course of the present year, by the craggy part of the rock falling away, so as to admit the probability of an entrance into the fortification; to obstruct which, a wall was erected 70 feet distant from the sea shore, and 57 feet perpendicular above high water mark. In blowing up the rock to make way for the foundation of the wall, there were discovered considerable quantities of petrified bones, as you may perceive on examining the piece of rock, which you may be certain was taken from the spot by Colonel Green, and has been in the possession of no person but himself.

Dr. Hunter's answer.—By the examination of two pieces of the rock of Gibraltar, which are in my possession, I find that they are not, what I at first, took them to be, human bones, but those of some quadruped. I discovered this, with my brother's assistance, by clearing the teeth of the crust that covered

them, so as to see their shape more distinctly. The two masses of bones are blended with pieces of the marble, of which the whole rock of Gibraltar, as I am informed, is composed; and all the constituent pieces are cemented strongly together with a brownish coloured calcareous crystallization, or stalactite. Where the interstices are large, there are vacant spaces, and the surfaces of all such cavities are covered with granulated crystallization about $\frac{1}{6}$ of an inch thick. This crystallized crust, no doubt, was deposited from the water passing through the cavern in which the bones had been lodged; and by soaking through the porous substance of every bone, the water had likewise deposited a crust of the same nature, but much thinner, on all the internal surfaces of the hollow and spongy bones. The bones were not in any other sense petrified.

XXXIV. Difficulties in the Newtonian Theory of Light, Considered and Removed. By the Rev. S. Horsley LL.B., F.R.S.* p. 417.

Dr. Franklin, in a letter to a correspondent at New York, the 23d of that valuable collection with which the public was obliged in the latter end of the year 1768, proposes some objections to the Newtonian theory of light. His words are these: "I am much in the dark about light. I am not satisfied with the doctrine that supposes particles of matter called light, continually driven off from the sun's surface with a swiftness so prodigious. Must not the smallest particle conceivable have, with such a motion, a force exceeding that of a 24-pounder discharged from a cannon? Must not the sun diminish exceedingly by such a waste of matter, and the planets recede to greater distances by the lessened attraction? Yet these particles, with this amazing motion, will not drive before them, or remove, the least and lightest dust they meet with: and the sun, for aught we know, continues of his ancient dimensions, and his attendants move in their ancient orbits."

Dr. Franklin's questions are of some importance, and deserve a strict discussion. On the supposition that light is a copious emanation of innumerable small particles of matter from the sun, I had once occasion to inquire, what the force of motion produced in every such emission could possibly amount to at the utmost. For this purpose, I made an estimate of the greatest probable magnitude of the particles of light; and of the greatest density of each. I likewise computed the greatest number of such particles, that could possibly fly off at once from the surface of the sun; supposing the sun's horizontal parallax to be no more than 8″. These computations, with an account of the principles on which they were founded, having been already given to the public;* I shall make use

* In a little treatise, entitled, the Power of God, deduced from the computable instantaneous productions of it in the solar system.—Orig.

of the results, which I shall here briefly state, as data for the discussion of Dr. Franklin's questions.

I suppose the particles of light to be equal spherules. This perhaps is not the case. Each color has probably its own size; but there will be a mean size, which is sufficient for my purpose. This mean size I suppose to be so small, that the diameter of each spherule does not exceed one millionth of one millionth of an inch. I shall show hereafter, that there is much reason to suppose, that the particles of light are in fact much less than spherules of this diameter. I suppose the density of each particle 3 times that of iron.* The number of such spherules that contain as much matter as an iron ball of one yard diameter, I have found to be 15552 xxxvi;†‡ consequently 576 xxxvi such spherules contain as much matter as an iron ball of 1 foot diameter. Let such a ball be supposed to move uniformly with a velocity that should carry it 1000 yards in 1″. The light of the sun traverses the semidiameter of the orbis magnus in 7^m nearly.

In the ensuing calculations, I shall reckon the sun's horizontal parallax 9″. According to this hypothesis, the semidiameter of the orbis magnus will contain 22919 semidiameters of the earth. In 1^s of time, light, according to the velocity assigned to it, traverses $\frac{1}{420}$ of this space, or 54.57 semidiameters of the earth, or 381092323 London yards. Hence the velocity of each particle of light, will be to that of the iron ball moving 1000 yards in 1^s, as 381092 to 1, very nearly. And the ball being 1 foot diameter, it has been shown that the matter in each particle is to the matter in the ball, as 1 to 576 xxxvi. Hence the force of the motion in each particle of light, is to the force of motion in the ball, as 381092 to 576 xxxvi; that is, as 1 to 1511444 xxvii, or, it is equal to the force of motion in an iron ball of 1 foot diameter moving $\frac{1}{1511444 \text{ xxvii}}$ of 1000 yards in 1^s, or to that of an iron ball of 1 inch diameter moving $\frac{1}{874679 \text{ xxi}}$ of 1 yard in 1^s, or to that of an iron ball of $\frac{1}{4}$ of an inch diameter moving $\frac{1}{13666 \text{ xxi}}$ of 1 yard in 1^s; that is, moving less than $\frac{1}{4555 \text{ xxi}}$ of 1 foot in 1^s; that is, moving less than a foot in 4555xxi seconds, or in more than one hundred forty-four thousand millions of millions of Egyptian years; or the force of motion in each particle of light coming from the sun, is less than that in an iron ball of $\frac{1}{4}$ of an inch diameter, moving at the rate of less than an inch in 12 thousand millions of millions of Egyptian years.

Dr. Franklin's first question is answered. A particle of matter, which is probably larger than any particle of light, moving with the velocity of light, has a

* Instantaneous product, &c. p. 30.

† Ibid.

‡ The Roman numerals placed after the Arabic characters, denote the number of zeros that must be added to the Arabic figures, to complete the expression of the number intended.—Orig.

force of motion, which, instead of exceeding the force of a twenty-four pounder, discharged from a cannon, is infinitely less than that of the smallest shot discharged from a pocket pistol, or less than any that art can create.

I proceed to the other questions.—And, I think that I shall make it appear, that it is very possible that light may be produced by a continual emission of matter from the sun, without any such waste of his substance as should sensibly contract his dimensions, or sensibly alter the motions of the planets, within any moderate length of time. Indeed, I do not think it necessary to the production of any of the phenomena of light, that the emanation from the sun should be continual in a strict mathematical sense, or without any interval. It seems sufficient to all purposes, that the intervals should be exceedingly short. But this I only mention.—I think it possible that a continual emanation might subsist, without any such dangerous consequences to the solar system, as Dr. Franklin apprehends. Dr. Franklin's character is not more distinguished by his superior talents, than by a candor truly philosophical. And on this circumstance I build the strongest confidence, that he will not be offended, that I differ from him: that, as a friend to inquiry, he will be pleased that I take the liberty to communicate my own notions, however opposite they may be to his.

It will be easily understood, that a continual emanation from the sun does not necessarily imply a continual waste, or loss, equal to the emanation.—If light is continually issuing from the sun in all directions, part of this is continually returning to him, by reflection from the planets, and other light is continually coming to him, from the suns of other systems. It is true, that the light which he receives, is but a very small part of the light which he gives. For if the light always coming to the sun were equal to the light always going from him, our atmosphere would be as strongly enlightened in the night as in the day.—But this is not the case; and the proportion of the light that comes, to the light that goes, cannot be greater than that of night-light at a medium, to day-light at a medium—still it is something—and the continual loss of substance that the sun sustains cannot be more than the difference between the light that he gives, and the light that he receives. And therefore, if there were no other recruit of the sun's substance (which is by no means a probable supposition) yet the continual waste will, on this account alone, be less than the continual emission; and the sun cannot lose so much of his substance as a single emission of light contains, but in some determinate time.

I shall suppose that the sun gives so much more than he receives, that he loses the amount of one emission in every second of an hour. Let us see what will be the consequence. Every particle of light that issues from the sun, must leave a spherical vacuity of one millionth of one millionth of an inch diameter. The greatest number of particles of this size that can issue from the surface of the

sun at once, if the horizontal parallax be 9″, is 104666 xli,* because this is the greatest number of such particles that would have room to lie at once on his surface. And the same will be the greatest number of spherical vacuities made by one emission. Many of these vacuities are no sooner made, than they are filled up by the light that is coming to the sun from other systems, or returning to him from the bodies of his own, or, perhaps with other matter which he may receive in various emanations from the planets; for I strongly suspect that a perpetual circulation of finer matter may subsist between all the large bodies of the universe. The emission of light, however, is supposed so far to exceed the whole supply, that in every second, over and above the vacuities that have been filled up with adventitious matter, the foregoing number, 104666 xli, have been formed that have received no such supply. The fluid matter of the sun† rushes from all sides into these, and fills them up to its own level. The sun by this means shrinks a little, and loses, once in every second, so much of its solid content, as the solid contents of these vacuities amount to.

I have found that the solid contents of 46656 xxxvi such vacuities are equal to the sphere of one yard.‡ Therefore the solid contents of 104666 xli such vacuities, are equal to 224335½ times the sphere of one yard. And so much is the sun's solid content diminished every second. The sphere of the earth is to the sphere of one yard, as 27247031 xiv to 1.§ Therefore the sphere of the earth is to 224335½ times the sphere of one yard as 121456 xi to 1. Therefore the sun loses an earth of its solid content in 121456 xi seconds, or 385130000 Egyptian years nearly.

If the sun's parallax be 9″, the solid content of the earth is $\frac{1}{1217420}$ of the solid content of the sun. Therefore, in 385130000 Egyptian years, the sun should lose $\frac{1}{1217420}$ of his solid content, and consequently in the same time the diameter of the sun should lose $\frac{1}{3652260}$ of its whole dimensions. And in the same time the apparent diameter should lose the like part of its quantity, if the distance between the earth and sun remained unaltered. The sun's mean apparent diameter contains 1922 seconds. Therefore $\frac{1}{3652260}$ of the ☉'s apparent diameter, is $\frac{1}{1900}$ of one second very nearly. So inconsiderable would be the whole diminution of the sun's apparent diameter, that could arise from the waste of his substance, in 385130000 Egyptian years.

* The greatest number of particles that can issue from the sun at once, was reckoned in the Instant. Product. 132467 xli; but then the ☉'s parallax was reckoned only 8″.—Orig.

† I suppose the sun to be a fluid mass: by this hypothesis, I give the utmost force to Dr. Franklin's objection; for, the more perfectly the sun is fluid, the more suddenly will the vacuities be filled up.—Orig.

‡ Vide Instant. Product. p. 30.—Orig.

§ Ibid. p. 16.—Orig.

But the waste of the sun's substance must lessen the attraction between the earth and sun. As the attraction lessens, the earth will recede to greater distances. And hence there will arise a further diminution of the sun's apparent diameter, and a prolongation of the anomalistic year. The density of each particle of light has been supposed 3 times that of iron, or 23 times the mean density of the earth.* Therefore, as often as the sun's loss by light amounts to an earth in size, it will amount to 23 earths in matter. The matter of 23 earths is $\frac{1}{13232}$d of the sun's matter, if the sun's parallax be 9″. Therefore in 385130000 Egyptian years, the sun loses $\frac{1}{13232}$d of his mattter; and the gravitation towards the sun, at any given distance, diminishes in the same proportion. But this alteration is much too small to discover itself in the motions of the earth or any of the planets. I will not at present consider, by what law the distance of the planets from the sun would increase, because the inquiry could not be reduced to a small compass; but it is obvious that, whatever that law may be, it must arise solely from the diminution of gravitation: and the like is to be said of the anomalistic periods. And therefore, while the diminution of gravitation is insensible, the changes in these circumstances must be insensible too. Of all the changes to which our system may be obnoxious, those which should arise from the waste of the sun's substance in light, on the supposition that light is an actual emanation of matter from the sun, reckoning that waste at the utmost, are perhaps the least considerable.

In the foregoing computations, the instantaneous emission of light has been greatly over-rated. For if the particles of light were of the magnitude and density which has been assigned to each, and were to issue from the sun in the close arrangement that has been supposed, they would form a sort of crust on the sun's surface, at least 12 times more dense than water, i. e. 9600 times more dense than our atmosphere in the parts next the earth's surface, if the density of common water compared to that of air be reckoned only as 800 to 1.† But if the density of light on the sun's surface be 9600 times that of our air, its density when it arrives at the earth, or its density on the surface of the orbis magnus,‡ should be more than $\frac{1}{5}$th part of the density of our air. When substances of different specific gravities, as a piece of gold and a piece of cork, descend, in the exhausted receiver, with equal velocities, and fall equal heights

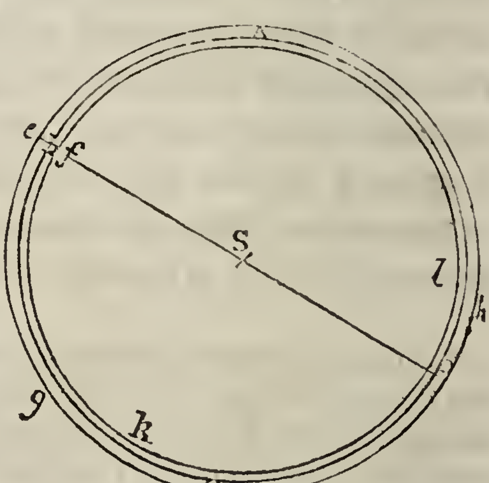
* I reckon the mean density of the earth no greater than that of common water. It is certain that it cannot be less. Sir Isaac Newton reckons it 5 or 6 times greater; but I confess that I am not satisfied with his reasons for making it so great. If I have underrated it, I have, in so doing, given the advantage to Dr. Franklin's objection.

† It is well known that the density of water to that of air, is as 850 to 1 at least.

‡ By the surface of the orbis magnus, I mean to denote a particular place in absolute space; namely the surface of that sphere which is concentric with the sun, and hath the earth's mean distance from the sun for its semidiameter.

in the same time, it is obvious, that the density of the medium, through which they fall, bears no sensible proportion to the density even of the lighter substance. The medium through which such substances fall, in a transparent glass receiver, is composed of some small portion of rarefied air, and, to all appearance, of the same quantity of light, as the receiver contains before exhaustion. For the quantity of light, in a transparent receiver, can by no means suffer any diminution, by the action of the air-pump. The density of the light therefore, in our air, is certainly too small to bear any sensible proportion to that of gold or even of cork. And the density of cork bears, though a great, yet a finite and a sensible proportion to that of the atmosphere; because in air gold and cork do not descend with equal velocities. Hence, I think, I may conclude with the greatest certainty, that the density of the sun's light at the earth, or on the surface of the orbis magnus, is too small to bear any sensible proportion to the density of common air; and I shall hardly underrate the density of the light, if I reckon it only $\frac{1}{10000}$ part of the density of the air, or $\frac{1}{8000000}$ part of the density of common water. Suppose this to be the density of light upon the surface of the orbis magnus, and it will be found by computation, that its density on the sun's surface, must be less than $\frac{1}{173}$ of the density of common water. From these considerations, I think it may be concluded with the greatest certainty, that the quantity of matter that issues from the sun in light, has been greatly overrated in the foregoing computations.

I apprehend, however, that the density of each separate particle cannot be less than has been supposed: but that the magnitude of each is less, and the arrangement less compact.* Let the density of each, and the number that issues at any one time from the sun, remain as before; and let us consider, in what proportion the magnitude of each particle, must be diminished so that they may altogether form a fluid, on the sun's surface, 173 times less dense than water. Let ABCD represent a section of the sun's sphere, through the centre S. In the spherical surface ABCD, take any point B. Join SB, and take B*e*, B*f*, each equal to the semidiameter of a particle of light. On the centre S, at the distances S*e*, S*f*, imagine two other spheres, *egh*, *fkl*, one inclosing, the other inclosed within the sphere ABCD. The solid space *efklgh*, is the space that contains all the particles of light, with their interstices, that issue together from the spherical surfaces ABCD; and because *ef* bears an exceedingly small proportion to SB, therefore the spherical surfaces *egh*, *fkl*, are very nearly equal each to the other, and to the spherical



* Vide Instant. Product. p, 37.

surface ABCD; and the solid space contained between them, is very nearly equal to a cylinder on a base equal to the spherical surface ABCD, and of altitude equal to *ef*. Therefore diminish the diameter of each particle of light, that is, diminish *ef*, in any proportion whatever, and the solid space ABCD $\times$ *ef* diminishes in the same proportion. And if the matter in that space were given, the density of it would always be as $\frac{1}{ef}$. But the matter of each particle of light, and consequently the matter of the given number in the space ABCD $\times$ *ef*, is always as ef^3. Therefore the density of the substance formed by light in the space ABCD $\times$ *ef*, is always as $\frac{ef^3}{ef}$, or as ef^2.

That is, the number of spherical particles of light, in the solid space ABCD $\times$ *ef*, being given, and the density of each particle, and the spherical surface ABCD being given, the square of the diameter of each particle will be as the density of the substance they compose, on the surface of the sun. But it has been found, that if the diameter of each spherule were one millionth of one millionth of an inch, the greatest number of such spherules that the sun's surface can contain at once, would, with the density which has been assigned to each separate particle, form a substance upon it 12 times more dense than water. Therefore, that the same number of such particles should form a substance about 173 times less dense than water, the diameter of each must not exceed $\frac{10}{456}$ of one millionth of one millionth of an inch. But I have showed that the greatest probable density of light on the sun's surface, does not amount to 173d part of the density of common water. Therefore the diameter of each spherule does not exceed $\frac{10}{456}$, or more accurately, $\frac{1000}{45648}$ of one millionth of one millionth of an inch; and is probably still less.* More than 95100 spherules of this size go to make up one of the size first assumed. Therefore, though the sun should lose as much matter as can be supposed to be contained in any single emission of light, 95100 times in every second, no sensible alterations in the system could take place in millions of years. And perhaps light does not issue from the sun so frequently as 95100 times in a second.

P. S. The late Dr. Pemberton, of Gresham College, to whom the foregoing paper was communicated, about 12 months before his death, in a letter with which he favoured the author, on the subject, remarked, that he had no material objection to any part of the reasoning, except it was, that the particles of light are too peremptorily spoken of as spherules. Their real figure is quite unknown; but the probability is, that they are not spherical, since Sir Isaac Newton found that their different sides have different properties. As the like objection may occur to others, it may not be improper to add a few words to obviate it.

* Instant. Product. p. 38—41.

I would by no means be understood to assert, that the particles of light are of a spherical figure. But, whatever their figure may be, I conceive that their size must be so small, that the diagonal of each little solid cannot exceed one millionth of one millionth of an inch, or, at least, that each is capable of being circumscribed within a sphere of that diameter. To the reasons which are given for making them so small, in the treatise so often referred to, I shall here add another, which may perhaps be more generally convincing: namely, that these bodies must be so minute as to find room to enter in, in swarms, at the pupils of the eyes of the smallest microscopic animals, and not to injure, by their stroke, the very delicate fibres of their optic nerves, nor to lacerate the edges of the uvea; which those that enter near the sides of the perforation, if their figure be not round, must often brush with their angles, as they pass by. Now, if it be granted, that the greatest diagonal of each solid corpuscle of light does not exceed one millionth of one millionth of an inch, or that each is capable of being circumscribed with a sphere of that diameter; then the solid content of a sphere of that diameter is the maximum of the solid content of each corpuscle, and the matter in such a sphere, of due density, is the maximum of the matter in each corpuscle. The maximum therefore of the force of motion in each particle, is the force of a spherule of the size assumed, moving with the known velocity of light; and therefore, be the figure what it will, my conclusion which rests entirely on the maximum of size and matter, will still hold good, unless it can be shown that I have underrated the density of each particle; and even if it could be proved that I have assumed the density too small in the proportion of 1 to 12 thousand times the square of one million, still the general conclusion will not be shaken: for this vast increase of the density will raise the force of motion, in each corpuscle, to no more than that of an iron ball of $\frac{1}{4}$ of an inch diameter, moving one inch in a whole year.

Again, in the preceding diagram, SB being the diameter of the sun, and *f*B, B*e* being each the half of one millionth of one millionth of an inch, the space contained between the spherical surfaces *fkl*, *egh*, is the maximum of the space that the particle of light with their due proportion of interstice can fill, as they start forth from the surface of the sun. For, whatever their figure be, it must be such a one as can be laid between these two spherical surfaces. Now the quantity of matter in this space must not be greater than it would on the hypothesis that each figure was spherical, and the number of spherical particles the greatest possible. Since on that hypothesis, the density of matter, crowded into this space, is vastly too great, to be consistent with the appearances of nature; and consequently a greater density would be inconsistent. Therefore the maximum of the matter, and consequently, if the density of each separate particle has been rightly assumed, the maximum of the solid content, in each

emission of light, is what I have made it; at least it does not exceed what I have made it, be the figure of the particles what it will; and my conclusions, which rest entirely on the maximum of the solid content, and that of the matter, and are the stronger the less these maxima be, will still hold good. But if these conclusions stand, the objections moved by Dr. Franklin vanish.

The only part of my reasoning which will be affected, by supposing the figure of the particles of light not to be spherical, will be that in which I attempt to show, in what proportion the magnitude of each particle, and the matter contained in each emission, must be less than the maximum, in order to make the density of light no greater than may be consistent with the appearances of nature. Here indeed the figure of the corpuscles is of great importance. The diminution necessary will be very different in different figures, and the figure, I confess, may be such, as to make it much less than what I have shown to be requisite on the spherical hypothesis. However, if $\frac{1}{10000}$ part of the density of our air be admitted to be as great a density as can reasonably be allowed to light, at the surface of the orbis magnus, the matter of each emission must not, on any hypothesis of the figure of the corpuscles, exceed $\frac{1}{2084}$ of the maximum.

For in order to bring the density down from the maximum, to any other given limit, either the matter must remain unaltered, and the space, which it is supposed to occupy, be increased, in the proportion in which the density is to be diminished; or the space must remain unaltered, and the matter be diminished in proportion to the density; or if both space and matter be altered, the matter must be changed in the proportion compounded of the two, in which both space and density are varied. Now the space which we suppose the light to occupy, as it is emerging from the surface of the sun, must not exceed the space contained between the spherical surfaces *egh*, *fkl*. For that space, as has been observed, is the maximum. This space may be diminished; but if it be diminished, the matter being diminished as the space and as the density jointly, must be more diminished than in the simple proportion of the density. Therefore the diminution of the matter will be the least possible, if, the space being supposed to continue at its maximum, the matter be diminished in the simple proportion of the maximum of the density to the density required. Various formations of the particles of light might be thought of, which would answer this purpose. It might be answered indeed, on the spherical hypothesis, by diminishing the number of spherules, retaining the maximum of their size; or retaining the maximum both of size and number, if it can be thought reasonable to diminish the density of each particle. But, on any hypothesis, the diminution of matter cannot be less than has been said. That is, the matter of each emission cannot possibly exceed $\frac{1}{2084}$ of the maximum. For the proportion of the maximum of the density to the density required, will be found by computation, to be that of 2084 to 1,

very nearly. Therefore the utmost probable amount of one emission is $\frac{1}{2084}$ of the maximum, be the figure of the corpuscles what it will. And the sun may lose the quantity of a whole emission 2084 times in a second without any sensible consequences.

I cannot apprehend, from any quarter, so unphilosophical an objection, as that the extreme minuteness of the particles of light, which I have shown to be necessary, if light be really matter, is an argument against their existence. Size is a mere accident, and no part of the essence of any being. Great and small, applied to finite things, are purely terms of comparison. One Being only is absolutely Great: he whose substance pervades and fills the whole and every part of absolute space; because, in respect of him, all things that are are little.

Notwithstanding the maximum of moving force, in each particle of light, is so inconsiderable as I have shown it to be, yet the number of particles, out of each emission, which are directed towards the earth, and fall upon its enlightened hemisphere, being exceedingly great, it may perhaps be imagined, that the force impressed by them all upon the earth, if they all actually strike its surface, may amount to something worth attending to. I have taken the pains to satisfy myself on this question, and shall briefly mention the result of my computations.

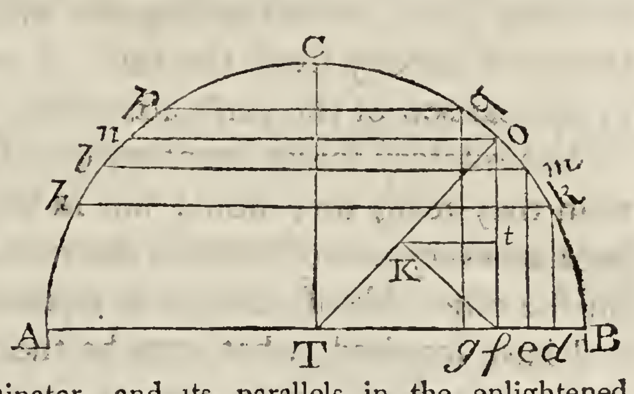
Reckoning every emission at its maximum, I find that the number of the particles out of each, which should fall on the earth's enlightened hemisphere, is 492023xxxi, or that of which the logarithm is 36.6919854. The moving forces of this whole number, amount to as much as the force of an iron ball, of one yard diameter, flying 68 miles and 887 yards in one second. But the progressive force, which they might communicate to the earth, does not exceed that of an iron ball of the same size, flying 34 miles and 443 yards in one second.* And, if this whole force be transferred from the iron ball to the globe of the earth,

* Only one particle out of each emission from any very small given part of the sun's surface, can strike the earth's surface perpendicularly. The force of every particle, which impinges obliquely, is resolvable into two, one perpendicular to the surface, and the other parallel to it. That part of the force, which is perpendicular to the earth's surface, produces a progressive motion of the whole globe. The other, which is parallel to the surface, contributes nothing to the progressive motion, but tends to produce a rotation of the globe on an axis. Hence the progressive force of motion, communicated to the globe, is less than it would be, if all the particles struck the surface perpendicularly. It is further lessened on another account, namely, that the whole of the perpendicular force is not effective in moving the earth's centre. I find by computation, that the diminution on the whole is $\frac{1}{2}$. For the ease of the mathematical reader, I shall briefly state the principles on which this computation of the force with which the earth may be struck by light, has been framed.

The number of particles, which are directed to the earth out of each emission, is to half the number of the whole emission, in the duplicate proportion of the chord of the sun's horizontal parallax, to the chord of 90°, by the known doctrine of Archimedes. Hence the number of the whole emission being determined, the number of those which tend towards the earth, is given. And the force of each single particle being given, the sum of the forces of that given number is given; and this is

the progressive velocity of the earth's centre will be found to be no more, than that with which a body would traverse about $\frac{1}{7}$ of one millionth of one millionth

the progressive force which would be impressed on the earth, if all the force of each particle were effective. In what proportion the progressive force is diminished, on account of the various obliquities of impulse, is thus investigated.



Imagine T to be the earth's centre, and ACB to be half a great circle of the earth, perpendicular to that which separates the enlightened and the dark hemisphere, and which shall be called the terminator. On the plane of this semicircle, suppose the terminator, and its parallels in the enlightened hemisphere, to be projected, into right lines AB, *kh*, *lm*, *no*, &c. which are diameters of the circles respectively, of which they are the projections. The sun's distance may be considered as infinite; and therefore the rays of light, i. e. the directions of the particles, when they reach the earth's surface, are to be considered as parallel to each other, and all of them perpendicular to the plane of the terminator. Now imagine the whole enlightened hemisphere to be divided into innumerable little zones AB *kh*, *hkml*, *mlno*, &c. by small circles parallel to the terminator. Let the breadths of these little zones, measured on a great circle passing through the poles of the terminator, that is, let the infinitesimal arcs B*k*, *km*, *no*, &c. be so proportioned to each other, that perpendiculars *kd*, *me*, *of*, &c. being drawn from the extremities of these arcs, to the right line AB, which is the common intersection of the great circle ACB, and the plane of the terminator, the infinitesimal segments of that line B*d*, *dc*, *ef*, &c. may be equal. Now imagine the particles of light which fall upon any one of these little zones, for instance, *noqp*, to meet with no resistance from the earth's surface, but to penetrate the globe, and to pass on without refraction or inflection, in the direction perpendicular to the terminator, till they arrive at the plane of the terminator, and there suppose them to stop, and each to lie still, in the place on which it falls. It is evident that the particles of light that fall upon, and have been supposed to pass through, the spherical zone *pqon*, will, with their proper interstices, cover that annular space on the plane of the terminator, which is the orthographical projection of the zone *pqon*, on that plane, and is comprised between the circumferences of circles, of which the right lines T*g* and T*f* are the radii. Hence the number of the particles of light, which fall upon the evanescent zone *pqon*, are as that evanescent annular space which they cover, that is, as $gf \times$ the circumference of the circle of which T*f* is the radius, that is, as $gf \times$ in the right line T*f*. But that part of the force of each particle, impinging on the zone *pqon*, which is perpendicular to the surface of the zone, is as *of*, if TB (the semidiameter of the earth) be put for the whole force. For join T*o*, and draw *f*K perpendicular to T*o*. The particle impinging at *o* moves in the direction *of*. Let the right line *of* then express its whole force, and this force *of* is composed of the two *o*K, K*f*, of which *o*K is perpendicular to the surface of the sphere at *o*, and K*f* is parallel to it. But *o*K : *of*$=$*of* : *o*T or TB. Again, through K, draw K*t* perpendicular to *of*. The force *o*K is resolved into two *ot*, *t*K, of which *ot* is perpendicular to the plane of the terminator, and is the only part of the force *o*K, which tends to produce a progressive motion of the globe, in the direction of the impinging particles, that is, directly from the sun. The other part *t*K urges the centre of the globe along the plane of the terminator; but the forces *t*K being equal and contrary on opposite sides of, and at equal distances from the perpendicular ray, destroy each other's effects. Now it has been shown, that the whole force of the particle impinging at *o*, is to that part of its force which is perpendicular to the earth's surface at *o*, as TB to *of*. And it is manifest that *o*K is to *ot*, that is, the perpendicular force, of the particle impinging at *o*, is to that part of it which is effective in moving the earth's centre, as

of an inch in 100^s. This is the utmost effect of the force impressed on the earth by each emission. If the emissions were incessant, this might be considered as a central force, counteracting the sun's attraction; for its tendency is to push the earth directly from the sun. I need not say, that it is infinitely too small, in comparison of the sun's attraction, to produce any sensible effect.

The rotatory forces mentioned in the last note, if they were infinitely greater than they really are, would not in the least degree disturb the diurnal rotation; because every one of them is destroyed, by an equal one, in a contrary direction, on the other side of, and at an equal distance from, the perpendicular ray.

I have inquired, what may be the utmost stroke, which the retina of a common eye sustains, when the eye, in a bright day, is turned up directly to the sun. This force will evidently be at its maximum, if the emission be reckoned at its maximum. The number of particles which enter an eye, looking up directly at the sun, are to the number out of each emission which are directed towards the earth, in the duplicate proportion of the diameter of the pupil to the diameter of the earth. And the force with which the eye is struck, is to the sum of the forces of all the particles which strike the earth, in the same proportion. If therefore the diameter of the pupil, when the eye is exposed to the direct impulse of the sun's rays, be reckoned $\frac{1}{10}$ of an inch, which I apprehend must rather exceed than fall short of its real magnitude, in those circumstances, it will be found that every stroke which it receives from them, exceeds not that which an iron shot, $\frac{1}{4}$ of an inch diameter, would give, moving only at the rate of 16.16

TB to *of*. Therefore the whole force of the particle impinging at *o*, is to its effective part, as TB^2 to of^2. That is, the effective part is as of^2. The number of particles impinging on the zone has been shown to be as $gf \times Tf$. The progressive force of motion excited in the earth's centre, by all the particles impinging on the infinitesimal zone *pqon*, must be as the number of the particles and the effective part of each jointly; that is, as $gf \times Tf \times of^2$, or writing a for TB, and x for B*f*, as $\dot{x} \times (a - x) \times (2ax - x^2)$. And this is the fluxion of the progressive force of motion excited in the globe, by the particles impinging on that segment of the sphere, of which *p*AB*q* is the projection. The number of particles impinging on the zone *pqon*, being as $gf \times Tf$, or as $\dot{x} \times (a - x)$, if each impinged perpendicularly, and its whole force were effective, the sum of the effective forces impressed on the whole, would be as $gf \times Tf \times TB^2$, or $\dot{x} \times (a^3 - a^2x)$. And this would be the fluxion of the progressive force of motion of the globe, excited by the particles impinging on the segment of which *p*AB*q* is the projection, if all impinged in directions perpendicular to the surface, and the whole of their forces were effective. The fluent of $\dot{x} \times (a - x) \times (2ax - x^2)$ is $\frac{1}{4} \times (2ax - x^2)$. And the fluent of $\dot{x} \times (a^3 - a^2x)$ is $a^3x - \frac{1}{2}a^2x^2$. When $x = a$, the first of these two fluents is the sum of the progressive forces actually impressed on the whole hemisphere ACB, and the latter is the sum of the forces which would be so impressed, if all the impinging particles impinged perpendicularly, and the whole force of each were effective. But when $x = a$ the first fluent becomes $\frac{1}{4}a^4$. And the latter becomes $\frac{1}{2}a^4$. Whence it is manifest, that the progressive motion communicated to the globe of the earth, by the particles of light, is to the force which they would communicate, if the whole force of each were effective, in the proportion before assigned, of 1 to 2.—Orig.

inches in a year. This would be the stroke if the emission were at its maximum. Is it not owing to the extreme minuteness of the fibres of the nerves, that a stroke, which is certainly less than the $\frac{1}{2084}$ part of this, is not sustained by our organs, without pain?

XXXVI. Some New Theorems for Computing the Areas of certain Curve Lines. By Mr. John Landen, F.R.S. p. 441.

The learned editor of Mr. Cotes's Harmonia Mensurarum first gave us, in that book, the celebrated theorems for computing the areas of the curves whose ordinates are expressed by $\frac{x^p}{a^n + x^n}$, $\frac{x^p}{(a^n + x^n) \times (e^n + x^n)}$, or $\frac{x^p}{a^{2n} + 2ca^nx^n + x^{2n}}$; and several other writers have since done the like. Which theorems consist of many terms, being obtained by previously resolving the expression for the ordinate, into others of a more simple form. Now I have found, says Mr. L., that the whole area of every such curve, when finite, may be assigned by theorems remarkably concise, without the trouble of resolving the expression for the ordinate as aforesaid; and as in the resolution of problems, the whole area of a curve is more commonly wanted than a part of it; and as these new theorems enable us to compute such whole areas as above mentioned, or the whole fluents of $\frac{x^p\dot{x}}{a^n + x^n}$, $\frac{x^p\dot{x}}{(a^n + x^n) \times (e^n + x^n)}$, and $\frac{x^p\dot{x}}{a^{2n} + 2ca^nx^n + x^{2n}}$, with admirable facility; I do myself the honour of communicating them to the Royal Society, presuming they may be thought worthy to be published in the Phil. Trans.

Theorem 1. m being any positive integer or fraction, and n any such integer or fraction, greater than m; the whole area of the curve, whose abscissa is x, and ordinate $\frac{x^{m-1}}{a^n + x^n}$, is $= \frac{a^{m-n}}{fn} \times$ A.

Theorem 2. m and n being as before mentioned, the whole area of the curve, whose abscissa is x, and ordinate $\frac{x^{n \pm m-1}}{(a^n + x^n) \times (e^n + x^n)}$ is $= \pm \frac{a^{\pm m} - e^{\pm m}}{a^n - e^n} \times \frac{\text{A}}{fn}$.

Note, when e is $= a$, the expression for the area becomes $= -\frac{ma^{\pm m-n}}{fn^2} \times$ A.

Theorem 3. m and n being as in the preceding theorems, the whole area of the curve, whose abscissa is x, and ordinate $\frac{x^{n \pm m-1}}{a^{2n} + 2ca^nx^n + x^{2n}}$ is $= \frac{ga^{\pm m-n}}{bfn} \times$ A.

Note. If m be $= 0$, the area will be $= \frac{a^{-n}\text{B}}{bn}$.

In these theorems,

A denotes the semi-periphery of the circle, whose radius is 1;

B an arc of the same circle, whose cosine is c and sine b;

f the sine of the arc $\frac{m}{n} \times$ A; g the sine of the arc $\frac{m}{n} \times$ B.

Concerning the investigation of these theorems, it is sufficient to say, they are

directly obtained by the help of my new method of comparing curvilineal areas, inserted in the Phil. Trans. for the year 1768.

It is obvious, that, by means of the above theorems, we may very readily compute the whole areas, when finite, of the curves, whose ordinates are $\frac{x^p}{p + qx^n + rx^{2n} + x^{3n}}$, and $\frac{x^p}{p + qx^n + rx^{2n} + sx^{3n} + x^{4n}}$, &c. seeing these expressions may be easily transformed into others similar to those already considered.

XXXVII. Transit of Venus observed in India. By Capt. Alexander Rose, of the 52d Regiment. Communicated by Dr. Murdoch, F.R.S. p. 444.

Having procured a telescope and stop-watch, Capt. R. made observations on the transit of Venus, which happened on the 4th of June 1769.

Phesabad, lat. 25° 30′ north.

Observed the planet a good way advanced on the sun's body at	5^h	35^m	57^s	(apparent time)
First contact at the egress	6	52	25	
Last contact	7	10	47	
* Time between the first and last contacts	0	18	22	

XXXVIII. Of a Periodical Fever, followed by a Separation of the Cuticle. By Mr. John Latham, Surgeon at Dartford, Kent. p. 451.

Mr. A. B., about 55 years of age, was a healthy man till about 20 years since, when he was first seized with a fever; at which time he followed the trade of a miller, and maker of French barley. This last business, he says, is attended with very great heat to the operator, and exposes him to a continual cloud of dust. As soon as he began to work, his breath became oppressed with a sensation of his body being puffed up all over; from which symptoms he was relieved by occasionally leaving off his business. On the first cold caught after his entering on this kind of employment, a fever attacked him; which has generally returned sometimes once, and sometimes twice in a year, chiefly in autumn; but sometimes in spring likewise: though he once missed being ill for 2 years to-

* Whence the planet's centre was on the sun's limb at 7^h 1^m 36^s; and this compared with an observation of the central egress or ingress, made at a distant place, will give the sun's parallax; the other necessary elements of the calculus being well established. In the mean time we see, from the Connoisance des Tems for 1769, that Phesabad in Bengal, where Captain Rose observed, is 81° 45′ east of Paris.

The watch had been regulated the preceding day, by equal altitudes of the sun; the sun's altitudes, at the two contacts, are likewise marked in the captain's letter; but this part of the work he had probably entrusted to a less skilful observer, while his own attention was engrossed by the telescope and the watch; as I find the difference of the times correspondent, to those altitudes, does not agree with the interval of the contacts; for which reason they are here omitted.—P. MURDOCH. —Orig.

gether. After carrying on this trade for 4 or 5 years, he left it off; as he attributed his disorder chiefly to the effects of the meal dust. The fevers have not been so violent since, as while he followed that occupation: though the cuticle, or outer skin, has come off, the same as before. As to the particulars of his illness, they are nearly as follow: the disorder begins with a violent fever, attended with pains in the head, back, and limbs, accompanied with continual retchings; he sometimes vomited up much bile, at other times little or none; the skin was dry, the tongue much furred, together with great thirst, costiveness, and the urine highly coloured. At the beginning of the fever he was generally let blood; this evacuation afforded some relief, and by keeping his body open, and taking cooling medicines, the retchings abated in about 5 or 6 days: the whole surface of the body became yellow, though this circumstance did not always happen. Afterwards it became florid, having the appearance of a rash; on which he felt a great uneasiness for several days, with a numbness and tingling all over him; when the urine became turned, and deposited a thick sediment. About the beginning of the 3d week from the first attack, the cuticle appeared elevated in many places. In 8 or 9 days afterwards it became so loose as to admit of being easily removed in large flakes. The cuticle of the hands from the wrist to the fingers' ends came off whole, bearing the resemblance of a glove. He never was disposed to sweat in any part of his illness, and when sweating was attempted by medicines he grew worse for it; nor was he much at ease till his urine deposited a sediment, after which he felt very little inconvenience, but from the rigidity of the skin. The nails of the patient, in a case communicated to the R. S., are mentioned to have come off after the illness; Mr. L. did not find that this was ever the case in this person.

Of a Very Small Fœtus. By Mr. Joseph Warner. p. 453.

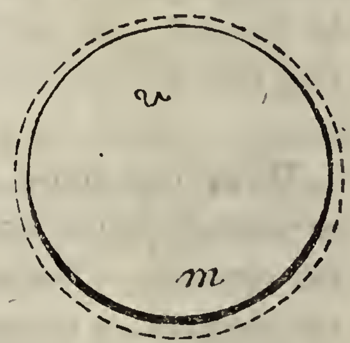
With the above cuticular glove was sent to the R. S. by Mr. Warner, a very small fœtus, brought into the world at the same time with a live child at its full growth. The woman was delivered before he came to her: on examining the placenta, a substance appeared somewhat unusual; and on washing it clean, he discovered the fœtus above mentioned. It had no visible communication with the placenta, but was squeezed flat, though not in the least putrid, and seemed shrivelled. He did not remember a case like this mentioned, except in Smellie's Midwifery, vol. 2, p. 85, where he relates one from the Academy of Sciences at Paris, nearly similar to this. May we not suppose the woman to have been with child of twins; and that this dying was not discharged, as was most likely to happen, but remained till the time of the natural birth, when they were both expelled together?

XXXIX. On the Transit of Venus, and other Astronomical Observations made at Cavan, near Strabane, in the County of Donegal, Ireland, by Appointment of the Royal Society. By Mr. Charles Mason. p. 454.

The first of these is a series of observed equal altitudes of the sun and stars, from April 3 till October 23, 1769, to regulate the clock. 2d. A like series of apparent zenith distances of the sun, moon, and stars. 3d. A like series of meridional zenith distances, for the latitude of the place, the medium of which is 54° 51′ 40″.8. 4th. Another series of the difference of right ascension between the moon's limb and stars. 5th. Eclipses of Jupiter's satellites, also occultations of fixed stars by the moon, and other phenomena, for the longitude. 6th. The sun, moon, and stars passing the meridian, by a transit instrument. 7th. Also the observations of the transit of Venus, June 3, 1769, and of the sun's eclipse the next day. The times of the transit by the clock were as below:

At 11^h 17^m 53^s the external contact of Venus and the sun.
11 35 30 internal ditto, judging by their peripheries.
11 36 8 ditto, when the thread of light broke out.
4 35 37.8 the sun passed the meridian.

Though the air at external contact was not quite so clear as sometimes seen, yet the sun's limb appeared well defined, and the spots in the disk very strong, their edges keen and distinct. At the internal contact, the air was much changed, and the limb of Venus seemed to cohere to the sun's limb, by a protuberance that appeared like a dark shade: which seemed to prevent seeing the thread of light for about 40^s longer than expected.



When the planet was upon the sun's disk, there appeared a faint light shade (having a gentle fluctuating motion) round its periphery, and widest on that part farthest on the sun's disk: it appeared as per fig., the black circle representing the periphery of Venus, and the dotted one that of the shade, which was very regular and well defined; *v* the upper, and *m* the lower part of the planet: and the whole shade was apparently of equal brightness.

At 22^h 15^m 0^s Cloudy with rain.
22 40 0 Cloudy.
22 49 28 The eclipse of the sun began.
22 49 35 Very plain.
22 50 0 Cloudy with rain.
23 28 0 The clouds began to break; and from this time to 23^h 54^m Mr. M. endeavoured with a micrometer (of Mr. Dollond's construction) to get measurements for determining the digits eclipsed; but was so interrupted by flying clouds, that nothing could be done with certainty; then cloudy with rain till the end of the eclipse was past.

XL. The Transit of Venus and other Astronomical Observations, made in the West Indies. By M. Pingré, of the Royal Acad. of Sciences at Paris. p. 497.

At Cape Francis in the island of St. Domingo.

June 3, first perceived Venus entering on the sun's disk, apparent time, as below:

At 2^h 26^m $14^s\frac{1}{2}$ With Dollond's $2\frac{1}{2}$ feet telescope. M. de Fleurieu.
2 26 $16\frac{1}{2}$ With a 3-foot achromatic of l'Estang. M. la Filiere.
2 26 $20\frac{1}{2}$ With a common telescope of 2 feet, only 2 lenses. M. des Saqui Tourés.
2 26 $12\frac{1}{2}$ With a 5-feet achromatic. M. Pingré.

After having given their eyes some respite, they returned to the telescopes; and M. de Fleurieu perceived a luminous little circle all round Venus, not yet entered more than about one third of her diameter. This luminous thread made, to all appearance, a perfect circle with the part of the circumference of Venus already advanced on the solar disk. Mr. P. likewise observed the same phenomenon, but a good while after M. de Fleurieu.

Venus appeared totally entered at 2^h 44^m 45^s By M. de Fleurieu.
44 41 By le Chev. de la Filiere.
42 50 By M. Saqui des Tourés.
44 44 By Mr. Pingré.

During both these observations, every thing was quiet and still, not a word uttered, to intimate that any one had observed the contact. Stormy weather almost every night hindered them from observing the eclipses of the satellites. However, the 10th of June proving a clear night, afforded an opportunity of determining the latitude of the observatory; which by meridian altitudes of several stars, both to the north and south, Mr. P. determined to be 19° 47′ 3″. The New church of the Cape, situated nearly in the middle of the town, may be about 20″ or 25″ more southward than their observatory, whence its latitude 19° 46′ 40″ N.

As to the longitude, they had no other way but to take with a quadrant some altitudes of the moon's lower limb:

Mr. P.'s were these:

Alt.	Times by clock.	Apparent times.
37°	9^h 12^m 53^s	9^h 8^m $30^s.9$
36	17 25	13 2.7
35	21 55	17 32.5
34	26 21	21 58.3
33	30 48.5	26 25.9

These altitudes taken with a quadrant of 2 French feet radius, 1′ 6″ must be added to each, to correct the error of the quadrant.

M. de Fleurieu's were :

Alt.	Times by clock.	Apparent times
34° 45′	9^h 22^m $28^s.5$	9^h 18^m $05^s.8$
34 15	24 41.5	20 18.7
33 45	26 55	22 33.2
32 45	31 23.5	27 0.6
32 15	33 36.5	29 13.5
31 45	36 51.5	31 28.5

These altitudes were taken with an English quadrant of M. Sisson's make, 16 inches radius. 8′ 34″ are to be added to each altitude to correctthe error of the instrument, and for the semidiameter of the wire.

On computing these altitudes by M. Clairaut's tables, corrected nearly by observations made at Paris the 30th of May and the 1st of June 1751, Mr. P. finds the longitude of Cape François, west of the meridian of Paris, by his own altitudes, 4^h 58^m 8^s, and by those of M. de Fleurieu 4^h 58^m 20^s.

The time above noted for the total entry of Venus, is that when they perceived a very slender thread of light between the limbs of the sun and Venus. They judged that the limbs were in contact, but a few seconds before that instant. At the exit of Venus in 1761, the limbs, being not yet in contact, and even sensibly distant asunder, Mr. P. saw as it were a dark spot detach itself from Venus, and gain the limb of the sun; at which instant he estimated the internal contact. Many have this year seen the same phenomenon at the total entry of Venus. Mr. P. was in expectation of it; but neither he nor his associates perceived any such thing. In 1761 the sun's limbs were most exquisitely well defined; in 1769 they undulated, especially at the beginning of the entry; at the total entry the undulation was considerably less; and notwithstanding this undulation he believes their observation a good one. On comparing the duration of the transit observed at the Prince of Wales's Fort, with that of Father Hell, at Wardhus, he finds on a first calculus, which he believes at least nearly exact, that the sun's parallax is 9″.11.

Aug. 16, at St. Croix in Teneriffe, the first satellite emerged at 9^h 16^m 5^s, apparent time.

XLI. Observations of Immersions and Emersions of Jupiter's first Satellite, made at Funchal, in Madeira, with a Reflecting Telescope of 18 *Inches Focus, made by Mr. Short. By the late Thomas Heberden, M.D., F.R.S.* p. 502.

The time was found by taking equal altitudes, with a quadrant of 12 inches radius, made by Mr. Bird, and with the help of a good pendulum clock made in London. And the latitude of the place of observation in Funchal, by a mean of several observations made with the same quadrant = 32° 33′ 35″. By comparing these observations with similar ones made at Greenwich, the mean among the the whole gives 1^h 6^m $55^s.6$ for the longitude of the place.

XLII. Account of the Transit of Mercury, observed at Norriton, in Pennsylvania, Nov. 9, 1769, *agreeable to an Appointment of the American Philoso-*

phical Society, held at Philadelphia, for promoting useful Knowledge. By Wm. Smith, D.D., John Lukens, Esq., David Rittenhouse, M. A., and Mr. Owen Biddle. Communicated by Ben. Franklin, LL.D., F.R.S. p. 504.

These gentlemen had the same telescopes now as before, in the transit of Venus, viz. the college reflector, with Dollond's micrometer; used by Dr. Smith, with a magnifying power of 200, to observe the contacts. 2. A refractor of 42 feet, magnifying 140 times, used by Mr. Lukens. 3. Mr. Rittenhouse's refractor, with about the same power, used by himself. Mr. Biddle had no telescope; but was very serviceable in the other parts of the observation.

The first external contact was observed to the same instant by all the three observers, who had no communication with each other, the two refractors being out of doors, and the reflector within the observatory; and the contacts noted, as at the transit of Venus, by signals given to persons set at the windows of the observatory, to count the clock.

The contacts were as follow: 1769, Nov. 9, apparent time,

At 2^h 35^m 17^s first external contact, by all the three observers.
2 36 35 first internal contact, by Dr. Smith and Mr. Rittenhouse.
2 36 33 first internal contact, by Mr. Lukens.

The sun's diameter, per micrometer . . . . 35′ 20″.24
Mercury's diameter taken backwards and forwards several times, and the sum halved, gave only 0 8.22

By the contacts of Mercury at Philadelphia and Norriton, they get the latter 55^s of time west of the state-house observatory; the same they made by the eclipses of Jupiter's satellites.

XLIII. Investigations of Twenty Cases of Compound Interest. By J. Robertson, Lib. R. S. p. 508.

The late Wm. Jones, Esq. F.R.S., among the variety of mathematical matters to which he gave attention, considered the business of compound interest fully, and did, many years ago, cause to be engraved on a copper-plate, more cases in interest than had been exhibited before that time: several copies of impressions from that plate were distributed among his friends; to whom it appeared that he had treated this subject in a more extensive manner than had been done by other mathematicians.

The theorems, or rules, for the cases of compound interest, without their investigations, were inserted by Mr. Jones, in the quarto edition of logarithms, published by Gardiner; and the rules were also communicated to Mr. Dodson, who published them, by Mr. Jones's leave, with examples to illustrate the use of his antilogarithmic tables: but the investigations of these theorems not having

yet been made public, it is apprehended that gentlemen curious in these speculations would be pleased to see them.

In this subject 5 particulars are taken into consideration. 1st. The annuity, rent, or pension; 2d. The times that annuity, rent, or pension is to continue; 3d. The rate of interest used in the computation; 4th. The amount of those rents, and their interest, when they are forborn to be received any times after they are due: 5th. The present worth of those rents, some times before they are due; or of a sum to be received before it is due, discount being allowed.

And the investigations naturally fall under two heads. 1st. The consideration of amounts. 2dly. The consideration of discounts. Under the first head an equation is to be obtained between the annuity, time, rate and amount, from the known proportion that subsists between sums of money put to interest, during the same length of time, and the amounts of the principal and interest together. Under the 2d head another equation is to be formed between the annuity, rate, time and present worth, from the known proportion that subsists between the sums discounted, and their present worths, when done for the same time. As these equations involve quantities common to both of them, therefore other equations may be thence deduced, containing all the 5 terms before specified. And hence, any 3 of the 5 terms being given, the other 2 are to be found; which admits of 20 cases.

Some of these cases will produce affected equations, where the index of the highest power of the unknown quantity will be the number of times the rent is to continue, or to be paid: therefore the solution of those cases will be given by a method of approximation, as no better way has yet been discovered for the solution of affected equations, in numbers, above the 3d or 4th degree.

The algebraical investigations are then given; but it is unnecessary that they should be here retained.

XLIV. A Copy of a Letter from John Ellis, Esq., F.R.S. to Dr. Linnæus, F.R.S., &c. with the Figure and Characters of that elegant American Evergreen Tree, called by the Gardeners the Loblolly-Bay, taken from Blossoms blown near London, and showing that it is not an Hibiscus, as Mr. Miller calls it; nor an Hypericum, as Dr. Linnæus supposes it; but a new Genus, to which Mr. Ellis gives the Name of Gordonia. p. 518.

We have lately got into a method of cultivating the loblolly-bay, or alcea floridana, &c. of Catesby's History of Carolina, vol. i. tab. 44, p. 44. This tree has lately produced some well-blown flowers, in the botanic garden of Mr. Bewick, at Clapham near London, who sent them to me to examine their characters while fresh: I had by me some dried specimens, which had been sent from our friend Dr. Alex. Garden, of Charlestown. On comparing these with the

fresh specimens, it soon became evident, why you judged it to be of the class of polyadelphia, and placed it among the hypericums with the trivial name of lasianthus. For the stamina in the dried specimen appeared to be divided into 5 distinct phalanges, or bundles, with their filaments united together; but when you observe the figure and description of the new-blown flower, you will find that the filaments of the stamina being united at the bottom, in a circle round the top of a funnel-shaped tube, will bring it to the class of monadelphia, and probably next to the stewartia. The only doubt I have in the description is, whether the style should be called 1 or 5, the latter of which numbers you have adopted, and perhaps more properly, but in that I shall submit to your decision.

If, after you have seen it, you think, with many of your friends, both here and in America, that it is a new genus, I desire it may have a place among your genera, by the name Gordonia, as a compliment to our friend, Mr. James Gordon, near Mile-end, to whom the science of botany is highly indebted, and whose merit is universally known for his great knowledge in the cultivation of exotic plants.

Mr. Miller, after telling us in his Gardener's Dictionary of the difficulty, or rather impossibility, to raise it, has placed it under the genus of hibiscus; but as both the characters of that genus, in which he has followed you, as well as the face and habit of this plant, differ so much from an hibiscus, I am convinced you will agree with me, that it does not belong to it.

Explanation of the Gordonia Lasianthus, pl. 2.

A is the flower, unopened, with its calyx and bracteal leaves; from the curious garden of Benjamin Bewick, Esq. at Clapham. B the petals dropt off; these are united at their base; see B. I. C the petals, or corolla expanded, to show the fleshy funnel-shaped tube, which unites the filaments together at their base. D the pistillum, whose germen, or seed-bud, has been surrounded by the base of the corolla at B I. E the calyx, or flower-cup, consisting of 5 little stiff leaves. FFFF the 4 bracteal leaves. G the short style, and 5 stigmata. G I the stigmata magnified. H the conical germen, or seed bud, surrounded by the calyx. H I the seed vessel before it is ripe, with the calyx reflexed and withered. I the pericarpium, or seed-vessel, with its valves open. KK two winged seeds. L three of the petals cut off, to show how they are united to the fleshy funnel-shaped tube, that supports the stamina. MMM the stamina with their filaments and summits a little magnified. N the bracteal leaves surrounding the flower-bud unopened.

XLV. The Copy of a Letter from John Ellis, Esq., F. R. S., to Mr. Wm. Aiton, Botanic Gardener at Kew, on a New Species of Illicium Linnæi, or Starry Aniseed Tree, lately discovered in West Florida. p. 524.

After a few preliminary observations Mr. Ellis proceeds thus:

I shall now give you a history of this curious tree, both as a native of Japan, China, and other parts of the east, as well as both the Floridas in North America. We meet with an account of the eastern one, with a figure of it, taken from Clusius, in Parkinson's Theatre of Plants, p. 1569; where he observes,

that some branches of it, with the husks and seeds only, without leaves or blossoms, were brought to England by Sir Thomas Cavendish, in Queen Elizabeth's time, from the Philippine islands, where he met with it in his voyage round the world. These branches were given to Mr. Morgan, the queen's apothecary, and to Mr. James Garrat, of whom Clusius received them.

M. Geoffroy, in his Materia Medica, translated in 1736 by Dr. G. Douglass, p. 322, calls it anisum sinense, semen badian, and fructus stellatus, and says it is highly esteemed in China, and all over the east. That it is used to cure any bad taste in the mouth, as a preservative against the effects of bad air, and also for the stone and gravel. The Indians likewise steep this fruit in water, and afterwards ferment the infusion, and thus make a vinous liquor: that the Dutch in the East Indies, as well as the natives, mix this fruit with their tea and sherbet.

Kæmpfer in his Amœnitates Exoticæ, p. 880, calls it somo, or skimmi, and has given a very good figure of a branch of it, with the leaves, flowers, and fruit. He found it in Japan, and says that the Japonese and Chinese esteem it a sacred tree, that they offer it to their idols, and burn the bark of it, as a perfume, on their altars; and lay the branches on the graves of the dead, as an offering to the ghosts of their pious departed friends; and that the public watchmen use the powder of this aromatic bark strewed in small winding groves, or little channels, on some ashes in a box secured from the weather, for the following purpose: this powder being lighted at one end, burns slowly on, and being come to certain marked distances, they strike a bell, and by means of this time-keeper, proclaim the hours of the night to the public. And lastly, that it has the remarkable property of rendering the poison of the bladder-fish (tetraodon ocellatus of Linn. Systema Naturæ, p. 333) more virulent, as many have experienced, that have used violent means to destroy themselves.

We are indebted for the first discovery of this curious American tree to a negro servant of Wm. Clifton, Esq. of West Florida, who was sent to collect specimens of all the rarer plants by his master, at my request; and in April 1765, he met with it growing in a swamp near the town of Pensacola; the specimens I received in July following.

After this, in the latter end of January, 1766, Mr. John Bartram, the King's botanist for the Floridas, discovered it on the banks of the river St. John, in East Florida, as appears from his description of it, and the drawing of a seed-vessel, with some of the leaves, which he sent to our late worthy member Peter Collinson, Esq. Mr. Bartram's description of it, as it appears in his journal up the river St. John's, published by Dr. Stork, in his account of East Florida, is as follows: "Near here my son found a lovely sweet tree, with leaves like the sweet bay, which smelled like sassafras, and produces a very strange kind of seed-pop; but all the seed was shed, the severe frost had not

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hurt it, some of them grew near 20 feet high, a charming bright evergreen aromatic." This observation of Mr. Bartram, relating to its bearing a severe frost, may afford us a useful hint in the cultivation of this tree, especially as I am convinced, from repeated accounts of the weather in West Florida, that the frost is much more intense there, whence those plants, which you now have in vigour, were brought, than in East Florida; so that the experiment is well worth making with one of them, to see how far it will stand the severity of our winters. Should it succeed, it would be a very great acquisition to our gardeners, and be highly ornamental to our plantations of evergreens.

The medicinal properties of this tree are certainly worth inquiring into. The leaves afford a most agreeable bitter. A sprig of it set to putrify in a phial of water, the bark soon became full of a clear mucilage. The young blossoms put into water, with a small quantity of oil of tartar per deliquium, from a dark reddish colour, became a light brown; but from the same proportion of oil of vitriol in water, they turned to a fine carmine colour, which stained the paper of a fine red. This points out its astringent quality.

Before I come to the botanical characters of our Florida illicium, I must observe, that it appears to me to be a different species from the oriental one. The seed vessels from China, which are to be seen in collections of the Materia Medica, especially among foreigners, smell very disagreeably of aniseed: our Florida seed vessel is agreeably aromatic, as are the leaves and young branches. The flower, according to Kæmpfer, is of a yellowish white, and looks at a distance like a narcissus: ours is of a dark red colour.

Kæmpfer reckons the number of petals 16, and the rays or seed vessels 8: the number of petals in ours is from 21 to 27, and the seed vessels 12 or 13 that ripen. In respect to the form and growth of the tree, they are much the same; for instance, they both grow to the size of a cherry tree; their leaves are of an oblong oval shape, pointed at both ends, fleshy, with few veins, growing alternately, and in tufts at the ends of the small branches. Dr. Linnæus, who takes his characters of the illicium anisatum (Gen. Plant. p. 244) from Kæmpfer, places it among the dodecandria polygnia. But I am persuaded you will agree with me, that from its characters ours must be of the polyandria polygnia, and should stand next to the magnolia.

Explanation of the Illicium Floridanum, in pl. 2 —A is a branch, drawn from a plant in her Royal Highness the Princess Dowager of Wales's garden at Kew. The flowers and seed vessels were drawn from specimens sent over from Pensacola by the Lieutenant Governor Durnford. BB The front view of two flowers. C The back view of a flower. D The bud of a flower unopened. EEE The pistilla, or female organs, being the embryo seed vessel, separated from the staminas, or male organs. F One single pistillum, with the germen, style, and stigma. G The male and female organs, a little magnified. H Two stamina, a little magnified. I The farina fœcundans or male dust. KK The calyx, with 5 little leaves. LL The seed vessels, with 13 capsules.

L l The seed vessel of the Chinese illicium, with only 8 capsules. Kæmpfer reckons the same number in the Japonese illicium, which he calls somo, or skimmi. M Two of the seeds; they are called semen badian, and used in medicine in Germany, Denmark, and Sweden. The Dutch import large quantities of them from China.

XLVI. Of a Very Remarkable Meteor seen at Oxford. By the Rev. John Swinton, B. D., F. R. S. p. 532.

The person who first saw the very remarkable luminous appearances in the air here, on Tuesday, Oct. 24, 1769, was the Rev. Mr. Cleaver, student of Christ-church; who, on his return home, at a village called Horton, 6 or 7 miles from Oxford, about $7^h\ 15^m$ P. M. observed, with some degree of astonishment, a dark fuscous vapour, resembling a blackish cloud, contiguous to the northern horizon: Out of this vapour there issued another of a flame colour, in the N. N. W. His account of it was, that "it looked like a house, or building, set on fire." This at first was confined to a very small space in the heavens, but soon after expanded itself in such a manner, that it covered a very large and extensive tract in that part of the hemisphere where it first appeared. In this state the meteor continued till $7^h\ 45^m$ P. M. when it assumed a deep blood-red colour, moving a little towards the west, which gave it a very awful aspect. Mr. Cleaver said, that he saw not the faintest traces of it after 8 o'clock, so that it might probably about that time, or a little before, have totally disappeared.

The same night, at $8^h\ 10^m$ P. M. Mr. S. saw in the great quadrangle of Christ-church, and that part of Fish-street adjoining to it, several lucid streamers, ascending in the N. and N. W. from the horizon, or rather a dusky kind of vapour contiguous to it, to a very considerable height. These all moved towards the S. and S. E. with great velocity; and soon after many other similar streams of light shot up from the horizon, in various parts of the hemisphere, particularly in the S. and S. E. They were all of a very pale yellow colour, such as those that form the auroræ boreales of the common kind. They constantly multiplied, in so amazing a manner, and with such surprizing celebrity, that by $8^h\ 15^m$ P. M. they seemed to have almost entirely covered the greatest part of the hemisphere, and then centred in a point a little to the S. of the zenith. They were attended by an infinite number of flashes, or corruscations, and undulations of the lucid matter, as is usual in such phenomena. In fine, the whole atmosphere, or rather the whole collection of the luminous vapour lodged in it, was in a continual agitation for above a quarter of an hour, during which time, the whole hemisphere seemed to be all on a blaze. This most glorious and extraordinary appearance was, however, of a very short and inconsiderable duration; the extinction of the whole being so completely effected by $8^h\ 40^m$ P. M. that no remains of the phenomenon, in any part of the heavens, could then be discerned.

But what principally engaged his attention this evening, was a luminous arch, or zone, of a very beautiful purple colour, such as he had never seen before; which presented itself to view about $8^h\ 40^m$. P. M. and extended from E. to W. nearly bisecting the hemisphere. This became fainter a little before 9 o'clock; and in less than 10 minutes time totally disappeared.

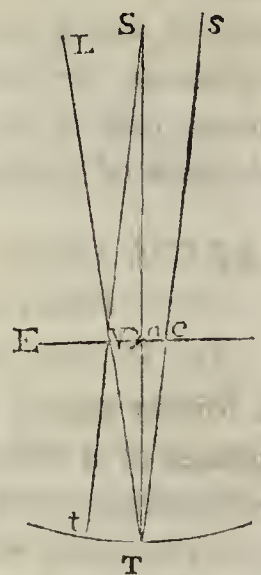
The light cast by the auroræ boreales above-mentioned was greater than any he had ever observed to attend such phenomena before. Nor did he ever meet with a description of any meteors resembling that mentioned here in every particular. The conversion of the flame-colour, in the first stage of the meteor, into a deep blood-red, with its wonderful expansion, and the beautiful purple zone, or coloured arch, which closed the whole, are singularities that probably never occurred before; or, at least, such as have never hitherto met with a proper and adequate description. Some of these phenomena seem, from the public papers, to have been seen at London, Windsor, and other places at a considerable distance from Oxford, about the same time that they appeared there; which remarkable circumstance, on several accounts, merits a place in this letter. The whole city, for a short time, seemed to be perfectly illuminated; the light cast by the auroræ succeeding the luminous appearance of a deep blood-red colour, being much superior to that of the full moon. In fine, the whole phenomenon (or rather all the phenomena) was so very striking and remarkable, that it was one of the most common topics of conversation, among all orders and degrees of people here, for above a month after it appeared.

XLVII. On the Effect of the Aberration of Light on the Time of a Transit of Venus over the Sun. By Rd. Price, D. D., F. R. S. p. 536.

Dr. P. does not doubt but that the observation made by M. Winthrop, in a former paper, is right. The aberration of Venus must, he thinks, affect the phases of a transit, by retarding them, and not by accelerating them. This retardation is $55\frac{1}{2}^s$; for that is the time nearly which Venus, during a transit, takes to move over $3''.7$. This, however, is by no means the whole retardation, of a transit occasioned by aberration. There is a retardation arising from the aberration of the sun, as well as from that of Venus. The aberration of the sun, it is well known, lessens its longitude about $20''$; and the aberration of Venus, agreeably to Mr. W.'s demonstration, increases its longitude at the time of a transit $3''.7$. Therefore Venus and the sun, at the instant of the true beginning of a transit, must be separated from one another by aberration $23''.7$; and since Venus then moves nearly at the rate of $4'$ in an hour, it will move over $23''.7$ in $5^m\ 55^s$. And consequently from the instant of the real beginning of a transit, $5^m\ 55^s$ must elapse before it can begin apparently.

It may be objected, that the aberration of the sun ought not to be taken into

consideration, because the calculations from the solar tables give the apparent places of the sun, or its longitude with the effect of aberration included, and therefore always about 20^m too little. But from this observation a conclusion will follow very different from that which the objection supposes. The retardation mentioned, is properly the time that the calculated phases of a transit of Venus will precede the apparent phases, supposing the tables, from which the calculation is made, to give the true places of the sun. If they give the apparent places of the sun, this retardation, instead of being lessened, will be considerably increased. In order to prove this, it may be remembered, that in deducing, by trigonometrical operations, the geocentric places of a planet from the heliocentric, the earth is supposed to be in that point of the ecliptic which is exactly opposite to, or 180° from the place of the sun, and that this supposition is just only when the sun's true place is taken. In reality, the earth is always about $20''$ more forward in its orbit, than the point opposite to the sun's apparent place; and in consequence of this it will happen, that in calculating a transit of Venus, from tables which give the sun's apparent places, a greater difference will arise between the calculated and the observed times, than if the tables had given the sun's true places.



For, let S be the sun, T the earth, V Venus. Were there no aberration of light, the sun would be always seen in its true place, or in the direction TS. But, in reality, in consequence of aberration, it will be seen $20''$ less advanced in the ecliptic, or in the direction T*s*, supposing STS to be an angle of $20''$. Now a calculation from tables giving the true places of the sun, would not be the time of the observed conjunction, to the time that Venus gets to TS; but this, though the time of the true conjunction, would not be the time of the observed conjunction; for the sun being then really seen in the direction T*s*, Venus, after getting to TS must move $20''$, or from *a* to *c*, before the apparent conjunction can take place.

But if the calculations are made from the apparent places of the sun, the conjunction will be fixed to the time Venus gets to *t* S, or a line drawn through S parallel to *s* T; for in this case *t* will be the point of the ecliptic opposite to the apparent place of the sun, and the longitude of the sun seen from *t* will be $20''$ less than its true longitude, and therefore the same with its apparent longitude. But the earth being then really at T, Venus will, at the calculated time of a conjunction, be observed at a distance from the sun equal to the angle LT*s*. This angle, supposing VT 277, and VS 723, may be easily found to be $72''.2$. Add to this $3''.7$, the proper aberration of Venus at the time of a transit, removing it more towards E, and the whole visible distance of Venus from the

sun's centre at the calculated moment of a conjunction, will be 75″.9, over which it will move in 19 minutes of time. And this, consequently, will be the retardation of the phases of a transit of Venus occasioned by aberration, on the supposition, that in calculating, the sun's apparent, and not his true place is taken.

In a postscript to this letter Dr. P. says: in a former letter, I gave, by mistake, the error occasioned by aberration, less than I have now given it. The discovery of this mistake I owe to the kind assistance and correction with which Mr. Maskelyne, the astronomer royal, has been pleased to favour me. I have, for the sake of more distinctness and clearness, supposed Venus to move in the plane of the ecliptic. Some differences will arise from the inclination of the path of Venus to the ecliptic, and also from taking the aberration of the sun, and the proportion of Venus's distance from the earth to her distance from the sun, exactly as they really are at the time of a transit. Thus, at the time of the last transit of Venus, supposing light to come from the sun to the earth in $8^{m}.2$, the aberration of the sun was 19″.8. The distance of Venus from the earth was to its distance from the sun as 290 to 726, and therefore the retardation $18^{m}\ 16^{s}$. Mr. Canton has observed, that in the Con. des Temp, Mr. De la Lande makes the effect of abberration, at the inferior conjunction of Venus and Mercury, to be an augmentation of their longitudes. Indeed, Mr. Bliss himself observes this; and yet through an oversight, makes the effect as to time to be an acceleration. Vid. Phil. Trans., vol. 52, p. 249.

XLVIII. A Catalogue of the Fifty Plants from Chelsea Garden, presented to the Royal Society by the Co. of Apothecaries, for the Year 1769, *pursuant to the Direction of Sir Hans Sloane, Bart. By Wm. Hudson, F. R. S.* p. 541.

This is the 48th presentation of this kind, completing to the number of 2400 different plants.

XLIX. A Short Account of the Observations of the late Transit of Venus, made in California, by Order of his Catholic Majesty, by Don Vincent Doz. Communicated by his Excellency Prince Masserano, Ambassador from the Spanish Court, and F. R. S. p. 549.

Don Vincent Doz, commander of a Spanish frigate, is just arrived at Madrid. He brought with him, and presented to the king, an account of his observation of the last transit of Venus at California, whither he was sent last year for that purpose, being in substance as follows:

The latitude of the village of St. Joseph, 8 leagues distant from Cape St. Lucar 23° 5′ 15″; the longitude from the meridian of Paris $7^{h}\ 28^{m}\ 17\frac{1}{2}^{s}$; the two internal contacts of the planets were at $17^{m}\ 25^{s}$, and at $5^{h}\ 54^{m}\ 44\frac{1}{2}^{s}$. Hence,

on the computation of Mons. Pingré, in his Memoir of the year 1767, the solar parallax is $8\frac{1}{4}''$. And the distance of the sun from the earth is greater than it was supposed to be $\frac{7}{33}$, or nearly 6,685,000 leagues.

L. Extract of a Letter, dated Paris, Dec. 17, 1770, to Mr. Magalhaens, from M. Bourriot; containing a short Account of the late Abbé Chappe's Observation of the Transit of Venus, in California. Translated by Dr. Bevis, F. R. S. p. 551.

The 7th of Dec. inst. the journals and MSS. of the late Abbé Chappe were deposited at the Royal Observatory, with M. Cassini de Thury, by the Sieur Pauli, one of the king's engineers and geographers, who had accompanied the Abbé in his voyage to California. M. Pauli relates, that M. Chappe chose to stay at St. Joseph, a small village 10 leagues from St. Lucar, though the contagious disease prevailed there, and relying on his own good constitution, because he had no more than 8 days to prepare for his observation.

Eight days after the transit, he sickened, yet continued his observations to the 18th of July; and, a little before his death, left his materials in writing, put into a box, with M. Pauli, to be delivered to the Royal Academy. He died about the 1st of August, as did, about the same time, the clock-maker, the interpreter, one of the two Spanish officers, besides 12 soldiers, and 4 officers sent from Mexico, and about 50 Indians.

The first internal contact was at	0^h	17^m	27^s
The second contact at	5	54	$50\frac{3}{10}$
The duration	5	37	$23\frac{3}{10}$
The latitude of the place	23°	3′	37″

Lastly, according to M. de la Lande, the parallax of Venus $8\frac{1}{2}$;
and her distance about 35000000 leagues of 2283 toises each, on a mean of comparisons with observations made in the north of Europe, at Cajaneburg and Wardhus.

END OF THE SIXTIETH VOLUME OF THE ORIGINAL.

I. Remarks on the Nature of the Soil of Naples, and its Neighbourhood. By the Hon. Wm. Hamilton. p. 1. *Vol. LXI, Anno* 1771.

This paper and the following one may be consulted in the collection of Sir W. H.'s Essays, published in 8vo. 1772.

II. Extract of another Letter from Mr. Hamilton, on the same Subject. p. 48.

III. Observation of the Transit of Mercury over the Sun, Nov. 9, 1769. By J. Winthrop, Esq., F. R. S., Cambridge, New England. p. 51.

On Thursday the 9th of November, Mr. W. had an opportunity of observing the transit of Mercury. He had carefully adjusted his clock to the apparent time, by correspondent altitudes of the sun, taken with the quadrant for several days before, and with the same reflecting telescope as he used for the transit of Venus. He first perceived the little planet making an impression on the sun's limb at $2^h\ 52^m\ 41^s$; and it appeared wholly within at $53^m\ 58^s$ apparent time. The sun set before the planet reached the middle of its course; and for a considerable time before sun-set, it was so cloudy, that the planet could not be discerned. So that he made no observations of consequence except that of the beginning, at which time the sun was perfectly clear. This transit completes 3 periods of 46 years, since the first observation of Gassendi at Paris, in 1631.

IV. Observations on the Heat of the Ground on Mount Vesuvius. By John Howard, Esq., F. R. S. p. 53.

Mr. H. here communicates some observations which he made in June, on the heat of the ground on mount Vesuvius, near Naples. On ascending the mountain, he often immerged the bulb of a thermometer in the ground, but found no sensible heat for some time; the first rising in the thermometer was 114°; every 2 or 3 minutes, he observed the instrument, till he gained the summit. At those times, he found it rising to 122°, 137°, 147° 164°, and 172°: on the top, in two places, in the interstices between the hard lava, it was 218°. Such a degree of heat, after he had overcome the inconvenience of the exhalations, raised his curiosity to know if there was a still greater degree of heat in the mouth of the mountain. Accordingly, he made a small descent, and, by 2 observations carefully and attentively made, the thermometer both times stood at 240°.

If it should be asked, how a person, either to their feet or in stooping or lying down to make the observations, could endure such a degree of heat; Mr. H. answers, that the heat, both at top and in the mouth of the mountain, was only in particular places. This was known by the fumes; the hard masses of lava were only warm, and even so tolerable as to permit him to lie on them; as he was often obliged to do, when the thermometer was immerged, to make a true observation.

V. Description of a Bird from the East Indies. By Mr. George Edwards, F. R. S. p. 55.

At Valentine House, near Ilford in Essex, the seat of Charles Raymond, Esq.; Mr. E. saw some curious birds and other animals, from the East Indies; among

these he discovered a rare bird, not before known to him.* It is of a new genus, and the only species of the genus hitherto known to him. It is about the size of a heron, see fig. 1, pl. 3;† and has a good deal of the appearance of birds of the heron and crane kind, except that the neck is a little shorter. On first sight, he thought the bird belonged to that genus; but, on a closer view, he judged it to be no wader in the water, for though the legs be as long or longer than in herons, &c. yet they are feathered down to the knees, which we do not find in birds who wade in shallow waters, to seek their food. The toes in this bird are also much shorter than they are in herons, so that he thinks it must be placed among land birds. The bill is exactly like those of hawks, and other birds of prey, which is the only instance he has discovered in any of the long legged kind of birds; the talons or claws are small and unfit for a bird of prey, and the eyes are of a dark colour, placed in spaces covered with a bare skin of an orange colour, on each side of the head. It has a beautiful crest, composed of many long painted feathers tipped with black, hanging backward. The beak, head, neck, back, breast, and upper covert feathers of the wings, are of a bluish ash-colour, rather lighter on the breast than on the back. The belly, thighs, the greater wing-feathers, and tail, are black, the tail feathers being tipped with white; the legs and feet are of a reddish flesh-colour, the claws black. This bird was called a snake-eater, by those who brought it from India. He believes it may prey on small serpents, lizards, and other small reptiles. Another bird was brought with this, supposed to be the male of this species, which died soon after it was landed. Mr. Raymond's servant said it was something larger, and the crest longer, the head black, but that in other respects the two birds agreed.

VI. An Extract from the Register of the Parish of Holy Cross in Salop, being a Second Decade of Years, from Michaelmas 1760, *to Michaelmas* 1770, *carefully digested in the following Table. By the Rev. William Gorfuch, Minister of that Parish.* p. 57.

		1761.	1762.	1763.	1764.	1765.	1766.	1767.	1768.	1769.	1770.	Total.	
Baptized,	males	17	18	20	18	19	23	22	17	22	18	194	382.
	females	14	20	22	21	19	16	30	9	20	17	188	
Buried,	males	11	11	16	14	19	33	11	19	12	15	161	365.
	females	14	13	20	13	19	54	18	20	14	19	204	
												Increase	17.

* This singular bird is the *Falco serpentarius* of the Gmelinian edition of the Systema Naturæ, but by Mr. Latham in his Ornithology is with more propriety referred to the genus *Vultur*, under the name of *Vultur serpentarius*.

† This bird was described, under the name of the sagittarius from the Cape of Good Hope, by Mr. Vosmaer, keeper of the Stadtholder's museum at the Hague, in one of his publications in low

There remain alive, under 10 years of age, males 126, females 122, in both 248. From 70 to 75, males 12, females 21, both 33. From 75 to 80, males 8, females 3, both 11. From 80 to 85, males 3, females 6, both 9. From 85 to 90, males 3, females 5, both 8.

Houses or families in 1765, 249—in 1770, 240. Ditto, paying window tax, in 1765, 70—in 1770, 65. Void houses, none. Number of persons in 1765, 1096; ditto, in 1770, 1046.

VII. On the Manner in which the Chinese Heat their Rooms. By Mr. Stephen de Visme. p. 59.

This is merely a letter of ceremony to introduce the following.

VIII. An Account of the Kang, or Chinese Stoves. By Father Gramont. Translated from the French. p. 61.

A kang is a kind of stove, that is heated by means of a furnace, which casts all its heat into it. Many kinds of stoves, ovens, and furnaces, have indeed been contrived elsewhere, which are somewhat like this, but the Chinese seemed to have found means to unite all their conveniences and uses in the kang. It is of various sorts; the kang with a pavement, or ti-kang; the kang for sitting people, or koa-kang; and the chimney kang, or tong-kang. As they are all made on the same principle, the description of the koa-kang may be sufficient.

The parts of a kang are, 1, a furnace; 2, a pipe for the heat; 3, a brick stove; 4, two funnels for the smoke. The furnace is proportioned to the size of the stove it is intended to heat. The lowest part is the ash hole. Next the cellar. Then the furnace; having a slit, or mouth, that conveys the flame and heat into the stove by a pipe or conductor for the heat, beginning at the mouth of the furnace, and forming a channel which falls in a right angle on a second, that goes quite through under the middle of the floor, and this last pipe has vent holes here and there. The stove is a pavement made of bricks, which being supported at the four corners by little solid piles, leaves a hollow space between them and the under pavement, where the heat remains pent up, and warms the floor. The smoke funnels are at both ends of the stove, with a little opening on the stove, and another outward, which carries off the smoke.

Nothing can be more simple than the effect resulting from the assemblage of all these parts. The heat of the furnace, impelled by the outward air, and attracted by the rarefied air of the stove, rushes through the slit, ascends into the tube, spreads through the stove by the vent holes, heats the bricks, and from

Dutch, printed at Amsterdam 1769, in 4to., with a coloured cut of the same bird. It seems to feed equally on flesh and fish; which accounts for his uniting the characters of birds of prey, and of waders in water. M. M.—Orig.

them the whole room. The smoke, which has a free passage, is carried off by the funnels.

The furnace may be placed either in the room itself, or in the next room, or without doors. The poor, who are glad to make the most of the firing that warms the koa-kang, on which they sit by day, and sleep by night, place the furnace in the same room; the middling sort put it in an adjoining room; the rich and great have it on the outside, and most commonly behind the north wall. The furnace must be much below the level of the stove, that the heat and flame may ascend with the greater impetuosity into the conductor, and not drive up the ashes. The furnace is in the form of a cone, somewhat arched, that the activity of the heat and flame may be all impelled into the stove, and not fly off when the aperture at the top is left open. The two little moveable slips are planks, that take up occasionally, when people want to go down into the cellar and empty out the ashes. The opening in the furnace is narrow, and the lower end of the conductor must go quick up into the stove. The conductor is to be walled in very close on all sides with bricks, and well cemented with mortar made of quick lime. That which the Chinese use, is made with 1 part of white lime to 2 of black. The black lime is found at the entrance of the coal pits, and seems to be no other than coals dissolved by rain waters. This substance mixed with white lime makes excellent mortar, nearly resembling cement. It is proof against rain and sun, and is used here to cover and shelter whatever is exposed to the weather. We should rejoice if this hint could prove useful to the British nation. If their country affords black lime, they are possessed of a great treasure.

The ground or flooring of the stove may be of beaten clay, or, what is infinitely better, bricks placed edgewise, or large paving tiles. The funnel for the smoke, or rather the two funnels, must be made with great care. Some make them terminate in little chimneys, that carry off the smoke above the roof. In the model, they open into the room, as the city poor have them, but in the country, and in gentlemen's houses, they are on the outside. It is of consequence that the little piles which support the great square bricks of the floor be very solid, and the bricks very thick and perfectly square. The Chinese bind them with a sort of cement made of white and black lime, tempered with tong yeou, which is a kind of varnish. We are apt to think walnut or linseed oil boiled would do as well. As soon as the kang is completed, fire is kindled in the furnace, to dry it quick and even. Great diligence must be used in examining it, in order to stop up all the little holes through which the smoke might escape. The wealthy, to make their kang neater, and to moderate its heat, oil the bricks of the floor, and light the fire, to make the oil penetrate deeper, and to dry them the faster. This oil is again the tong yeou, and may be supplied with walnut oil.

The bricks in the royal apartments are 2 feet square, and 4 inches thick. They cost near 100 crowns a-piece; and are so beautiful, good, and solid, that you can have no conception of any such thing beyond the seas. They are grey; but this is owing to the Chinese manner of baking their bricks and tiles, which comes nearer to that of the ancients than ours. These bricks when coloured and glazed appear as fine as marble.

When a kang is thoroughly heated, very little fire is required to keep it warm, though here the thermometer is almost all the winter at 9, 10, and even 12 or 13 degrees below the freezing point, in Reaumur's thermometer; and though all the rooms are on the ground floor, and have nothing but windows, and those paper windows, all over the front, which is commonly to the south, the warmth of the kang is sufficient to keep up their temperature at 7 or 8 degrees above frost, with very little fire constantly kept up. It seldom rises to more than 4 or 5 degrees in the emperor's apartments, owing to the double row of bricks; but the warmth is very gentle and very penetrating.

As a kang is heated by a furnace, any kind of fuel will do, viz. wood, charcoal, sea coal, furze, &c. The Chinese make the most of every thing. In the palace they burn nothing but wood, or a kind of coal which neither smokes nor smells, and burns like tinder. The generality of people burn sea coal: the poor in the country make use of furze, straw, cow dung, &c. A great saving may accrue from the following observation: the Chinese, to save coals, pound them to the size of coarse gravel, and mix them with one third, or even an equal quantity, of good yellow clay. This mixture being well kneaden, they make it up into bricks, which strike a greater heat than wood, and come incomparably cheaper. The sea coal thus tempered is far less offensive; and besides, the Chinese, in order to draw off the noisome vapours of the air, constantly heated by the coal fire, always keep bowls of water in the rooms, and renew them now and then. The gold fishes that are kept in these bowls are both an ornament and amusement. In the palace, the emperor's apartments are decorated with flower pots, and little orange trees, &c. The Chinese philosophers pretend that this is the best way to sweeten the air, and absorb the fiery particles dispersed in it. They likewise leave 2 panes open night and day at the top of each window, to renew the air, which they think is too much rarefied by the heat.

The kang is attended with many advantages and conveniencies. 1°. The rich and great are not exposed to the troublesome attendance on a fire in the chimney, and enjoy all its benefits. 2°. The poor use all sorts of fuel without any other expence than what the kitchen requires, and have the comfort of sitting warm by day, and lying warm by night. The fire in the furnace serves to dress victuals, and to heat the stove. The poor go still further, they enclose within the brick work of the kang a vessel, either of copper tinned, or of iron, which

supplies them with hot water for their tea. This water evaporates in the night, moistens the air of the room, and absorbs the noxious particles of the sea coal.

The Chinese sea coal may give some insight into the formation, qualities, uses, and nature of this singular fossil; but this would require a separate paper. All we shall here observe is, that, as far as we can judge from the samples we have seen, it seems for the most part to be a stone dissolved by the waters, and impregnated with sulphur. Its hurtful qualities proceed from a mixture of antimony, copper, iron, &c. The best coal, and that which burns fiercest, is glossy, hard, and brittle. The Chinese are very fond of that sort that flies and snaps in the fire, to burn in their forges, because it contains a great deal of saltpetre. When the flame is blue, it is very fierce, but it is too dangerous, as the sulphur is too predominant.

IX. Of a Remarkable Thunder Storm. By the Rev. Anthony Williams, Rector of St. Keverne in Cornwall. p. 71.

For several days before the thunder storm which fell on St. Keverne spire and church, on Sunday the 18th day of February 1770, the wind was very hard at N. and N. W. accompanied with violent showers of hail, which had done some damage to the roof of the church, and many houses in the church town. On the Sunday morning above-mentioned, the wind being at N. W. from 5 o'clock during almost the whole day the wind was excessive hard; and about 6, were some few faint flashes of lightning. The weather being so bad, prevented many people from coming to church, which probably was a happy circumstance; for, about a quarter after 11 o'clock, while Mr. W. was in the latter part of the Litany service, there was a very fierce flash of lightning, followed at the distance of about 4 or 5 seconds by the loudest thunder he remembers ever to have heard; but which did no damage, nor seemed in the least to disturb any of the congregation, though at the same time the roof of the church was rifling, and the hail made a noise terrible to be heard. In half a minute after this, the whole congregation, except 5 or 6 persons, were at once struck out of their senses. Mr. W. received the shock so suddenly as not to remember he either heard the thunder or saw the lightning; the first thing that he recollected with any degree of certainty was, that he found himself in the vicarage seat, which is very near the desk, without either gown or surplice, bearing in his arms as he then thought a dead sister, and God knows it was a miracle that she was not so; he perceived a very strong sulphureous smell, almost suffocating, and a great heat. At this time the confusion among the congregation was inconceivable, some running out of the church for safety, and returning into it again, for the stones from the roof were falling on their heads both in and out of the church; some on their knees, imploring the divine assistance, giving themselves up to certain

destruction; and a great many, in different places of the church, lying quite motionless, whom he thought then to be quite dead.

In the afternoon, Mr. W.'s thoughts being a little composed, he walked to the church, to see what damage was done; and such a scene presented, as is horrible to think of, much more to see. The church-yard was almost full of ruins; the spire, which was about 48 feet high from the battlements of the tower, was carried off half way down, and the remaining part cracked in 4 places very irregularly down to the bottom. The north side of the tower, from the battlements to the arch of the bell chamber window, was quite out, except the corner stones, which remained firm; the lead on the top of the tower was greatly damaged, melted in several places, and as it were rolled together. The arch of the belfry door, which was very strongly built with a remarkably hard iron stone, laid in lead, was also greatly damaged; some of the stones were cracked cross-ways, and just removed out of their places, others were quite thrown out, and the lead between the joints not only melted, but loosened so as that they might be picked out with your fingers. The traces of the lightning were here discovered along the surface of the earth; the stones were thrown from the spire on the tops of many houses in the Church town, but did no great hurt; in a gentleman's house, one stone weighing 14 pounds fell through the roof into the chamber, but did no further hurt than to make a hole in the roof and plastering. The stones from the spire were scattered in all directions, as well against the wind as with it, some of which, but not very large, were found but a little short of a quarter of a mile. The spire from the top 6 feet downwards was solid, through which passed an iron spindle to fix the weather-cock on. Did not the lightning first strike on this iron, and was conducted through the solid part of the spire, and having not iron to conduct it any further, burst in the hollow part of the spire, and threw the stones about in all directions? It is remarkable that the spindle was found in the bell-chamber, and the weather-cock in the battlements; and that the bells were not in the least damaged, though a deal board, that lay across the beams to which the bells were hung, was split long-ways in 2 pieces. The inside of the church still presented a much more horrible spectacle; the roof of the church was almost all gone, and some of the timber-work in the north aisle shattered to pieces; every seat in the church had rubbish in it, more or less, and stones of large size, some of 150 pounds weight and upwards, scattered here and there amidst the congregation, which damaged the seats, &c. but did no hurt to the people, though they sat in those very seats where the stones fell. The lightning entered at the three ends of the church at west, made its way through the body of the church, and went out through the 3 ends of the church at east; the holes where it came in and where it went out are not large,

neither are the walls much damaged. The belfry window was shattered to pieces, not one whole pane to be found in it; many other windows also suffered greatly, the glass and munnions being much shattered. The lightning entered also through two places in the roof, one near the singing loft, and struck on the top of a pillar just by it: the traces of it are to be seen from the top of the pillar almost to the bottom: there were then sitting by this pillar 2 young men, one in the singing loft, and the other under him in the church, who were both lightly scorched; he in the loft from head to foot, and the other in the face only; but it is remarkable that his hat, which hung on a nail just above him, was cut in two pieces. In the other place, the lightning entered just over the desk and pulpit, and fell in like manner on a pillar that stands in the vicarage seat; but here it was a great deal more violent, and the object of its fury was Mr. W.'s sister. On this pillar rested a large oak soil, the bottom of which was burst into 6 pieces, and one of the pieces, being a very large one, was thrown from its place to the distance of about 20 feet, and appeared to be burnt; the other pieces did not fall. Hence the lightning came down the pillar with great force, tore the seat into many pieces, knocked down his sister, and made its way through the bottom of the seat into the earth. She had pattens on, and the wooden part of one of them was broke into 3 pieces; the holes through which the ribbon is put to tie them together, were quite burnt out, and the ribbon found in the seat without the least damage, or so much as the knot loosened; her shoe was burnt, and rent from the toe to the buckle; but the buckle, which was of silver, remained unhurt; her stocking was burnt and rent in the foot, just in the same manner as her shoe, and scorched along to the garter, and two little holes were burnt through in the leg of it: her apron, petticoats, &c. were burnt through and through, and she had several slight burns on several parts of her body, besides two bruises on her head and breast, caused by the rubbish that fell into the seat. As she was carrying out of church, she greatly complained of a deadness in her legs, which, as she could not move them at all, he supposed were broken; however they were only a little burnt, and turned as black as ink; which, by timely care, not only came to their natural colour by Tuesday noon, but could support her also to come down stairs; and, excepting a hurry of spirits, got quite well that week.

Not more than 10 persons out of the whole congregation were hurt, and none of them to any great degree; one young fellow, who was more frightened than hurt, remained ill a long time, but he is now quite well; the lightning touched his watch in his pocket, the marks of which may be seen on the crystal and silver part of it. Nobody remembers to have heard any more thunder, or seen any lightning after this, though the weather continued very stormy all that day; so that this thunder-storm, from beginning to end, could last but a very short time.

X. Explication of an Inedited Coin, with Two Legends, in Different Languages, on the Reverse. By the Rev. John Swinton, B.D., F.R.S. p. 78.

This coin on one side presents the head of Jupiter, and on the other the prow of a ship, which indicates the place where it was struck to have been a maritime town. Above the prow of the ship are 2 characters, either Punic or Phœnician. Besides these 2 there is a monogram, formed of the three Latin letters V, A, B, very indifferently preserved, in the exergue, with which the Punic or Phœnician elements perfectly correspond. Hence the learned will easily admit the medal in question to have been struck at Vabar, a maritime city of Mauritania Cæsariensis, after that place had been ceded to the Romans, and was inhabited by them, and either the Carthaginians or the Phœnicians.

XI. Remarks on Two Etruscan Weights, or Coins, never before published. By the Rev. John Swinton, B.D., F.R.S. p. 82.

The first piece to be considered here is an Etruscan as, or weight, exhibiting on one side the head of Janus, covered with a cap; and on the reverse a club, attended by the mark of the as, and a legend in Etruscan characters. Between the two faces of Janus, the head of a buffalo, or wild ox, presents itself, as does a sort of concha marina, or sea-shell, contiguous to the cap; both of which have not a little suffered from the injuries of time. The letters on the reverse are more rude and barbarous than those of any similar Etruscan coins hitherto published, which is an incontestible proof of the exceeding high antiquity of this piece. The forms of several of them are likewise somewhat different from those of the correspondent elements on all the other similar Etruscan weights, hitherto communicated to the learned world. The concha marina, and perhaps the buffalo's head, is a singularity that will anounce the weight to be an inedited coin. The piece weighs precisely 5 ounces, and 12 grains; and is, in all respects, except what relates to the concha and buffalo's head, tolerably well preserved.

The first riches of mankind were their flocks and their herds, and particularly their oxen. Hence the first money in Italy, from pecus, was called pecunia, and the most ancient brass coins had the figure of an ox impressed on them. Hence also the Greeks, in the days of Homer, estimated the value of their properties according to the number of oxen they were equivalent to, as we learn from that celebrated poet. For he informs us, that Glaucus's golden armour was worth 100 oxen, whereas that of Diomedes, for which it was exchanged, did not exceed the value of 9 of those animals. The figure of the ox on the most ancient money seems to have been soon converted in Etruria into the symbol of the head of that beast connected with the head of Janus, who is said to have first introduced the use of money into Italy.

From what has been observed, as well as from the thickness, high relief, and extreme rudeness of the workmanship, or rather in conjunction with these, we may conclude, that our as is either coëval with some of the earliest pieces, or weights, ever used in Italy, or but little posterior to them. That the weight here considered is to be assigned to a maritime town, the concha marina, or sea-shell, irrefragably proves. He therefore attributes it to Volterra, which was the most ancient city of Etruria, the seat of a lucumo, and one of the most considerable places in Tuscany. It was also a maritime city, as we learn from Strabo, being seated not far from the Vada Volaterrana, near the place where the river Cæcina threw itself into the Tyrrhenian sea. Mr. S. therefore reads the legend on the reverse of this coin, FELATHERI, FELATERI, or FELATERRI; the 5th letter being sometimes endued with the power of Theta, and sometimes with that of Tau; and a duplication of consonants, in writing, having been unknown to the most ancient Etruscans.

The second piece, or weight, is a stips uncialis, as appears both from the weight and size of it, of the earliest date. On one side it has preserved the head, or rather a full face, of the sun; the workmanship of which is more rude and barbarous than that of any other similar piece that ever fell under Mr. S.'s view, and done perfectly in the most ancient Etruscan taste. The reverse had originally on it the prow of a ship, which has been so totally effaced by the injuries of time, that only a very few exceedingly faint traces of it are now to be seen. The relief on the face-side is very high, as was doubtless at first that on the other; but the reverse being in a manner quite smoothed, nothing there remains but the vestiges of the prow of a ship, that are barely visible. However, just over the prow, we may discover clearly enough a legend in Etruscan characters, though but very indifferently preserved. That word is apparently equivalent to ROMA, and consequently the piece itself must be deemed an uncia, or stips uncialis, of Rome, though the globule, or uncial mark, has not escaped the ravages of time.

That the piece in question is an uncia of Rome, appears not only from the legend on the reverse, as just observed, but likewise from another uncia of Rome, with the full face of the sun on it, as here, though done in the more modern Roman taste, now in his collection. We may therefore safely enough pronounce the coin here described a stips uncialis of Rome, of a very remote antiquity, with the Etruscan name of that capital of the world on the reverse. The Etruscan letters were doubtless the first alphabetic characters of Italy. Nay, they prevailed at Rome, and in every part of Italy, till after the regifuge. And he is induced to conclude, that it is at least coeval with the regifuge, which happened in the year of Rome 245; or rather, that it may be a considerable number of years anterior to that event.

XII. Interpretation of Two Punic Inscriptions, on the Reverses of two Siculo-Punic Coins, published by the Prince di Torremuzza, and never hitherto explained. By the Rev. John Swinton, B.D., F.R.S. p. 91.

These two Punic legends have been published, with 5 others, by the Prince di Torrimuzza, in his volume of ancient inscriptions, printed at Palermo in 1769.

The first of these minute inscriptions, which is the first of those published by the Prince di Torremuzza, in the place here referred to, adorns a fine Punic tetradrachm, as it should seem, well enough preserved; which on one side presents the head of a woman, and 3 fishes, but on the reverse the head of a horse, behind which stands a palm tree, attended by an inscription in the ezergue formed of 7 Punic letters. The workmanship, as well as the types, is probably similar to that of the silver medals of Menæ, described and explained in a former paper. The import of the inscription, in Roman letters, he thinks is AM SEQHEGT, or SEGEGHTH, which is but a small variation from the word SEGESTE, or SEGESTA, the Greek and Latin name of a considerable maritime city of Sicily, not far from Eryx, where money was coined, after the Greeks had possessed themselves of the place. The medal therefore adorned with this minute Punic inscription may, without any impropriety, be supposed to have been emitted from the mint at Segesta, as the Punic words, AM SEGHEGT, or SEGEGTH, POPVLVS SEGESTANVS, appear on it, when the Carthaginians were masters of that city, and occupied all the adjacent territory appertaining to it.

As no chronological characters occur on the piece considered here, the time when it was struck cannot with any precision be ascertained. That operation must however have preceded the conclusion of the first Punic war; since the Carthaginians, by the treaty of peace which terminated that war, ceded the whole of their possessions in the island of Sicily to the Romans. Nay this medal was probably prior, perhaps many years, to the surrender of Segesta to the Romans, in the beginning of the first Punic war, when the inhabitants of Segesta put the African garrison there to the sword, about 258 years before the birth of Christ; the Carthaginians seeming never to have been possessed of this ancient city, after that tragical event.

The second of the inscriptions is composed of 7 letters, which he shows from the two words עמ המחנת, AM HAMMAHANOTH HAMMEHNOTH, or HAMMENOTH, POPVLVS MENENIVS, or MENARVM POPVLVS, as we may find rendered incontestable by other similar coins.

The medal which has conveyed down to us this inscription, through such a series of ages, is of the tetradrachmal form, and of a very considerable antiquity. On one side it exhibits the head of a woman, goddess, or tutelary deity of the

place where it was struck, with 3 fishes sporting round it; and on the reverse a horse's head, under which appears the inscription. It will be almost needless to remark, that the horse's head is one of the most usual symbols on the reverses of the ancient Carthaginian coins.

XIII. On a New Comet. By M. Messier, of the Royal Acad. of Sciences, and F.R.S. Translated by Dr. Bevis, F.R.S. p. 104.

M. Messier discovered a new comet, the 10th of Jan. instant, 1771, about 8 o'clock in the evening; it was between the head of Hydra and the Little Dog, over the parallel of Procyon. The position of which he determined by comparing it with that star, and the star δ in Hydra. The observations are as follow:

1st Obs.	The 10th of Jan. 1771, at	10^h	16^m	45^s true time.
	Right Ascension of the Comet	121°	47	16
	North Declination	5	21	15
2d Obs.	Same night at	21	19	5
	Right Ascension of the Comet	140	24	31
	North Declination	6	4	46

From which observation it appears, that in 3^h 2^m 20^s of time, its motion in right ascension was $1^\circ 22' 45''$, and $43' 31''$ in declination: this comet was perceived by the bare eye. In the telescope its nucleus is bright, of a whitish complexion, and not very well defined, surrounded with an atmosphere several minutes wide, with a faint tail 5 or 6 degrees long. Its apparent motion among the fixed stars is contrary to the order of signs, from the equator towards the north pole.

This makes the 12th comet he discovered and observed in 13 years past.

From his further observations, M. Pingre deduced the following elements of its orbit:

Ascending ☊	3^s 18° 42′ 10″	Place of the perihelion	8^s 28° 22′ 44″
Indication of the orbit	31 25 55	Log. of the perihel. dist.	9.722833

It passed the perihelion Nov. 22, 1770, at 22^h 5^m 48^s mean time, at the royal observatory, motion retrograde. He adds, 'that the comet resembles none of those whose elements are determined on comparing its motion with the places of its perihelion and ☊: it is easy to see, that it was impossible to discover it at Paris before the year 1771; and it may even be added, that it must frequently have passed in the sun's neighbourhood, imperceptible to the northern parts of the earth.'

XIV. Description and Use of a New Constructed Equatorial Telescope or Portable Observatory, made by Mr. Edward Nairne, London. p. 107.

The instrument consists of the following parts: A mahogany triangular stand, and 3 adjusting screws; a moveable azimuth circle, which is divided into degrees, and by a vernier index to every 6 minutes; above this azimuth circle is the horizontal plate, to the under part of which is fastened the vertical conical axis; on

the middle of the upper surface of the horizontal plate, is placed a ground glass level, by which the plate is set parallel, and the pillar perpendicular to the horizon; from this plate rise perpendicularly two quadrants, one of which is divided for the latitude into half degrees, and has a vernier index to 3 minutes: the equatorial plate, with its hour circle, is supported by the two quadrants; its axis of motion, which is placed near the hours XII, XII, passes through the centres of the quadrants, and carries the index I, pointing to the divided quadrant; the equatorial plate is divided into half degrees, and has a vernier index showing every 3 minutes of right ascension, or 12 seconds of time; it is figured to show both degrees and time; to prevent misapprehension, it may be right to remark that the hours XII, XII, ought properly to have been placed according to the meridian line; they are here placed otherwise, for the convenience of better seeing the meridian distance shown by the vernier; on the upper part of the equatorial plate is a plate, on which are fixed two supporters, which support the axis, under which is fastened the semicircle of declination, divided into half degrees, and has a vernier index subdividing it to 3 minutes; on the upper part of this axis, is fixed an achromatic telescope, which magnifies about 50 times; to the eye end of this telescope, is applied a small reflecting speculum, making an angle of 45° with the axis of the telescope, by which objects that are in the zenith, or any other altitude, may be observed, without putting the body in any inconvenient position; to the under part of the axis N, is fastened a brass arm carrying the weight Q, which counterbalances the telescope, and the brass work annexed to it; while 2 weights counterbalance in like manner the whole of the instrument that is moveable on the equatorial axis; so that whatever position the instrument is put in, it will there remain, being perfectly balanced; the 4 motions of this instrument may, when required, be moved extremely slow, by means of the indented edges of the circle and semicircles, and the screws or worms to which the handles are fixed, viz. that for the horizontal motion, called the horizontal handle, the handle of latitude, the equatorial handle, and the declination handle.

To adjust the instrument for observation, the first thing to be done is to make the horizontal plate level, by means of the spirit level, and the 3 adjusting screws at the bottom of the stand; this being done, move the equatorial plate either with or without the latitude handle, until the index on the quadrant points to the latitude of the place; and then the equatorial plate will be raised to the elevation of the equator of the place; which is equal to the complement of the latitude; and thus the instrument is ready for observation.

XV. *Experiments to show the Nature of Aurum Mosaicum. By Mr. Peter Woulfe, F.R.S.* p. 114.

After describing various processes (with observations thereon) for making aurum mosaicum (sulphuretted oxyd of tin) which in the present improved state of chemical knowledge it would be useless to retain in these Abridgments, Mr. W. subjoins the following account of an apparatus for making aurum mosaicum in the cheapest manner.

A glass vessel cannot be used for this operation more than once, because it is necessary to break it to get out the aurum mosaicum. The following utensil may be employed a great number of times, and save the expence of glass. Take a black lead crucible, N° 60; bore a round hole in its bottom about 3 inches diameter; and saw off an inch of its upper edge; if it has a lip, get a round piece of burnt clay, of an inch thick or rather more, to fit exactly into this edge; the composition, which is used for making paving-tiles, answers very well for this purpose. In order to make use of this apparatus, fit the round piece of burnt clay to the inner edge of the crucible, by means of some loam softened with glue, and dry it slowly; then turn it upside down, and lay it in a proper furnace on 2 iron bars. The mixture* for the aurum mosaicum is to be put in through the round hole at top, and then covered with an aludel and luted; this serves to collect the flowers and the sublimate which rises. The fire is to be made under and all round the crucible. 11 lb. Troy of aurum mosaicum may be made here at a time; and when the operation is over, the bottom or round piece of burnt clay will easily come out with the aurum mosaicum. A large crucible may be made use of, if a larger quantity be required to be made at once. The operation cannot fail of success, provided the fire be made of a sufficient strength, and of an equal degree from the bottom to the top of the crucible, which is easily done in a good furnace. The operation is finished in 8 hours, unless the volatile liver is wanted.

White arsenic, digested with a solution of tin in the acid of salt, becomes soon black; it hereby regains its phlogiston, and is reduced to the taste of regulus of arsenic, and will by this means readily combine with copper, and other metallic substances; which it would not do without the help of phlogistic substances. This is the most easy and ready way of reducing arsenic to its metallic form: the arsenic may be deprived of the solution of tin, which adheres to it, by

* The mixture recommended for the preparation of aurum mosaicum by Mr. W. is as follows: tin, 12 oz.; sulphur 7 oz.; sal ammoniac, 3 oz.; mercury, 3 oz. From this mixture (varied from the formula in the Lond. Pharmacopœia of that day) he obtained $17\frac{1}{2}$ oz. of aurum mosaicum; but Mr. W. showed by other experiments, that mosaic gold could be prepared without employing either mercury or sal ammoniac. The celebrated French chemist, Mons. Pelletier, has since shown that aurum mosaicum may be obtained by a very short and simple process; viz. by subjecting to a proper degree of heat equal parts of oxyd of tin and sulphur; thus demonstrating its composition, viz. that it is a sulphuretted oxyd of tin.

washing it with water. It is to be dried slowly, for otherwise it is apt to catch fire.

Then follows an account of a method of dying wool and silk, of a yellow colour, with indigo; and also with several other blue and red colouring substances.

The Saxon blues have been known for some time; and are made by dissolving indigo in oil of vitriol, by which means the indigo becomes of a much more lively colour, and is extended to such a degree, that it will go very far in dying.

A receipt for making the best Saxon blue will, Mr. W. doubts not, be agreeable to many; he therefore gives the following, which produces a very fine colour, and never fails of success. Mix ℥ 1 of the best powdered indigo, with ℥ 4 of oil of vitriol, in a glass body or matrass: and digest it for one hour with the heat of boiling water, shaking the mixture at different times; then add ℥ 12 of water to it, and stir the whole well, and when cold, filter it. This produces a very rich deep colour; if a paler blue be required, it may be obtained by the addition of more water. The heat of boiling water is sufficient for this operation, and can never spoil the colour; whereas a sand heat, which is commonly used for this purpose, is often found to damage the colour, from its uncertain heat.

Indigo, which has been digested with a large quantity of spirit of wine, and then dried, will produce a finer colour than the former, if treated in the same manner, with oil of vitriol. No one, that he knew of, had before made use of the acid of nitre, instead of the acid of vitriol; and it is by means of the former that the yellow colour is obtained: it was nevertheless natural to use it, on account of its known property of making yellow spots, when dropped on any coloured cloth. The acid of salt does not dissolve indigo, and therefore is of no use in dying.

Mr. W. further communicates a receipt for making the yellow dye.

Take ℥ $\frac{1}{2}$ of powdered indigo, and mix it in a high glass vessel, with ℥ 2 of strong spirit of nitre, previously diluted with ℥ 8 of water; let the mixture stand for a week, and then digest it in a sand heat for an hour or more, and add ℥ 4 more of water to it; filter the solution, which will be of fine yellow colour. Strong spirit of nitre is liable to set fire to indigo; and it is on that account that it was diluted with water, as well as to hinder its frothing up. ℥ 2 $\frac{1}{2}$ of strong spirit of nitre will set fire to ℥ $\frac{1}{2}$ of indigo; but, if it be highly concentrated, a less quantity will suffice. If the indigo be digested 24 hours after the spirit of nitre is poured on it, it will froth and boil over; but after standing a week or less, it has not that property.

One part of the solution of indigo in the acid of nitre, mixed with 4 or 5 parts of water, will dye silk or cloth of the palest yellow colour, or of any shade to the deepest, and that by letting them boil more or less in the colour. The addition of alum is useful, as it makes the colour more lasting; according as the

solution boils away, more water must be added. None of the colour in the operation separates from the water, but what adheres to the silk or cloth; of consequence this colour goes far in dying. Cochineal, Dutch litmus, orchel, cudbear, and many other colouring substances treated in this manner, will all dye silk and wool of a yellow colour. The indigo which remains undissolved in making Saxon blue, and is collected by filtration, if digested with spirit of nitre, dyes silk and wool of all shades of brown inclining to a yellow. Cloth and silk may be dyed green with indigo; but they must first be boiled in the yellow dye, and then in the blue.

XVI. Account of an extraordinary Steatomatous Tumour, in the Abdomen of a Woman, By P. Hanley, M. D. p. 131.

Mrs. Reily, aged 36, pale, tall, fleshy, and formerly of a healthy constitution, was brought to bed of a strong, lively daughter, on the 23d of May, 1770, in the parish of St. Anne, Dublin. In the 5th month of her pregnancy, she felt an uncommon lump in her stomach, as she expressed it, about the size of a hen's egg, which did not then give her much pain or uneasiness, and she was in hopes that her delivery would carry it off: she had towards the end of her pregnancy frequent retchings, sometimes puked, and became emaciated; three days after she was brought to bed, she found the lump and retchings had increased; she became very uneasy, and sent for Dr. H. On examining her abdomen, he felt a considerable tumour contiguous to her stomach, which afterwards had greatly increased, and was extended obliquely to her right side, as low as her navel; it lay immediately under the peritoneum and abdominal muscles, and in the progress of its increase, he could plainly feel one large, and other smaller protuberances, of a firm substance, in some measure resembling the head and superior extremities of a fœtus. It could be easily moved from side to side, without giving her any pain; but it resisted, and made her uneasy, when he attempted to move it downwards: her abdomen appeared plump and full, as if she had not been brought to bed; but the hypochondres were more prominent and distended, than the region below her navel. He ordered for her the simple bitter infusion with absorbent powders, and delayed giving deobstruent medicines, till she had recovered her strength after lying-in. He also desired her not to suckle her infant; but, as her husband was poor, she did not comply, by which means she quickly became greatly exhausted and emaciated.

In a fortnight after her delivery, she got up daily, walked about her room, sometimes went abroad, and continued to suckle her child; but the retchings returned at intervals, the tumour increased in size, its protuberances became larger and more distinct, she was often restless, and in pain at night on lying in bed, had a hectic fever, and daily became weaker and more emaciated, with a sharp pinched-up nose, hipprocratic countenance, small, quick, weak, thread-like pulse, loss of appetite, and night sweats.

In 5 weeks after her delivery, the tumour had greatly increased in all its dimensions; and its protuberances, which to the feel seemed to resemble the head, trunk, and extremities of an extra-uterine fœtus, became more palpable and distinct, as the abdominal muscles from their distension became thinner. He brought 10 physicians, surgeons, and accoucheurs to visit her; and they were all so much deceived as to be of opinion, that the tumour was an extra-uterine fœtus: however they were deterred from attempting the Cæsarean operation, from a conviction that she was too weak, hectical, and reduced, to encourage any hopes of her recovery, in case it had been performed; and therefore they determined to leave the event to nature, especially as they could perceive no motion of any particular parts of the tumour, though it had greatly increased, and as it was possible that it might be some other tumour. She continued gradually declining; the tumour and symptoms increasing, during May and June; and the 23d of July following, Dr. P. perceived a small fluctuation of water in her abdomen, and gave her an intimation of it, which determined her to procure another nurse for her infant; but the ascites daily increased, and in 9 or 10 days after, her legs and feet became œdematous, her night sweats still continued, though her dropsy augmented, and she languished under the acute pains, more frequent retchings, hectic fever, loss of strength, want of appetite, and restless nights, except when she took an opiate, which often proved a great relief and refreshment to her. Her posture in bed now was half sitting, half lying, which was the only position she could bear without great pain and shortness of breathing.

About 7 days before her death, she was seized with a smart lax, which, in a few days, carried off part of the swelling in her left leg; she became somewhat lighter, and less distressed in her breathing, which made her vainly hope, that her disorder might be carried off in that manner; but the tumour, weakness, and other symptoms increased, till the 2d of Sept. instant when she expired.

On opening her abdomen, in presence of 7 gentlemen of the faculty, they found about a gallon of water, and a large steatomatous tumour just under the peritoneum, near 3 inches in thickness, 7 inches in length from her stomach to the obtuse angle of her ribs, and in some places near 5 inches in breadth from her sternum to the vertebræ of her back, full of prominences of different sizes. It was of a hard consistence, like tallow in its anterior part, but softer posteriorly, and divided by thin membranes into numerous cells, which were distended with hard and softer fat; it weighed 7 lb., was of an irregular figure, adhered to, and compressed, the anterior part of her stomach, and was so firmly united to the inferior surface of the liver, that it could not be separated from it without force. It pressed and concealed the colon, and extended from the stomach by her liver to the right ovarium, and vertebræ of her back: the small guts were greatly

squeezed, and mostly forced towards the left side; and the anterior lobe of the liver was so compressed between the diaphragm and tumour, that it appeared flattened, smaller than usual, and in a withered, decaying state.

There was nothing preternatural in the matrix, or any of the other bowels; but they were greatly compressed, and the tumour, from its membranes and contained fat, seemed to be a production and distension of that part of the omentum which adheres to the stomach, though it reached and adhered to the right ovarium, liver, aorta, and colon, as well as to the stomach. The operator was for some time in search of the colon, before he found it, adhering to, and almost forming a part of, the posterior edge of the tumour.

XVII. A Letter from Dr. Ducarel, F. R. S., F. S. A., to Dr. Wm. Watson, M. D., F. R. S., concerning Chestnut Trees; with two other Letters to Dr. Ducarel, on the same Subject. p. 136.

Sir,—In a letter addressed to you, on the trees which are supposed to be indigenous in Great-Britain, published in the Philos. Trans., vol. 59, p. 23, the Hon. Daines Barrington has attacked a prevailing notion among the learned, that chestnut trees are the native production of this kingdom. Mr. Barrington argues that they are not; and his reasonings on this are now to be considered.

In my Anglo-Norman Antiquities, p. 96, I had observed that "many of the old houses in Normandy, when pulled down, are found to have a great deal of chestnut timber about them; as there are not any forests of chestnut trees in Normandy, the inhabitants have a tradition, that this timber was brought from England; and there are some circumstances which, when rightly considered, will add strength to this tradition; for many of the old houses in England are found to contain a great deal of this kind of timber: several of the houses in Old Palace-Yard, Westminster, and in that neighbourhood, which were taken down in order to build Parliament and Bridge-streets, appeared to have been built with chestnut; and the same was observed with regard to the Black Swan Inn, in Holborn, and many other old buildings lately pulled down in different parts of England." And to this I had subjoined the following account in a note. "Chestnut timber being at present rarely to be found growing in the woods and forests of England, many persons are induced to think that the sweet chestnut was never an indigenous tree of this island: but a little consideration will plainly evince, that it always was, and is to this day, a native of England. It is generally allowed, that all the ancient houses in the city of London were built of this timber. Certainly it did not grow far off; and most probably it came from some forests near the town; for Fitz Stephens, in his description of London, written in the reign of King Henry II. speaks of a large and very noble forest, which grew on the north side of it. Rudhall, near Ross, in Herefordshire, an

ancient seat of the family of Rudhall, is built with chestnut, which probably grew on that estate; for though no tree of the kind is now to be found growing wild in that part of the country, yet there can be no doubt, but that formerly chestnut trees were the natural growth of the neighbouring wood lands, since we find that Roger Earl of Hereford, founder of the Abbey of Flaxley, in Gloucestershire, by his charter, printed in Dugdale's Monasticon, tom. 1, p. 884, gave the monks there, the tythe of the chestnuts in the forest of Deane, which is not above 7 or 8 miles from Rudhall. The words are, singulis annis totam decimam castanearum de Dena. In the court before the house at Hagley Hall, in Worcestershire, the seat of Lord Lyttelton, are 2 vast sweet chestnut trees, which seem to be at least 2, if not 300 years old; and Mr. Evelyn, in his Sylva, p. 232, mentions one, of an enormous size, at Tortsworth, in Gloucestershire, which hath continued a signal boundary to that manor, from King Stephen's time, as it stands upon record; and which tree is still living, and surrounded by many young ones, that have come up from the nuts dropped by the parent tree. Mr. Evelyn also assures us, that he had a barn framed entirely of chestnut timber, which had been cut down in its neighbourhood. In the forest of Kent, adjoining to Sussex, there still remain large old chestnut stubs, which were left by the woodmen as termini, or boundaries, either of parishes, or private property. Besides this, there are to this day in the N. E. part of Kent, several large woods, consisting principally of chestnut trees and stubs. In the parish of Milton, near Sittingborne, is a manor called Norwood Casteney, otherwise Chesteney, from its situation among chestnut woods, which reach to the highway from London to Dover, and give name to a hill between Newington and Sittingborne, it being called Chestnut Hill, the chestnut trees growing plentifully on each side of it, and in woods round it for many miles. And by the particulars for leases of crown lands in Kent, temp. Eliz. Roll 3, N° 8, now in the augmentation office, it appears that there is, in the same parish of Milton, a wood containing 278 acres and a half, called Cheston, otherwise Chestnut wood. To conclude, my worthy friend, Edward Hasted, Esq. of Sutton at Hone, near Dartford, in Kent, F. R. S., and F. S. A., assures me that one of his tenants at Newington, a few years since grubbed up forty acres of wood, which were entirely chestnut."

In the very out-set of the argument, Mr. Barrington imposes on himself, by changing the terms of the question. " Since you sent me, says he to Dr. Watson, the specimen of supposed chestnut, which was taken from the old hall of Clifford's Inn, I have been at some pains to examine the authority for the prevailing notion, with regard to this being an indigenous tree" but in p. 24, he says, " I shall begin by considering the proofs, which are commonly relied on to the Spanish or sweet chestnut being indigenous in Great-Britain."—Though not one word has preceded, though not one word follows, of the Spanish and the

common chestnut being the same. He then alleges, "that the very name of Spanish, seems strongly to indicate the country from which it was originally introduced here." This is surely a striking instance of an inaccuracy of language; the whole controversy between us turns only on that which is commonly called the chestnut tree, and which is therefore denominated Castanea Vulgaris, by all the ancient botanists. It is so called by Dr. Johnson in his Mercurius Botanicus: by the same author, in his Iter Cantianum; and by Blackstone, in his Specimen Botanicum; and in this true view of the controversy, let us examine the principal parts of it.

I have, Sir, in the above-mentioned quotation, particularly noticed a large tract of chestnut woods, to continue to this day near Sittingborne in Kent; in opposition to this, Mr. Barrington says, that he has taken a very minute inspection of these woods; and that, "finding them planted in rows, and without any scattering trees to introduce them, he is convinced that they are not natives." Such is the argument by which my assertion is endeavoured to be set aside.

I shall not here enter into an examination of the 4 general rules laid down by Mr. Barrington, "from which it may be decided, whether a tree is indigenous or not in any country. That I leave to the consideration of two of my particular friends, who have entered into the botanical reasons produced by Mr. Barrington, and whose letters to me on this subject are hereunto annexed. I confine myself to the fact. "Remember, says Dr. Plot, in his MS. Collectanea of Kent (in the library of Edward Jacob, Esq. of Faversham) the iron oar smelted in Chestnut wood, in the confines of Borden and Newington." Dr. Johnson, in his Iter Cantianum, 1632, speaks of the Castanea Vulgaris inter Sittingbourne et Rochester. And this chestnut wood is equally mentioned as early as the 22d of Elizabeth, under the title of Quædam Sylva, vocata Chestenwode, in a conveyance. This wood then is not very modern; and if ever it was planted by any human hand, must have been planted 2 or 3 ages ago; but it was never planted by any human hand; the whole wood covers more than 300 acres of land. In one part of Chestnut wood, on the hanging banks of Chestnut-street, and in the way from Kay-street to Stockbury, are now the remains of large chestnut trees and pollards, which were plainly planted by the bold irregular hand of nature.

I had also mentioned a grant, or rather a confirmation of a grant, made to the Abbey of Flexeley, which was the tithe of chestnuts in the forest of Dean; "totam Decimam Castanearum de Denâ." But Mr. Barrington objects to the supposition "of Dena, in the record, meaning the forest of Dean, as there are so many places of the name of Dean in the kingdom." This however is surely an objection of no weight. The Cistertian Abbey of Flexeley, or Dene, was actually situated in the forest of Dean, and was anciently called Flaxlyn Abbey

of St. Mary de Dean. This abbey, together with Dean Magna, (alias Mitchell Dean), and Dean Parva, all lie in the same hundred with the forest (the hundred of Saint Briannell), and are included in the ecclesiastical deanery, called Forest: where, therefore can the Dene of Flexeley be placed, but at the forest in which it was situated, and from which it derived half of its appellation? And what pretence can a Dene in Hampshire, or a Dean in Lancashire, have to a place in a record, which relates only to the abbey of Saint Mary de Dene, in the forest of Dean? But all such reasonings are unnecessary: the point is ascertained beyond the possibility of a doubt, by Henry the 2d's confirmation of the original grant. The King, by this record, confirms to the monks, locum qui dicitur Flexleia ubi abbatia fundata est, by the title of Locum quendam in foresta de Lenâ. He afterwards goes on, to give them omnia asiamenta in eadem foresta mea de Denâ; and then he particularly subjoins, et de eadem foresta dedi eis Decimam Castanearum mearum. Can any words possibly be more explicit than these? And can Mr. Barrington aver against the testimony of an authentic record? But, though the Dena of the record does mean the forest of Dean, Mr. Barrington has still an objection in reserve; and asserts that there are not the least vestiges of any such trees in this forest at present. But is Mr. Barrington sure there are no vestiges of chestnut trees in the forest? Did Mr. Barrington inspect into every part of this ample area? And did no trees, no stumps, no stools, escape his eye in this wide unbounded range? But the fact appears otherwise. There are not merely stumps, not merely stools, of chestnut trees; but actual and absolute trees of chestnut existing at this day, in the forest of Dean.

In a letter to me, dated Dec. 10, 1770, from the Rev. Mr. William Crawley, resident at, and minister of Flaxley (uncle to Thomas Crawley Bovey, Esq. the present owner of Flaxley Abbey); is the following account:—"In this very forest and near Flaxley is a parcel of land, about 3 or 4 hundred acres, which is still denominated chestnut: though neither chestnut, nor any other kind of tree is to be seen there, excepting what we call underwood or coppice, mostly hazel. Indeed in many places of the forest, I find chestnut trees are (sparingly) to be met with; but within a few yards of the above spot, in a wood of my nephew's, are many of remarkably fine growth." But, even if the fact had been as Mr. Barrington hath stated it, the faith of record attesting the existence of chestnut trees formerly, in the forest of Dean, was surely not to be superseded by the non-existence of such trees at present; they might have existed formerly, though they do not exist at present. And the record explicitly assures us that they did exist, and as early at least as the reign of Henry II.

The chestnut tree, therefore, may still claim a natural relation to this island, notwithstanding the two arguments of Mr. Barrington against it: and if we

look into this kingdom, we see the chestnut tree, not confined to Sittingbourne woods, or to Dean forest; but scattered with a free hand, through many parts thereof; shooting up with all the healthy vigour of genuine natives, and giving denomination to several places among us. Thus the chestnut wood of Sittingbourne, has given the name of Chestnut-street, to the neighbouring road; and the old Saxon half of the name, Street, strongly intimates the other half to be very ancient. The appellation occurs in the first map that notices the names of the roads, the map of Kent by Morden. In Hertfordshire is a town, called in old writings, Cheston, Chesthunte, Shesterhunte, and Cestrehunt; and Norden, (in his description of Hertfordshire, p. 15), says, Cur non Cheꞅꞇin? Castanetum of Chesse-nut trees?

The Saxons were well acquainted with this tree, and, according to Skinner and Lye, called it Cyꞅꞇel-and Cyꞅꞇ-beam; the same word evidently with our present Chest-nut. Dr. Johnson, in his Mercurius Botanicus, 1634, remarks the chestnut to have been not unfrequent in the woods, as well as in the plantations, of his own times; Castanea Vulgaris in sylvis nonnullis et viridariis;— Mr. Dale, in his History of Harwich, mentions various chestnut trees to be growing in Stour wood, within the parish immediately adjoining to Harwich. Blackstone, in his Specimen Botanicum, p. 12, speaks of chestnut trees growing in Bulwin woods, between Dartford and Bexley, in Kent, plentifully; not 20 miles distant from London. Mr. Philipot, in his Villare Cantianum, which was printed in 1659, says in p. 237, "There is a manor called Northwood Chasteners, which name complies with the situation; for it stands north from the town, in a wood where chestnut trees formerly grew in abundance." "The noble chestnut tree, says Morton, (Northamptonshire, p. 397), belonging to the worshipful Thomas Tryst, Esq. of Marford, is the largest of that kind I have any where seen: the body of it is no less than 15 feet 8 inches in circumference; and it extends its branches proportionably." On the outside of the Roman station at Temple Brough, near Sheffield, in Yorkshire, says Gibson's Camden, (vol. 2, p. 847), "is a large bank, on which are huge trees, and on the side of the bank of the highway, there grew a chestnut tree that had scarcely any bark on it, but only on some top branches which bore leaves; it was not tall, but the bole could scarcely be fathomed by 3 men." "There was standing, says Evelyn, (in his Sylva, Fol. London, 1706, p. 223), an old and decayed chestnut at Fraiting, in Essex, whose very stump did yield 30 sizeable loads of logs. I could produce you another of the same kind in Gloucestershire, which contains within the bowels of it, a pretty wainscoted room, enlightened with windows, and furnished with seats, &c." And to these we may add 2 great chestnut trees flourishing at Tortworth, in Gloucestershire, and at Writtlepark, in Essex; the former is allowed, even by Mr. Barrington, "to be the oldest tree that we have

any account of, perhaps in Europe." And the following description of both, was published about 12 or 13 years ago; "At the seat of the Lord Ducie, at Tortworth, in Gloucestershire, there is now growing an English chestnut, which measures 51 feet about, at the height of 6 feet above the ground. This tree divides itself, at the crown, into 3 limbs, one of which measures 28 feet and half in the girt, and 5 feet above the crown of the tree. The soil is a soft clay, somewhat loomy; the situation is the N. W. side of a hill; this tree was stiled, in King John's time, the great and old chestnut tree at Torteworth; so it is supposed to be now above one thousand years old.

"There is another stately chestnut, but little inferior to that at Torteworth, in Writtle park, 3 miles to the left of Ingatestone, in Essex. The late Lord Petre measured this tree, and found it 45 feet girth, 5 feet from the ground; this vast trunk supports a lofty head, which, at a distance, affords a noble prospect, and well deserves to be surveyed by all that admire such wonderful productions." At Little Wymondley, near Hitchin, in Hertfordshire, is an old decayed chestnut tree, the trunk whereof (measured within these 2 years) was found to be 42 feet circumference in one part, and 48 feet in another, as I am credibly informed. And, to give additional force to an argument which is already decisive of itself, we may observe, that in the New Forest, there are very many chestnuts irregularly scattered among the oaks and other trees; and now to be seen in the road from Limington to Southampton.

In this great abundance of chestnut trees formerly among us, we need not wonder that chestnut timber was frequently used in old houses, preferably to oak; it was then the timber most esteemed by our joiners and carpenters. And, though very lasting, yet it has been justly discredited, in these latter ages, for houses, because, when it begins to decay, the consumption commences at the core, and the heart is the first destroyed. And we can produce some proofs, additional to the many that have been formerly produced, of chestnut timber actually employed in buildings. "The old houses in the city of Gloucester, (as the Reverend Mr. Crawley informs me that he has often been assured) are constructed of chestnut, derived assuredly from the chestnut trees in the forest of Dean." In many of the oldest houses at Feversham is much genuine chestnut, as well as oak, employed. In the nunnery of Davington, near Feversham, (now entire) the timber consists of oak intermingled with chestnut. And the great chestnut beam which supported the leads of the church tower at Feversham, when it was lately taken down, was found rotted for many feet at the extremity; and had, as it were, a mere shell of sound timber remaining about it.

Thus have I endeavoured, with all the respect due to genius and truth, to point out some of the mistakes into which, I apprehend, Mr. Barrington has fallen. I might have dwelt more largely on the antiquarian part of my subject;

but the botanical was more immediately my point. And in the examination of this, I have shown, that the chestnut tree flourishes greatly in this kingdom; that it appears wildly scattered over the face of the country; that it was actually settled among us many centuries ago; and used by our ancestors in buildings; and that it was even familiarly known to the Saxons. All these united evidences strongly co-operate to prove it a native of this island, and must absolutely be allowed to prove it, till Mr. Barrington, or some other person, can produce superior evidence to the contrary.

XVIII. Copy of Mr. Thorpe's Letter to Dr. Ducarel, concerning Chestnut Trees. p. 152.

XIX. Extract of a Letter from Edw. Hasted, Esq., F. R. S., and F. S. A., to Dr. Ducarel, concerning Chestnut Trees. p. 160.

These last two extracts contain the particulars of several notices referred to in Mr. Ducarel's letter preceding them; all tending to show, that there have, many ages past, been large quantities of chestnut trees in this country; and that many are still remaining in different parts. But all this is no proof of their being indigenous: and all the circumstances of their former prevalence, and later decline, could be easily explained without that supposition.

XX. A Letter from the Hon. Daines Barrington; occasioned by the three preceding Letters. p. 167.

I have lately had an opportunity of perusing 3 letters from Dr. Ducarel, Mr. Thorpe, and Mr. Hasted, which contend that the sweet chestnut is an indigenous tree of this country. As I do not see any reason for altering the opinions which I have happened to form on this subject, from what is contained in these 3 letters, I should not trouble the society with any answer to the contents of them, did not Mr. Thorpe contradict, on the testimony of another person, what I have asserted I was an ocular witness of. I must therefore a second time repeat, that the chestnut woods near Newington, in Kent, are planted in rows at 4 or 5 yards distance (other trees often intervening); and for a proof of this fact, I refer Mr. Thorpe to the woods on the N. E. of the church, or at least they are very near to it; as also the wood to the eastward of the great road to Canterbury, immediately after you leave the town of Newington. I spent very near a whole day in the examination of these woods; but I would more particularly refer to the two chestnut plantations above specified, as they were just then shooting from the stools, when I took this very minute view of them.

I believe I may say, that I have been almost in every corner of the 12 Welsh counties; and never saw a beech tree in any of them, which had the least

pretence to be indigenous. I will suppose, however, that a wood of any given number of acres, with beech in it, was found in the central part of the principality; and that these trees were not planted in rows (as at Newington and Sittingbourne); but dispersed, as happens in other indigenous woods. Could it possibly be contended, that such beech trees had not been introduced by some planter; notwithstanding it might be proved to be a wood of great antiquity? If this was insisted on, it must at the same time be conceived, that when the beech mast was wafted by the wind to such a most selected spot, some preternatural cause must have prevented its being sown in any intermediate place.

XXI. An Account of the Nyl-ghau, an Indian Animal, not hitherto described. By Wm. Hunter, M. D., F. R. S.* p. 170.

Among the riches which of late years have been imported from India, may be reckoned a fine animal, the nyl-ghau; which it is to be hoped will now be propagated in this country, so as to become one of the most useful, or at least one of the most ornamental beasts of the field. It is larger than any ruminant of this country, except the ox; its flesh probably will be found to be delicious; and if it should prove docile enough to be easily trained to labour, its great swiftness, with considerable strength, might be applied probably to valuable purposes.

Good paintings of animals give much clearer ideas than descriptions. Whoever looks at the picture, which was done under Dr. H.'s eye, by Mr. Stubbs, that excellent painter of animals, (see fig. 2, pl. 3) can never be at a loss to know the nyl-ghau, wherever he may happen to meet with it.

At first sight, the male nyl-ghau appeared to be of a middle nature, between black cattle and deer; such an animal as we might suppose a mule would be, that was the produce of those two species of beasts. In size, it is as much smaller than the one, as it is larger than the other: and in its form there is a very apparent mixture of resemblance to both. Its body, horns, and tail, are not unlike those of a bull; and the head, neck, and legs, are very like those of deer.

Colour. The colour in general is ash, or grey, from a mixture of black hairs and white: most of the hairs are half white and half black; the white part is towards the root. The colour of its legs is darker than that of its body; the same thing may be said of its head, with this peculiarity, that there the darker colour is not general and uniform, but some parts are almost quite black. In some parts, to be mentioned hereafter, the hair is of a beautiful white colour.

Trunk. The height of the back, where there is a slight eminence over the

* This animal is the *Antilope picta* of the Gmelinian edition of the Systema Naturæ, and the *White-footed Antilope* of Pennant. It has been often imported into Europe of late years, and has bred in England. The figure here given is very good. A good representation both of male and female may also be found in the sixth supplemental volume of the Natural History of the Count de Buffon.

shoulder-blade, is 4 feet and 1 inch; at the highest part, immediately behind the loins, it is only 4 feet. The general length of the trunk, as seen in a side view, from the root of the neck to the pendulous tail, is about 4 feet; which is nearly the height of the animal; so that in a side view, when it stands with its legs parallel, its back and limbs make nearly 3 sides of a square, and the ground on which it stands makes the 4th. Round the body, immediately behind the shoulder, it measures 4 feet 10 inches; and a little more just before the hind-legs; but this last dimension no doubt will vary considerably, as it happens to be more full or empty of food and drink.

There are six grinders in each side of each jaw, and 4 incisor teeth in each half of the lower jaw. The first of the incisors is very broad; and the rest smaller in gradation, as they are placed more outwards or backwards. It eats oats, but not greedily; is fonder of grass and hay;* but is always delighted with wheat bread. When thirsty, it would drink 2 gallons of water. Its dung is in the form of small round balls, of the size of a nutmeg; and it passes a quantity of these together, with a rushing sound. Though it was reported to have been exceedingly vicious, it was in reality a most gentle creatare while in Dr. H.'s custody; seemed pleased with every kind of familiarity, always licked the hand which either stroked, or gave it bread, and never once attempted to use its horns offensively. It seemed to have much dependence on its organs of smell, and snuffed keenly, and with noise, whenever any person came within sight. It did so likewise when any food or drink was brought to it; and was so easily offended with a smell, or so cautious, that it would not taste the bread which he offered, when his hand had touched oil of turpentine or spirits.†

Its manner of fighting is very particular: it was observed at Lord Clive's, where 2 males were put into a little inclosure; and it was related to Dr. H. by his lordship thus: while they were at a considerable distance from each other, they prepared for the attack, by falling down upon their fore-knees; then they shuffled towards each other with a quick pace, keeping still upon their fore-knees, and when they were come within some yards, they made a spring, and darted against each other. All the time that 2 of them were in my stable, I observed this particularly, viz. that whenever any attempt was made upon them, they imme-

* General Carnac said, that no hay is made in India; their horses are fed with grass fresh cut, and a grain of the pulse kind, called gram.—Orig.

† General Carnac, in some observations which he favoured Dr. H. with on this subject, says, 'All of the deer kind have the sense of smelling very exquisite. I have frequently observed of tame deer, to whom bread is often given, and which they are in general fond of, that if you present them a piece that has been bitten, they will not touch it. I have made the same observation of a remarkable fine she-goat, which accompanied me most of my campaigns in India; and supplied me with milk, and which, in gratitude for her services, I brought from abroad with me.'—Orig.

diately fell down upon their fore-knees; and sometimes they would do so when I came before them; but as they never darted, I so little thought this posture meant hostility, that I rather supposed it expressive of a timid or obsequious humility.*

The female differs so much from the male, that we should scarcely suppose them to be the same species. She is much smaller, both in height and thickness. In her shape, and in her yellowish colour, she very much resembles deer; and has no horns. She has 4 nipples, and is supposed to go 9 months with young. She commonly has one at a birth, and sometimes twins. The young male nyl-ghau is like the female in colour, and therefore like a fawn.

Of late years several of this species, both male and female, have been brought to England. The first were sent from Bombay, by Governor Cromelen, as a present to Lord Clive: they arrived in August 1767. They were male and female, and continue to breed every year. Afterwards two were brought over, and presented to the queen by Mr. Sullivan. From her majesty's desire to encourage every useful or curious inquiry in natural knowledge, Dr. H. was permitted to keep these 2 for some time; which enabled him to describe them, and to get a correct picture made; and with his brother's assistance to dissect the dead animal, and preserve the skin and skeleton. At all the places in India, where we have settlements, they are rarities, brought from the distant interior parts of the country, as presents to nabobs and great men. Lord Clive, General Carnac, Mr. Walsh, Mr. Watts, and many other gentlemen, who have seen much of India, say they never saw them wild. So far as Dr. H. has yet found, Bernier is the only author who has even mentioned them.† In the 4th vol. of his Memoirs, he gives an account of a journey which he undertook, ann. 1664, from Delhi to the province of Cachemire, with the mogul Aurengzeb, who went to that terrestrial paradise, as it is esteemed by the Indians, to avoid the heat of the sum-

* The intrepidity and force with which they dart against any object, may be conceived from the following anecdote, of the finest and largest of those animals that has ever been seen in England. The violence which he did to himself was supposed to occasion his death, which happened soon after. A poor labouring man, without knowing that the animal was near him, and therefore neither meaning to offend, nor suspecting the danger, came up near to the outside of the pales of the inclosure; the nyl-ghau, with the quickness of lightning, darted against the wood-work, with such violence, that he broke it to pieces; and broke off one of his horns close to the root. From this piece of history and further inquiry, I was satisfied that the animal is vicious and fierce in the rutting season, however gentle and tame at other times.—Orig.

† Since the reading of this paper, I have received the following information from Dr. Maty. In the 4th vol. of Valentyn's description of the East Indies, published in Low Dutch, 1727, under the article of Batavia, p. 231, I find among the uncommon animals kept at the castle, this short indication, 'There was a beast, of the size and colour of a Danish ox, but less heavy, pointed towards the mouth, ash-grey, and not less than an elk, whose name he bore.' It was a present from the mogul.—Orig.

mer. In giving an account of the hunting, which was the emperor's amusement in this journey, he describes, among others, that of le nyl-ghau; but without saying more of the animal, than that the emperor sometimes kills them in such numbers, as to distribute quarters of them to all his omrachs; which shows that they were there wild, and in plenty, and esteemed good or delicious food. This agrees with the rarity of these animals at Bengal, Madras, and Bombay: for Cachemire is the most northern province of the empire; and it was on the march from Delhi to that place that Bernier saw the emperor hunt them.

The word nyl-ghau, for these are the component letters corresponding to the Persian, though pronounced as if it were written neel-gaw, signifies a blue cow, or rather a bull, gaw being masculine; and the male animal of that name has a good title to the appellation, as well from the likeness he bears in some parts to that species of cattle; as from the bluish tinct which is very discernible in the colour of his body; but this is by no means the case with the female, which has a near resemblance, as well in colour as in form, to our red deer. The nyl-ghaus which have been brought to England have been most, if not all, of them received from Surat or Bombay; and they seem to be less uncommon in that part of India than in Bengal; which gives room for a conjecture that they may be indigenous perhaps in the province of Guzarat, one of the most western and most considerable of the Indoostan empire, lying to the northward of Surat, and stretching away to the Indian ocean.

XXII. Observations on the Aphides of Linnæus. By Dr. Wm. Richardson, of Ripon, Yorkshire. p. 182.

The learned Linnæus, by his unwearied application, having reduced the various productions of nature into one regular system, and clearly distinguished the numerous tribe of insects into their distinct classes and subdivisions, seems to have laid a more solid foundation for the natural history of these minute animals, than any other writer who has gone before him. Difficult however as it is to lay so firm a foundation, the superstructure must still be esteemed a more arduous undertaking; as it is easier to distinguish the outer form, even of the minutest insects, than to discover their internal nature and disposition. This is a knowledge not to be attained by any single person, be his genius and diligence ever so great; but to bring it to any degree of perfection, will require the joint endeavours of the curious in all ages, and in all the different parts of the world. From which considerations, Dr. R. is induced to throw in his mite towards promoting so useful an undertaking; by reducing his orbservations on this surprizing kind of insect, into a more concise and regular form.

Though the aphides are distinguished by Linnæus into more than 30 species; still Dr. R. is satisfied, from his own observation, that the distinct species are

even double that number: nor can he altogether agree with this ingenious author, that there are a greater variety of plants producing aphides, than there are different sorts of this insect. Where plants are of a like nature, they are usually frequented by the same insects; but many of these plants will be found to support two or more quite different sorts. On the peach and nectarine indeed the aphides are the same, and he did not find on these trees more than one sort. The plum-tree, on the other hand, has 2 sorts, very distinct from each other; one of a yellowish green, with a round short body; the other of a blueish green, as it were enamelled with white, and the shape more oblong. On the gooseberry-bush and currant the same aphides may be found; but each of these is inhabited by 2 very different species; one being of a dusky green, with a short plump body; the other of a paler green, the body more taper, and transversely wrinkled. To these instances he further adds, that the rose-tree supports not less than 3 distinct species: the largest of which is of a deep green, having long legs of a brownish cast, with the joints of a very dark brown, as are also the horns and antennæ; a 2d sort is paler green, has much shorter legs and a more flat body; the 3d sort is of a pale red, its body transversely wrinkled, and is most frequently on the sweetbriar.

The great variety of species, which occur in the insects now under consideration, may indeed make an inquiry into their particular natures seem not a little intricate and perplexed; having them however skilfully reduced under their proper genus, the difficulty is by this means considerably diminished. All the insects comprehended under any distinct genus, we may reasonably suppose to partake of one general nature; and by diligently examining any of the particular species, may thence gain some insight into the nature of all the rest. With this view Dr. R. has chosen, out of the various sorts of aphides, the largest of those found on the rose-tree; not only as its size makes it the more conspicuous, but as there are few others of so long a duration. This sort, appearing early in the spring, continues late in the autumn; while several are limited to a much shorter term, in conformity to the different trees and plants whence they draw their nourishment.

Sect. 1. If, at the beginning of February, the weather happens to be so warm, as to make the buds of the rose-tree swell and appear green; small aphides are frequently to be found upon them, not larger than the young ones in summer, when first produced. But there being no old ones to be found at this time of the year, which in summer he had observed to be viviparous; he was formerly not a little perplexed by such different appearances, and was almost induced to give credit to the old doctrine of equivocal generation. That the same kind of animal should at one time of the year be viviparous, and at another oviparous, was an opinion he could then by no means entertain. This however frequent observation has at last convinced him to be fact; having found those aphides,

which appear early in the spring, to proceed from small black oval eggs, which were deposited on the last year's shoots in autumn: though when it happens that those insects make too early an appearance, he had observed the greatest part to suffer from the sharp weather that usually succeeds; by which means the rose trees are some years in a manner freed from them.

Those which withstand the severity of the weather seldom come to their full growth before the month of April; at which time they usually begin to breed, after twice casting off their exuviæ, or outer covering. It then appears that they are all females, which produce each of them a very numerous progeny, and that without having intercourse with any male insect. As before observed, they are viviparous; and what is equally uncommon, the young ones all come into the world backwards. When they first come from the parent, they are enveloped by a thin membrane, having in this situation the appearance of an oval egg; which probably induced Reaumur to suspect that the eggs discovered by Bennet, were nothing more than abortions. This egg-like appearance adheres by one extremity to the mother, while the young one contained extends the other; by that means gradually drawing the ruptured membrane, over the head and body, to the hind feet. During this operation, and for some time after, by means of something glutinous, the fore part of the head adheres to the vent of the parent. Being thus suspended in the air, it soon frees itself from the membrane in which it was confined, and after its limbs are a little strengthened, is set down on some tender shoot, and then left to provide for itself.

When the spring proves mild, and consequently favourable to this kind of insect, Dr. R. has observed not only the rose-trees, but various kinds of fruit-trees, to be greatly injured by them. Hence he was first induced to investigate the nature of these insects; in order to find out some expedient, by which so great an evil might be prevented. To avoid being tedious by descending to particulars, he recommends the following general rule; viz. to lop off the infected shoots before these insects are greatly multiplied; repeating the same operation before the time their eggs are deposited. By the first pruning, you will prevent a very numerous present increase; and by the second, may entirely cut off the next year's supply.

Sect. 2. In the spring months, there appear on the rose tree only 2 generations of aphides, including those which immediately proceed from the last year's eggs; the warmth of the summer adds so much to their fertility, that no less than 5 generations succeed each other during that interval. One is produced in May, which twice casts off its covering; while the months of June and July each supply 2 more, which cast off their coverings 3 or 4 times, according to the different warmth of the season. This frequent change of the outer covering is the more extraordinary, as it is the oftenest repeated when the insects come the

soonest to their growth; which Dr. R. has sometimes observed to happen in 10 days, where warmth and plenty of nourishment have mutually conspired. From which considerations, he is thoroughly convinced, that these various coverings are not connate with the insect; but that they are, like the scarf-skin, successively produced.

Early in the month of June, some of the 3d generation, which were produced about the middle of May, after casting off their last covering, discover 4 erect wings, much longer than their bodies: and the same is observable in all the succeeding generations, which are produced during the summer months; without however distinguishing any diversity of sex, as is usual in several other kinds of insects. For some time before the aphides come to their full growth, it is easy to discover which of them will have wings, by a remarkable fullness in the breast, which in the others is hardly to be distinguished from the body. When the last covering is rejected, the wings, which were before folded up in a very narrow compass, gradually extend themselves in a most surprizing manner, till their dimensions are at last very considerable. But these winged ones have this further peculiarity, that the number of them does not seem so much to depend on their original structure, as on the quantity or quality of the nourishment with which they are supplied: it being frequently observable, that those on a succulent shoot have few or none with wings among them; while others of the same generation, on a less tender branch, are most of them winged: as if the first rudiments of the wings were composed in the former, while nature thought proper to expand them in the latter, that they might be more at liberty to supply their wants.

The increase of these insects in the summer time is so very great, that by wounding and exhausting the tender shoots, they would frequently suppress all vegetation, had they not many enemies which restrain them. To enumerate the variety of other insects, that in their worm and fly state are constantly destroying them, would exceed the bounds of the present design: there is one however so singular in the manner of executing its purpose, that he cannot pass it by without some further notice. This is a very small black ichneumon fly, with a slender body, and very long antennæ; which darts its pointed tail into the bodies of the aphides, at the same time depositing an egg in each. This egg produces a worm, which feeds on the containing insect, till it has acquired its full growth; when it is usually changed to that kind of fly from which it had its origin. In this however it is sometimes prevented by another sort of small black fly, which wounds this worm through its pearl-like habitation; and by laying one of its eggs in it, instead of the former fly, produces its own likeness.

Dr. R. however further observes that notwithstanding these insects have many enemies, they are not without friends; if we may consider those as such, who are very officious in their attendance, for the good things they expect to reap by

them. The ant and the bee are both of this kind, collecting the honey in which the aphides abound; but with this difference, that the ants are constant visitors, the bee only when flowers are scarce. To which he also adds, that the ants will suck in the delicious nectar, while the aphides are in the act of discharging it from the anus; but the bees only collect it from the leaves on which this honey-dew has fallen.

Sect. 3.—In the autumn, Dr. R. finds 3 more generations of aphides to be produced; 2 of which make their appearance in the month of August, and the 3d usually before the middle of September. As the first 2 differ in no respect from those which are met with in summer, it would be wasting time to dwell any longer on them; but the 3d, differing greatly from all the rest, demands our giving it a more serious attention. Though all the aphides which have hitherto appeared were females, in this 10th generation are found several male insects; not that they are by any means so numerous as the females, being only produced by a small part of the former generation. To which Dr. R. further adds, that he has observed those which produce males, previously to have produced a number of females, which in all respects resemble those already described.

The females have at first altogether the same appearance with those of the former generations; but in a few days their colour changes from a green to a yellow, which is gradually converted into an orange-colour, before they come to their full growth. They differ likewise in another respect, at least from those which occur in the summer, that all those yellow females are without wings. The male insects are however still more remarkable: their outward appearance readily distinguishing them from the females, of this and all other generations. When first produced they are not of a green colour, like the rest, but of a reddish brown; and have afterwards, when they begin to thicken about the breast, a dark line along the middle of the back. These male insects come to their full growth in about 3 weeks time, and then cast off their last covering; the whole insect being after this operation of a bright yellow, the wings only excepted. But they soon change to a darker yellow, and in a few hours to a very dark brown; if we except the body, which is something lighter coloured, and has a reddish cast. They are all of the winged sort; and the wings, which are white at first, soon become transparent, and at length appear like very fine black gauze.

The males no sooner come to maturity, than they copulate with the females; in which act they are readily discovered, as they remain in conjunction for a considerable time, and are not easily disturbed. The commerce between them continues the whole month of October, and may be observed at all times of the day; though he has found it most frequent about noon, especially when the weather is moderately warm, with the sun overcast. The females, in a day or

two after their intercourse with the males, are observed to lay their eggs; which they usually do near the buds, when they are left to their own choice. Where a number are crouded together, they of course interfere with eash other; in which case, they will frequently deposit their eggs on other parts of the branches, or even on the spines with which they are beset. He does not however find that the eggs produced by these insects bear any proportion to the number of young ones which proceed from the females of other generations; not having observed any one insect to produce more than 2 or 3, and that in appearance with great difficulty.

Having now traced their progress through the different seasons of the year, and observed the various metamorphoses which they successively undergo; Dr. R. cannot help suspecting the insufficiency of human reason, in setting any scheme to which the different changes of insects may be accurately reduced. Though the indefatigable Swammerdam seems to have been fully convinced that there is no insect, whose changes may not be reduced to one or other of the 4 orders he has described; still the insect now under consideration, having at different seasons quite different appearances, cannot, Dr. R. thinks, with strictness be confined to any of them. In the spring they seem in some measure to coincide with the first order, though in summer those with wings more properly belong to the 2d; but in autumn, the males may seem to come under one order, and the females under another; or, he rather thinks, these insects are not clearly reducible to any order.

Sect. 4.—Some of the insects now under consideration continuing to lay their eggs till the beginning of November, Dr. R. chose to defer giving a more particular account of them, till the season for which they seem by nature to have been designed. These eggs are of a regular oval figure, being about the 10th part of an inch in length, and the 20th in breadth; which, though it may seem a very inconsiderable bulk, is certainly large for so minute an insect. When they are first produced, their colour is green, but in a few days turns to brown, and by degrees becomes quite black. The covering of the eggs may be called thick, if compared with its small size, which at first is rather of a yielding nature; but, after being exposed to the air, soon contracts a greater firmness. If this covering is wounded, there issues forth a mucilaginous fluid, which is very transparent, and in appearance of a uniform consistence. These eggs adhere firmly to the branches on which they are deposited, by means of something glutinous with which they are besmeared, and in a most surprising manner resist all the severity of the winter.

Though Dr. R. has just observed that the contents of the eggs have the appearance of a uniform fluid; that this cannot in reality be the case, sufficiently appears from t e aphides they produce in the spring, without any other aid than

the warmth of the season. Nor is a single insect to be esteemed the whole product of an egg, since it has been cleaely shown, that 10 generations succeed each other; the first rudiments of which must have been originally in the egg, as the females have no communication with the males but in autumn. The wonder however becomes still greater, when we consider the number of individuals in each generation; this being, he is fully convinced, at a medium, not less than 50. Whoever pleases to multiply by 50, 9 times over, may by this means form some notion of the great number of insects produced from a single egg; but will at the same time find that number so immense, as to exceed all comprehension, and indeed to be little short of infinity. How far this can be reconciled with any theory of generation which the ingenuity of man has hitherto invented, may be a contemplation not altogether unworthy our curiosity, though he fears it will not turn out much to the credit of our reasoning faculties.

The ancient doctrine of equivocal generation, as also that from an admixtion of the seminal matter of both sexes, being now quite rejected by all modern naturalists; 2 other opinions seem to have sprung up in their stead. While one party asserts that the original organization of the fœtus exists in the ovary of the female, and that it is vivified by a subtile spirit in the spermatic fluid of the male; the other lays it down for a certainty, that the eggs of the female are only to be considered as a proper nidus, provided for the reception of those minute animalcules, with which the male semen is found to abound. As the former opinion does not appear to have any certain fact to support it, we may well suspect an insufficiency in the cause to produce the effect assigned; but, supposing it adequate to the production of one generation, who can conceive a subtile spirit to remain in force for 10 generations, and that through all the various seasons of the year? With regard to the latter, he must observe, that the animalcules of Leuwenhoeck being compared with Malpighi's first rudiments of the chick, their resemblance is not so striking as to afford the least conviction: but should we allow these animalcules requisite to produce the first generation, how then are the subsequent 9 generations produced without them? Not being able to answer these queries himself, nor expecting them to be readily answered by others; it seems most prudent to observe with diligence what nature does, without being over anxious to discover by what means. Let us rest satisfied in admiring the wonderful effects of generation, while we refer the primary efficient cause to the eternal will and power of an Almighty Creator.

XXIII. Meteorological Observations at Ludgvan in Mount's-Bay, Cornwall, 1770. *By William Borlase, D.D., F.R.S. Communicated by Dr. Jeremiah Milles, Dean of Exeter, and F.R.S.* p. 195.

This is another register of the weather, 2 times in each month of the year

1770, of the highest and lowest states of the barometer and thermometer, with the monthly quantity of rain, the sum of which for the whole year amounts to 44.64 inches.

XXIV. Description of a New Hygrometer. By Mr. J. Smeaton, F.R.S. p. 198.

Mr. S. having some years before attempted to make an accurate and sensible hygrometer, by means of a hempen cord, of a very considerable length; he quickly found, that though it was more than sufficiently susceptible of every change in the humidity of the atmosphere, yet the cord was, on the whole, in a continual state of lengthening. Though this change was the greatest at first, yet it did not appear probable that any given time would bring it to a certainty; and it also seemed, that as the cord grew more determinate in mean length, the alteration by certain differences of moisture became less. Now as, on considering wood, paper, catgut, &c. there did not appear to be a likelihood of finding any substance sufficiently sensible of differences of moisture, that would be unalterable under the same degrees of it; this led him to consider of a construction which would readily admit of an adjustment; so that, though the cord by which the instrument is actuated may be variable in itself, both as to absolute length, and difference of length under given degrees of moisture, yet that, on supposition of a material departure from its original scale, it might be readily restored to it, and in consequence that any numbers of hygrometers, similarly constructed, might, like thermometers, be capable of speaking the same language.

The 2 points of heat the more readily determinable in a thermometer, are the points of freezing and boiling water. In like manner, to construct hygrometers which shall be capable of agreement, it is necessary to establish 2 different degrees of a moisture which shall be as fixed in themselves, and to which we can as readily and as often have recourse as possible. One point is given by making the substance perfectly wet, which seems sufficiently determinable; the other is that of perfect dry; but which he does not apprehend to be attainable with the same precision. A readiness to imbibe wet, so that the substance may be soon and fully saturated, and also a facility of parting with its moisture, on being exposed to the fire to dry; at the same time that neither immersion in water, nor a moderate exposition to the warmth of the fire, shall injure its texture; are properties requisite to the first mover of such an hygrometer, that in a manner exclude all substances that he was acquainted with, besides hempen and flaxen threads or cords, and what are compounded of them.

On these ideas, in the year 1758, Mr. S. constructed 2 hygrometers, as nearly alike as possible, that he might have the means of examining their agreement or disagreement on similar or dissimilar treatment. The interval or scale between dry and wet, he divided into 100 equal parts, which he calls the degrees of this

hygrometer. The point of 0 denotes perfect dry; and the numbers increase with the degrees of moisture to 100, which denotes perfect wet.

On comparing them for some time, when hung up near together in a passage or stair-case, where they would be very little affected by fire, and where they would be exposed to as free an air as possible in the inside of the house, he found that they generally were within one degree, and very rarely differed 2 degrees; but, as these comparisons necessarily took up some time, and were frequently interrupted by long avocations from home, it was some years before he could form a tolerable judgment on them. One thing he soon observed, not altogether to his liking; which was, that the flaxen cords which he made use of, seemed to make so much resistance to the entry of small degrees of moisture, such as is commonly experienced within doors in the situation above-mentioned, that all the changes were comprised within the first 30 degrees of the scale; but yet, on exposing them to the warm steam of a wash-house, the index quickly mounted to 100. He was therefore desirous of impregnating the cords with something of a saline nature, which should dispose them more forcibly to attract moisture; in order, that the index might, with the ordinary changes of moisture in the atmosphere, travel over a greater part of the scale of 100: how to do this in a regular and fixed quantity, was the subject of many experiments, and several years interrupted inquiry. At last, he tried the one hereafter described, which seemed to answer his intentions in a great measure; and though, on the whole, it does not appear likely, that this instrument will ever be made capable of so accurate an agreement, as mercurial thermometers are made to be; yet if we can reduce all the disagreements of an hygrometer within $\frac{1}{40}$ part of the whole scale, it will probably be of use in some philosophical inquiries, instead of instruments which have not as yet been reduced to any common scale at all.

The description of the hygrometer, as to its mechanical structure, is then given, but is not necessary to be here retained.

The cord here made use of is of flax, and between $\frac{1}{20}$ and $\frac{1}{30}$ of an inch in diameter; which can readily be ascertained by measuring a number of turns made round a pencil or small stick. It is a sort of cord used in London for making nets, and is of that particular kind called by net makers, "flaxen three threads laid." Mr. S. does not imagine that the fabric of the cord is of the most material consequence; but yet he supposes, when cords can be had of similar fabric, and nearly of the same size, that some small sources of variations will be avoided. In general, he thinks, that the more cords are twisted, the more they vary by different degrees of moisture, and the less we are certain of their absolute length; therefore those moderately twisted, he supposes are likely to answer best.

A competent quantity of this cord was boiled in one pound avoirdupois of

water, in which was put 2 dwts. troy of common salt; the whole was reduced by boiling to 6⅜ avoirdupois, which was done in about half an hour. As this ascertains a given strength of brine on taking out the cord, it may be supposed that every fibre of the cord is equally impregnated with salt. The cord being dried, it will be proper to stretch it; which may be done so as to prevent it from untwisting, by tying 3 or 4 yards to 2 nails, against a wall, in a horizontal position, and hanging a weight of a pound or 2 to the middle, so as to make it form an obtuse angle. This done for a week or more in a room, will lay the fibres of the cord close together, and prevent its stretching so fast after being applied to the instrument, as it otherwise would be apt to do.

Adjustment of the hygrometer.—The box cover being taken off, to prevent its being spoiled by fire, and chusing a day naturally dry, set the instrument nearly upright, about a yard from a moderate fire; so that the cord may become dry, and the instrument warm, but not so near as would spoil the finest linen by too much heat, and yet fully evaporate the moisture; there let the instrument stay, till the index is got as low as it will go, now and then stroking the cord between the thumb and finger downwards, in order to lay the fibres close together, and thereby causing it to lengthen as much as possible; when the index is thus become stationary, which will generally happen in about an hour (more or less as the air is naturally more or less dry) by means of the peg at top raise or depress the index, till it lays over the point 0; this done, remove the instrument from the fire, and having ready some warm water in a teacup, take a middling camel's hair pencil, and dipping it in the water, gently anoint the cord, till it will drink up no more, and till the index becomes stationary, and water will no more have effect on it, which will also generally happen in about an hour. If in this state the index lays over the degree marked 100, all is right; if not, slack the screw, and slide the scale nearer to or farther from the centre, till the point 100 comes under the index, and then the instrument is adjusted for use; but, if the compass of the slide is not sufficient to effect this, as may probably happen on the first adjustment, slack the proper screws, and move the sliding stud nearer to or farther from the centre of the index, according as the angle formed by the index, between the points of dry and wet, happens to be too small or too large for the scale; the quantity can easily be judged of, so as the next time to come within the compass of the slide of the scale; the quantity of slide being $\frac{1}{3}$ of the length of the index, and consequently its compass of adjustment $\frac{1}{3}$ of the whole variable quantity. Now as sliding the stud will vary the position of the index respecting the point of 0, this movement is only to be considered as a rough or preparatory adjustment, to bring it within the compass of the slide of the scale; which will not often happen to be necessary after the first time;

but in this case, the adjustment must be repeated in the same manner, by drying and wetting as before-described.

It is to be remarked that, as the cord is supposed impregnated in a given degree with common salt, and this not liable to evaporate, care must be taken in wetting, that no drops of wet be suffered to fall from the cord, to preserve the original quantity in the cord.

It appears from many observations, that in the compass of 11 months, the cords had stretched the value of 5°: and he also observed that they both had contracted their compass about 10°. Mr. S. would therefore recommend, that an hygrometer should from its first adjustment, be re-adjusted at the end of 3 months, and again at the end of 6 months from the first; after that, at the interval of about 6 months, to the end of 2 years from the beginning; and after that he apprehends that once a year will suffice. The best time of adjustment, being in the dry and warm weather of July or August: and by these means, he apprehends the instrument will be always kept within 2° of its proper point.

Mr. S. is aware that an hygrometer actuated by any principle of the kind here made use of, may not be a measurer of the quantity of moisture, actually dissolved in, and intimately mixed with the air; but only indicates the disposition of the air to part with, or precipitate the water contained in its substance; or, on the contrary, to dissolve and imbibe a greater quantity: but as it is by separating the effects of natural causes, that we are enabled to judge of these causes, and thence their effects when again compounded; every attempt to ascertain the operations of a simple cause will have its value in the search into nature: nor can we a priori determine the value of any new instrument; for if it should lead to a single discovery, or even to ascertain a single fact, this may again lead to others of great importance, of which we might have, either none, or an imperfect idea of before. For his own part, he has always looked on a thick fog, and the sweating, or condensation of the water's vapours on the walls in the inside of buildings, to be the greatest marks of a moist atmosphere; whereas he has not always found the hygrometer affected at these times in the highest degree. On the contrary, at the close of a fine day, and the fall of the dew on the sudden approach of a frost, he has found the hygrometer more affected by moisture, than in some of the preceding cases; and still more by a falling dew in the time of a hard frost.

XXV. Letter from Mr. John Baptist Beccaria, of Turin, F.R.S., to Mr. J. Canton, F.R.S., on his New Phosphorus receiving Several Colours, and only Emitting the Same. From the Latin. p. 212.

Mr. B. made several cylindrical boxes of thin iron plates, black withinside; and covered with a lid; in which was inserted a glass of any colour different from

that of the box. Into every box he put similar bits of the phosphorus. These inclosed bits were exposed to the sun altogether; then taking the boxes into the dark, and opening them, he saw that the piece of phosphorus was green, which was imbued with the light through the green glass; but red, through the red glass; and yellow, through the yellow glass: that is, it appears by this experiment that the phosphorus emits not only the light imbibed, but each its own light also.

XXVI. Remarks on the Effects of the late Cold in February last: in a Letter from the Rev. R. Watson, Professor of Chemistry at Cambridge. p. 213.

Reprinted in Bishop Watson's Chemical Essays.

XXVII. A Letter from Thomas Barker, Esq., of Lyndon in Rutlandshire, concerning Observations of the Quantities of Rain fallen at that Place for several Years. p. 221.

Subjoined is the quantity of rain, which has fallen at Lyndon in Rutland, since May 1736, with a table of the mean rain in the first 4 or 5 years, and every 10 years since; which shows that there has been more rain in the latter part of this period than in the former. But the least 4 years were from 1740 to 43, little more than 16½ inches a year; and the greatest 4 years from 1767 to 70, above 25½ inches a year. For comparing of dry seasons and wet ones, Mr. B. has made a table of the 3 driest months, the 3 driest 2 months, 3, 4, &c. to 12 successive months; and a like table of wet ones: but as the years 1763, 68, 70, exceeded any others, he made another like table of them. No 3 months come up to the last quarter of 1770, 7½ inches of which came in 3 weeks, from Nov. 6 to 26; but 1763 and 68, were better than 70, except those 3 months: and in this country 63 was the wettest; yet he supposes 68 exceeded it in many places.

Feb. 12 last, the thermometer abroad was down at 4 of Fahrenheit's scale, which is lower than he had observed it in above 20 years past; the lowest he had before observed was 10½, Jan. 5, 1768.

It was remarkable, that as long as the wind continued N. E. the frost was moderate; when it turned S. W. it became very severe; and when the wind turned back into the east again, the frost went away. This looks as if the weather was severer southward than here; as he thinks was likewise the case in Feb. 1754, which was also a very cold season.

XXVIII. A second Letter from Mr. Barker, on the same Subject; together with the Determination of the Latitude of Stamford, in Lincolnshire. p. 227.

Mr. B.'s rain cistern has all along stood on the top of a wall, where another meets it at right angles. The top of the cistern on the north side, is 7 feet 3

inches; on the south-west side, 8 feet 6 inches; and on the south-east side, 10 feet above the ground; it is all open southward for 25 yards; the north side is an orchard, but no tree hangs over it.

The latitude of Mr. Neal's house, at the south end of St. Martin's, adjoining to Stamford in Lincolnshire, as taken by Mr. Edward Lawrence, in 1736, is 52° 39′ 0″. St. Mary's, the middle of Stamford, is half a mile farther north, therefore its latitude is 52° 39′ 30″.

XXIX. Observations on some Bivalve Insects, found in common Water. By Mr. Muller, of the new Acad. of Sciences in Bavaria, &c.* p. 230.

The name of bivalve is given only to those shell-fish, whose houses are composed of 2 parts, such as muscles and oysters. Few of these are to be met with in fresh water; whereas a vast number are inhabitants of the sea. Mr. M. was acquainted with no more than 4 different species, like the sea bivalve; they are found in the waters of Fridericksdal, near Copenhagen, and among them, one has hitherto escaped the researches of conchiliogists.

In return, nature has liberally stocked the same waters with small insects, much more perfect than the inhabitants of the sea-shells, and likewise provided with a double shell. It is sufficiently known, that muscles and oysters are animals extremely simple; since they want several of the more perfect organs, and consequently enjoy life in an incomplete manner. The want of eyes, arms, legs, &c. obliges them to lead an idle life, deprived of all the advantages which arise from sight and motion. Nature, from which they received an habitation sufficient to protect them from external injuries, seems to have fixed for life their abode to one dark spot. Our bivalve insects, on the contrary, by opening their two folding gates, enjoy both sight and motion, alternately dipping in the mud, and darting through their element the water; whenever they meet with bad com-

* The Linnæan genus *Monoculus*, to which the insects described with too much minuteness in this paper, belong, is of great extent, and the species differ so much in habit or general appearance from each other, that several genera might be instituted from the single Linnæan genus. This however is not necessary, though it has been done by Muller and others. It should however be observed that the title *Monoculus*, applied to this genus by Linnæus, was given on account of the close approximation of the eyes, which in some species appear single. It must therefore be confessed that Linnæus has somewhat unhappily distinguished the whole genus by a name which can only be applied with strict propriety to some particular species. Of those described in the present paper by Mr. Muller, the most common is the *Monoculus conchaceus* of Linnæus, a very frequent inhabitant of stagnant waters, and which has so much the appearance of an extremely minute muscle, that it has often been mistaken for such, by those who have attended to the shell alone, without considering the insect.

Mr. Muller's list of the Monoculi inhabiting the waters about Fridericksdal is here omitted, as, being unaccompanied by figures, it could have been of no service; but it may be necessary to add, that he has, since the publication of this paper, been the author of an admirable work, in which the several species observed by himself are excellently described and figured.

pany, they hide themselves in their shells, and shut up the valves, which force and distress attempt in vain to force open.

Mr. M. discovered several different species of these animals in the waters of Fridericksdal, one only of which is known to the naturalists. Mr. Baker, of the Royal Society of London, is the first who mentions it; he says, 'that the insect swims very fast; that it procures its nourishment by means of a whirlpool, which it raises in the water by means of its arms; that on meeting with a solid body, it stops itself by means of its feet; that on the slightest touch it shrinks into its shell; and lastly that it bears much resemblance to a bivalve shell-fish.' To this description he joins a figure, which though imperfect, represents the insect. Linnæus and Geoffroi call it the monoculus, and without taking notice that Mr. Baker knew it already, they observe that its antennæ are composed of small white threads; and that the shell is oblong, smooth, and greyish, round on one side, flat on the other, and nearly of the same size at each end. None of the above-mentioned writers have had the satisfaction of inspecting the inhabitant of the shell, which indeed is very difficult. Now as this insect bears a strong likeness to the new species, Mr. M. takes a view of both together.

These minute bivalves are found adhering to confervas. That now described, he found, in the autumn of 1768, at the bottom of a ditch of standing water. The transparency of the shell gave him an opportunity of examining the inhabitant; and the examination cleared up the doubt he had about its species. The new shell is a bivalve; white, smooth, shining, and transparent, without the least spot, hair, or down. Its figure is oblong, rounded at both ends, and the hinge somewhat sinuated at the opening, and convex at the sides, in such a manner as when seen out of water, it is very like the seeds of some plants; and this is common to all the species of this genus. The substance is coriaceous, or like hardened glue; thin and very brittle when dried. When seen by the microscope, some of them appear very like net-work. The valves are equal, a little broader at one end than at the other, and somewhat flatted at the slope; they are not however more elevated at the opening than at the hinge, but rather the contrary; for on the inside they show another edge, less elevated than that of the outside, and which becomes less and less towards the hinge.

The length of the shell is half a line, and its greater breadth above a quarter of a line. That species mentioned by the above writers is 3 times longer when at its full growth. It is hairy, though smooth to the naked eye, more indented at the slopes where the valves are projecting, and more depressed towards the hinge; it is opaque, and of a changeable colour. Some of these insects are of a light, and others of a dark green, marked with an oblique stripe lighter than the rest. Some of them are bright, and others grey and dirty; but the down with which

the shell is covered, and to which the dirt sticks, is only visible with the microscope. Mr. M. examined several of these, at different ages, and at different times of the year, and found them all rough; whereas every one of those of the new species is smooth. He calls this new species the white smooth bivalve, to distinguish it from another, the shell of which is white and rough; and from that of the above-mentioned authors, which he calls the sordid, in allusion to the dirty shell in which it is often found.

The smooth white bivalve has 5 capillary threads at each antenna, 4 of which are at top, and the 5th somewhat lower. The sordid appears to have 10 at each antenna; in several, the antennæ appear yellowish, and their basis seems to consist of 4 rings. It is by means of these antennæ, which are real fins, that the animal changes its position, from one place to another, being able to move them several ways; when it has a mind to move fast, they are first extended straightways, and appear like two bristles; in an instant the threads are unfolded, and the animal swims with great quickness. As for walking, it sometimes joins the threads, sometimes unfolds only a single one, and sometimes scatters them about all together; sometimes it bends them between the valves, which are opened towards the place of the eye; it often hides one or both of them under the breast between the 4 legs; these antennæ seem to afford as great an amusement to the animal, as they do to the spectators. At the place where the head joins the body, towards the border of the hinge of the shell, is perceived a little black spot, which is the animal's eye. This extraordinary situation of the organ of sight on the neck seems astonishing; every thing that is new is so, but the surprize arises only from the narrowness of our ideas.

Some aquatic insects have the eye in the forehead, others at the bottom, on the fore or back part of the head, at the side or under it; nay there are some whose head consists of the eye only. The breast jets out a good deal towards the opening of the shell, and constitutes the greater part of the animal's body. The feet, mouth, and little bristles are placed on it.

There are 4 feet, whose position resembles a good deal that of quadrupeds, only that their reciprocal bent is more marked. The two foremost are at the top of the breast, in the part where it appears most sloped. They are white, transparent, and jointed, bent towards the back legs, and terminated by two points in the shape of claws. The joints have very thin hair on the inferior part. The 2 hind legs are fixed to the lower part of the breast. They are longer than the fore legs. Each joint has a couple of small threads at the end, and each leg terminates in a claw somewhat lengthened; as to the rest, they are like the fore legs, and bend towards them. The bivalve insect makes use of its claws, not only to walk on the conferva, some parts of which are true labyrinths,

and others forests to him; but likewise to remove the dirt, to seize its prey, and to fasten itself to other animals of its kind, or to neighbouring bodies.

Under the breast, and near the fore feet, is a black spot, which is the insect's mouth; it is covered with a small transparent skin, which opens in the middle, and shows a couple of jaws, marked with a very black spot at the place where they join. Between these jaws hang very small white beards like those of the tipula; and above these again, there appears a small black transversal line. About the mouth there are several other little beards, somewhat in the shape of feet, which are constantly in motion.

The belly is almost as broad as the breast, but has scarcely above half its length. The breadth decreases towards the tail. When seen from before, the belly appears composed of two conical lobes, marked in the middle with a black circle. It moves alternately to, and retires from, the breast. The tail comes out between these two lobes; it is of the same length with the body, and consists of 2 straight white and transparent canals, which are joined together till towards the end, where they separate, and each terminates in 2 curved points. On the back of the insect, are likewise seen 2 large round bodies, which he takes to be the ovaria.

The several hypotheses of naturalists, on the formation of shells, are known; some will have them increase by intussusception, and others by juxtaposition. This latter opinion, which M. de Reaumur patronized, and which nature seemed to justify, became, in consequence, the most general; but if the friends of the other system were thought to lose their cause, it was only for want of observing with a sufficient degree of accuracy the operations of nature, whose variety would have furnished them with instances in their favour. Our bivalve insect offers one, which the desertion of the old shell and the formation of a new one, in proportion as the animal grows, put beyond a doubt. The fact itself appears, not only from the observation of empty shells of different sizes, which are to be met with in waters, and are nothing more than the spoils of our bivalve insects; but, from the singular good fortune Mr. M. had, in seeing one of the animals strip itself, entirely in his presence, of the membrane of its shell, and of the exterior parts of its body, and show itself at last absolutely renewed. The exuviæ both of the shell and the animal's body were transparent as the brightest crystal. The joints of the antennæ, the bristles, the feet, the smallest hairs, were more distinguishable than in the animal itself. How infinitely small are the organs, which, hid as it were in sheaths and cases, only become visible when they are magnified some thousands of times! and how many are there which escape the best microscope! In the clearest water that we drink, we can often see with the naked eye spoils of this insect, joined to those of its shell, floating along, like fine white cotton.

Mr. M. has observed that they lay eggs, but this does not prevent their being likewise viviparous: he has seen other species of monoculi, some of which had their ovaria full of eggs, and others of little live insects, which at times they hatched, and at others put down in the shell.

The sordid species is the most commonly met with; we find it all the year, even in the time of frost, from under which Mr. M. has often drawn it. It is found in all pure waters, and even in the little ditches which are exposed to be overflowed by the sea. He has preserved it from May to November, full of life and motion, in a glass of water, which he did not renew the whole time. The smooth white insect lives at the bottom of marshes, and pools, in which the conferva grows.

The knowledge of these insects has been almost entirely neglected, though in reality very interesting; not to speak of their wonderful make, the difference of their motion, and their singular mode of copulation, are worthy of our inquiries. Let it be sufficient to say, that we swallow them and their shells, either living or dead, both in our victuals and drink; so that Mr. H. would not be surprized, if some time or other they were found in our intestines, or in those of beasts, and several of our diseases attributed to them.

Explanation of the Bivalve Insect pl. iii.—Fig. 3. The smooth insect as it is naturally. Fig. 4. The same, seen through the magnifier. Fig. 5. The same magnified by the microscope. The transparent shell shows the inhabitant lying at its full length; with the antennæ, legs and tail, out of the valves; a the edges of the two valves; b the antennæ; c the eye; d the head; e the ovaria; f the fore legs; g the hind legs; h the tail; i the fore part of the breast, where the beards and mouth are placed; k the belly. Fig. 6. The sordid shell of its natural size. Fig. 7. The same, as seen through the glass. Fig. 8. The same, with the shell a little opened, and more magnified. a The rough shell; b the oblique stripe; c the antennæ; d the fore legs; e the hind legs; f the mouth and joints; g the tail. Fig. 9. The same, with the shell shut.

XXX. Of a Singular Fish, from the South Seas. By the Rev. Michael Tyson, p. 247.

This curious fish, preserved in spirits, was brought, by Commodore Byron, from the new-discovered islands in the South Sea. As there is the greatest reason to believe that it has never been figured or described by any author, and indeed never before seen in Europe, Mr. T. sends the following description and drawing of it, fig. 10, pl. 3. He could not count the branchiostegous rays, without really injuring the specimen, but there is no doubt of its being one of the perca genus of Linnæus. It is called by the Commodore the zebra fish, not knowing its proper name. The drawing is exactly measured from the real fish, and is in every part of the same size.

A Thoracic Fish.

Perca...........................

Chætodon.... ....

Head obtuse, naked in front. Mouth ascending, edged by fleshy lips, the lower jaw longer than the upper. Teeth in both jaws equal, chaffy, approximated. Suture of the jaws on each side oblique, and dentated. Gill-covers edged with serrate-ciliate spines. Nostrils single, round, margined. Body ovate, compressed, scaly. Fins scaly on the back, black on the margin, with fibres projecting beyond the rays. Dorsal fins 2, subunited: the first rounded, with 10 spiny rays, the second angulated, with 16 soft rays, the pectoral fins rounded, with 14 rays, the ventral with 6 rays, the anal angulated, with 14 rays, of which the first two are spiny, the candal rounded, with 18 rays. Colour grey, with 6 transverse black bands surrounding the whole fish: the first runs over the head beyond the eyes; the second over the margin of the gill-covers; the third, angular and oblique, goes between the first dorsal fin and the anus; the fourth goes straight from the union of the dorsal fins to the space behind the anus; the fifth is bowed between the second dorsal fin and the anal fin; and the sixth is nearly straight, at the base of the candal fin.

Perch........(Chætodon) with subunited dorsal fins, rounded tail, and ovate body, with 6 transverse black bands.

Notwithstanding Mr. Tyson's observation relative to the genus of this fish, it should, no doubt, be considered as a species, not of perch, but of chætodon.

XXXI. An Account of Elden Hole, in Derbyshire. By J. Lloyd, Esq., With some Observations upon it, by Edward King, Esq., F. R. S.,* p. 250.

Mr. L. having seen several accounts of the unfathomable depth of Elden-hole,

* Edward King, Esq., F. R. and A. S. S., died April 16, 1807, at 72 years of age. He was a native of Norfolk, of a good family, and in 1748 entered a fellow commoner in Clare-Hall, Cambridge, where for several years he most assiduously pursued his literary course. After leaving the university, being intended for the law, he entered of Lincoln's Inn, by which society he was called to the bar, and for some time he followed the profession with considerable success; till the decease of his father, when coming to the possession of an ample fortune, he quitted Westminster-Hall, and devoted himself to the quiet pursuits of learning, which he cultivated with great assiduity during the rest of his life. In his attendance on the circuit, he defended a lady against a faithless lover, and successfully offered her his hand. He was also for some time recorder of Lynn. Though Mr. K. was a writer of considerable eccentricity, he appears to have been a very pious and well-meaning character: writing on many subjects, without the appearance of seeing deeply or clearly into any one. His various lucubrations were the effect of assiduous reading; and, as he expressed it, intense thinking. Whatever opinions he imbibed, they were maintained with pertinacity: and he would contend with equal zeal for the genuineness of the correspondence between St. Paul and Seneca, and of the apocryphal books, as for the holy scriptures.

in Derbyshire, and being in that neighbourhood, he was inclined to make some inquiries about that noted place, of the adjoining inhabitants; who informed him that about 14 or 15 years before, the owner of the pasture in which this chasm is situated, having lost several cattle, had agreed with 2 men to fill it up; but finding no visible effects of their labour, after having spent some days in throwing down many loads of stones, they ventured to be let down into it, to see if their undertaking was practicable; when, on finding at the bottom a vast large cavern, they desisted from their work, as it would have been almost impossible to have

Besides several essays, of different kinds, in the collections of the Royal and Antiquarian Societies, Mr. King was also author of numerous other publications, on various subjects.

His first publication was, "An Essay on the English Constitution and Government," 8vo. 1767. In 1773 he published "A letter addressed to Dr. Hawkesworth, and humbly recommended to the perusal of the very learned Deists."

His next publication was, Observations on Ancient Castles, 1777 and 1783. In 1780 he published, Hymns to the Supreme Being; but without his name, which on this occasion he affected to conceal, but was the first to betray his own secret. In 1788 he produced a 4to volume, entitled "Morsels of Criticism; tending to illustrate some few passages in the Holy Scriptures on philosophical principles, and an enlarged view of things." Here it seems Mr. K. seriously amused himself with some almanac prognostications; also proofs that John the Baptist was an angel from Heaven, and the same who formerly appeared in the person of Elijah; that there will be a second appearance of Jesus Christ upon earth; that this globe is a kind of comet, which is continually tending towards the sun, and will at length approach so near as to be ignited by the solar rays or the elementary fluid of fire; and that the place of punishment allotted for wicked men, is the centre of the earth, which is the bottomless pit, &c. In 1784 he circulated, without his name, "Proposals for establishing at sea a Marine School, or Seminary for Seamen, as a means for improving the Marine Society, and of clearing the streets of the metropolis from vagabond youth, &c." The proposal was to fit up a man of war as a marine school. In 1793 he published "Considerations on the Utility of the National Debt, and on the present Alarming Crisis, &c." His chief remaining publications were, "Vestiges of Oxford Castle; or a small fragment of a work intended to be published speedily on the History of Antient Castles, and on the Progress of Architecture," 1796. And the same year, "Remarks concerning Stones, said to have fallen from the Clouds, both in these days and in Antient Times." The former of these merged in a large History of Antient Castles and the Progress of Architecture, entitled "Munimenta Antiqua", divided into British or Druidical Roman, British in imitation of Foreign Nations, and Saxon; of which the first volume came out in 1799, the 2d in 1802, and the 3d in 1804. Mr. Dutens holding a different opinion from Mr. K. about the antiquity of arches, the latter replied by an anticipation of part of his 4th volume. Mr. D. defended himself in a Supplement to his "Recherches sur les Voutes," &c. in 1795; and thus the dispute was arrested by Mr. King's death. In 1801 came out a 2d volume of the "Morsels of Criticism," and in 1798, "Remarks on the Signs of the Times;" in which, among other peculiarities, Mr. K. asserts the genuineness of the apocryphal 2d book of Esdras; also a Supplement to the same Remarks in 1799. But concerning these, Mr. K. met with a powerful antagonist in the late learned Bishop Horsley, in his "Critical Disquisitions on Isaiah xviii, in a letter to Mr. King. Many other particulars of this gentleman may be seen in the Gent. Magazine, vol. 77, p. 388; and in the Mon. Magazine, vol. 23, p. 566."

procured a sufficient quantity of stones to have filled it up. On inquiry of one of these men whether there were any damps at the bottom; and being assured in the negative; Mr. L. procured 2 ropes of 40 fathoms nearly in length, and 8 men to let him down.

For the first 20 yards Mr. L. was let down, he could assist himself with his hands and feet, as it was a kind of confined slope; but after that the rock jetted out into large irregular pieces, on all the 3 sides next him; and on that account he met with some difficulty in passing, for about the space of 10 yards more; at which depth the rope was moved at least 5 or 6 yards from the perpendicular. Thence down, the breadth was about 3 yards, and the length at least 5 or 6, through craggy irregular slits in the rock, which was rather dirty, and covered with a kind of moss, and pretty wet, till he came within about 12 or 14 yards of the bottom, and then the rock opened on the east side, and he swung, till he descended to the floor of the cave, where he perceived there was light enough came from the mouth of the pit, though at the distance of 62 perpendicular yards, to read any print. When at the bottom, he perceived that the cavern consisted of 2 parts; the first being a cave, in shape not much unlike that of an oven; and the latter, a vast dome of the form of the inside of a glass-house; with a small arched passage from the one to the other, through which a slope of loose stones, that have been thrown in from time to time, extends from the wall at the west side of the first dome, to almost the bottom of the 2d cave, or dome, with such an angle, that the farther end of the cave is lower by 25 yards, than the place where he first landed.

The diameter of this cavern may be nearly 50 yards: the top he could not trace with the eye; but he had reason to believe it extended to a vast height; for when nearly at the top of one of the incrusted rocks, at the height of about 20 yards, he could find no closure of the dome, though he then saw much farther than when he stood at the bottom.

As to the particular curiosities to be met with in the small cavern, they are not worth mentioning; indeed he did not meet there with any stalactitical incrustations whatever; but the wall consisted of rude and irregular fragments of rock. Among the singularities in the 2d cavern, he particularly observed the following; climbing up a few loose stones on the south side, he descended again, through a small slit, into a little cave, 4 yards long, and irregular, as to height not exceeding 2 yards; and the whole lined with a kind of sparkling stalactites, of a fine deep yellow colour, with some small stalactitical drops hanging from the roof. Facing the first entrance is a most noble column, of the same kind of incrustation, above 30 yards high: and proceeding on to the north, he came to a large stone, covered with the like matter; and under it was a hole, 2 yards deep,

lined with the same; whence sprung a rock consisting of vast solid round masses, like the former in colour, though not in figure, on which he easily ascended to the height of 20 yards, and got some fine pieces of stalactites, pendent from the cragged sides which joined this rock.

After this, proceeding forward, he came to another pile of incrustations, different from the two former, and much rougher: and which was not tinged with such a yellow, but rather with a brown colour; and at the top of this also is a small cavern, into which he went. The last thing he took notice of, was the vast drops of stalactites, hanging like icicles from every part of the vault; some of which were as large as a man's body, and at least 4 or 5 feet long. The greatest part of the walls of the large cavern was lined with incrustations, and they were of three kinds: the first, being the deep yellow stalactites; the 2d being a thin coating, like a kind of light stone-coloured varnish on the surface of the limestone, and which glittered exceedingly by the light of the candles; and the 3d being a sort of rough efflorescence, every minute shoot resembling a kind of rose-flower.

Having satisfied his curiosity with a view of this astonishing vault, he began to return. Fastening the rope to his body, he gave the signal to be drawn up; which he found to be a much more difficult and dangerous task than the descent, owing to his weight drawing the rope into clefts, between the fragments of the rock, which made it stick; and to his body jarring against the sides, which he could not possibly prevent with his hands. Another circumstance also increased the danger, which was, the rope loosening the stones over head, whose fall he every instant dreaded.

P. S. After writing the above, Mr. L. was informed, there was formerly the mouth of a second shaft in the floor of the great cavern, somewhere under the great heap of stones; and that it was covered up by the miners, at the time when so many loads were thrown in from the top. It is reported to have gone down a vast depth farther, and to have had water at the bottom; but he did not perceive any remaining appearance of such opening himself, nor did the miners, who went down with him, say any thing about it.

Remarks on the above. By Edw. King, Esq. p. 256.

After some remarks on part of Mr. Lloyd's description, Mr. K. adds, if it be further considered, that in sounding such great depths, the weight of the rope may often be mistaken for the weight of the plummet; and that hence the rope may continue descending, and coiling up, first at the bottom, and afterwards at other places where it is accidentally stopped, till it be at length hindered in its descent by some projections of the rock nearer the mouth of the shaft; this will account for Mr. Cotton's letting down 884 yards, while the water at the bottom

of the 2d shaft will account for 80 yards being wet; as so many might coil up in the water, let it have been ever so shallow, and as the rest, beyond the real depth of the chasm, might coil up either in the great or little cave. Again, the many craggs on each side the first shaft, and probably also on each side the 2d, must retard any stone in its fall; and by that means will account for the length of time a body takes in descending; which must be a great deal longer than if it fell in open space: and hence Dr. Short (who has given a calculation, formed from the time of the descent of heavy bodies, according to the Newtonian principles of gravitation) was misled to conclude, though very ingeniously, that this chasm was 422 yards deep. And lastly; the falling of stones into the water, at the bottom of the 2d shaft, and the increase of the sound thus made, partly from the reverberation at the sides of the great cavern, and partly from the form of the upper shaft, which is not very unlike that of a speaking trumpet, might occasion that astonishing noise, which is said to have been heard at various times formerly, on throwing stones into this gulf; but which has not been heard of late years, in a manner at all agreeable to old reports.

And as both Mr. Lloyd, and also a miner's wife, from whom K. had his information, mentioned there being water at the bottom of the 2d shaft, it appears highly probable, that this water is the continuation of a subterraneous river; and indeed of that very river which runs out of the mouth of the great cavern at Castleton: for it is observed by the country people in the neighbourhood, that there is a large quantity of grit stone grows in the earth near Elden Hole, but none near Castleton; and yet, on high floods, the river at Castleton washes great quantities of fragments of that very grit-stone out of the mouth of Castleton cavern.

There is also a tradition, which however ridiculous, ought to be preserved. Many years ago, an old woman, hunting her goose, it fell down into Elden Hole; but some days after, she heard it was seen at the mouth of Castleton cavern, and actually received it safe again from thence: the goose having, by the fluttering of its wings, preserved itself from being dashed to pieces in its fall; and having found its passage safely through the subterraneous river.

XXXII. Account of Two New Tortoises. By Tho. Pennant, Esq., F.R.S. p. 266.

The first of these was communicated by Dr. Garden, of Charlestown, in South Carolina, in the following words: ' I now come to speak of a species of turtle or tortoise, peculiar to our southern rivers. We call it the soft-shelled turtle; because, when alive, the covering looks like leather, very smooth and pliable, without any appearance of bone in it. It is very swift and fierce. They are not commonly got here in Charlestown, though by chance this last summer I had

two sent me. One of them I had preserved entire and sent to our friend Mr. Ellis; the other, less perfect, I have sent you. This is a very curious animal, and I think a nondescript, for there is none of Linnæus's 15 species that resemble it, except the first; and that he particularly mentions is found in the Mediterranean;* but this always inhabits fresh waters, remote from the sea. The head and snout are particularly distinguished from every other turtle; and what is more, I am told they exceed any turtle in the delicacy of their taste and flavour. I never ate any of them; but have heard many speak of them who were great epicures, and they have assured me, that they were far preferable to the green kind.'

Fresh Water Turtle,† *commonly called Soft-shelled Turtle.* Pl. 4.

'They are found in large quantities in Savannah and Alatamaha rivers; and I have been told that they are very common in the rivers in East Florida. They grow to very large sizes, though the largest that ever I heard of was 70 pounds. The turtle which I now have by me, weighs 20 pounds; and probably when I first got it, it might have weighed from 25 to 30 pounds, as I have observed that it has become poorer every day. I have had it now near 3 months, and I never could observe that it has eaten any thing that has been given it, though a variety of things have been tried. It is twenty inches long, from one end of the shell or covering to the other, and 14 inches and a half broad. The colour of this shell or covering in general is dark brown, with a greenish cast.

'The middle part is hard, strong, and bony; but all round the sides, especially towards the tail and hinder part, it is cartilaginous, soft and pliable, resembling thick tanned sole-leather, yielding very easily to any force in any direction whatever, but thick enough and strong enough to defend the animal from any injury. All the hind part of the back is full of oblong smooth knobs; and the fore part, just where it covers the head and neck, is studded full of large knobs. The under side of this plate is very beautiful, of a lively whitish colour, interspersed with innumerable very fine ramifications of blood vessels, running from the margin of the plate into larger and larger branches, till the sight of them is at once lost by their entering the body of the animal. The under, or belly plate, or rather sternum, is of a fair whitish colour, and extended forward 2 or 3 inches more than the back plate; so that the head rests on it very conveniently. The hind part of this plate is hard and bony, shaped very much like a man's riding

* There are two species of tortoises in that sea, a coriaceous one, and another resembling that of the West Indies, which is scarcely eatable. The last I procured from Leghorn, and at this time am doubtful whether it differs specifically from the West Indian turtle.—Orig.

† This species is the *Testudo ferox,* Lin. Gmel. *T. testa cartilaginea ovata, pedum unguibus tribus, naribus tubulatis prominentibus.*

saddle, with two pieces for the thighs to rest on. The fore part of the plate is pliable and cartilaginous. The head is somewhat triangular and attenuated, rather apparently small for the animal, but growing gradually larger towards the neck, which is thick and long, and easily extended out (the neck of the present subject was 13 inches and a half long) to a great length, or drawn back again under the shell or plate.

'The eyes are placed in the fore and upper part of the head, near to each other, having pretty large loose palpebræ. The pupil is small and lively, surrounded by a lemon-coloured iris, perfectly round, and giving much life and fire to the eyes. When danger approaches, or when it goes to sleep, it covers its eyes, by bringing the inner and loose part of the lower palpebræ over its eye, like a membrana nictitans. The upper and under lips are both large, but especially the upper. The mandibula are both entire, each being one entire bone all round, of the same shape as the mouth. The nostrils are the most singular part, being a cartilaginous production of at least 3 quarters of an inch, beyond the upper and fore angle or point of the upper lip, perforated with 2 apertures reaching back and opening into the roof of its mouth, having a smooth septum, but fimbriated on each side. This at first sight, in some manner resembles the snout of the mole; but it is tender, thin, and transparent, and cannot be intended for digging in the earth or land.'

'The arms are thick and strong, consisting of 3 distinct joints, viz. the upper, the fore arm, and hand. The hands have each 5 fingers, of which the first 3 are shorter and stronger, and furnished with strong nails, or rather claws. The last 2 fingers have more joints, but are smaller, and instead of being furnished with claws, are covered with the membrane, which is extended even beyond their extremities. Towards the back or hind part, there are two spurious fingers, which just serve to support the membrane when extended. The upper side of these arms and hands are covered with a wrinkled loose skin, of a dusky greenish colour. The legs consist of the same number of joints, and have the same number of toes as there are fingers on the fore-feet, and these are furnished with nails in the same manner, only there is one spurious toe. Both the fore and hind legs are thick, strong, and muscular; and as the animal is very fierce, when it is attacked or disturbed, it often raises itself on its legs, and will leap forward to bite its disturber or enemy, which it does with great fury and violence.'

'They are likewise very strong, and of a lively whitish colour, because they are generally, if not always, covered with the upper plate, which, as before said, is extended a great way behind. The tail is large and thick, and generally as long as the hind part of the upper plate. The anus is placed about an inch from

the extremity of the tail on the inside. The turtle from which these characters were taken was a female; after she came into my possession she laid 15 eggs, and about the same number were taken out of the belly when she died. The eggs were nearly an inch diameter, and perfectly spherical. It is esteemed very good eating, and said by many to be more delicate than the green turtle.'

The other species of tortoise, which I name the tuberculated,* was communicated to me by Mr. Humphries, of St. Martin's lane, merchant of minerals, shells, and insects. He was unacquainted with its place and history; therefore I must content myself with giving a mere description of it, deprived as I am of the knowledge of its manners and uses, without which even natural history is as replete with dulness as with inutility. Its length, from nose to the extremity of the back, is 3 inches 3 lines; its greatest breadth one inch and a half. The head is large and scaly. The neck thick and wrinkled. Eyes full; nostrils small and oval; the end of the upper mandible long and bifurcated, lapping very far over the lower.

The back is divided lengthwise, with 5 prominent ribs covered with large yellow tubercles; the intervening part is dusky, and divided by multitudes of lesser and more depressed tubercles. The whole circumference of the back bounded by a tuberculated rib, like those on the upper part. The extremity furcated. The whole is coriaceous and pliant. The tail is depressed sideways, tapers to a point, and reaches beyond the end of the back. The belly is yellow, tuberculated like the back, but marked with 6 rows greatly prominent. The prior fins are longer than the whole body, very thin, dusky, and edged on their interior sides with white, and both the surfaces are covered with depressed tubercles. The hind fins are broad, much dilated near their end, and slightly bilobated: none of these fins had the least marks of toes or nails. This may probably be the same with the testudo coriacea of Linnæus, p. 350, or the coriaceous one above mentioned: but as I have not at present before me the authors cited by that able naturalist, I will not pretend to pronounce with certainty whether it is the same.

Pl. 4, fig. 1, is the soft-shelled tortoise. 2. The same on its back. 3. The same with its neck exerted; drawn from the dried animal. 4. The tuberculated tortoise. 5. Exhibits the form of the mouth.

* This is probably no other than the young of the *Testudo coriacea*, Lin. *T. pedibus pinniformibus muticis, testa coriacea, caudæ angulis septem exaratis.* It grows to a vast size, and is found in the Mediterranean and Atlantic.

XXXIII. Meteorological Observations at Caën in Normandy; for 1765, 1766, 1767, 1768, 1769. *By Nathaniel Pigot, Esq., F.R.S.* p. 274.

This meteorological register contains the greatest and least heights of the barometer and thermometer, in each month of the years above mentioned; with remarks on the winds and the weather. At the end of the register, the means of the heights of the barometer, are collected, and ranged in a table, as follows:

Mean Heights of Barometer at Caen in Normandy.

Months.	Mean Heights in Inches, in 1765.	1766.	1767.	1768.	1769.
January	29.650	30.375	29.575	29.550	29.825
February	29.505	30.065	29.665	29.965	29.725
March	29.495	29.565	.. .. ..	29.705	29.835
April	29.975	29.875	.. .. ..	29.795	29.605
May	29.900	.. .. ..	29.860	29.760	29.945
June	30.000	.. .. ..	29.770	29.855	29.920
July	29.990	29.885	29.825	29.845	29.600
August	29.660	30.065	.. .. ..	30.015	29.810
Sept.	.. .. ..	29.990	29.890	29.740	29.785
October	.. .. ..	29.555	29.920	.. .. ..	29.980
Nov.	29.645	.. .. ..	29.790	29.410	30.035
Dec.	29.730	.. .. ..	29.815	29.770	29.590
Means	29.655	29.992	29.790	29.828	29.805

And the mean of all these 5 means is 29.802.

These observations were made at noon, in a south-west room, with a barometer, whose tube is about $\frac{3}{10}$ of an inch diameter; in which the motion of the quicksilver, in squalls and gusts of wind, is extremely perceptible; yet for further satisfaction, Mr. P. ordered another to be made in London, with the greatest care, by Heath and Wing, with a nonius giving the $\frac{1}{100}$ part of an inch. He placed this barometer by the other, in July 1769; and having compared them every day for a year, he found that the ancient one marks $\frac{5}{100}$ of an inch more than the other; therefore, if from the mean height as above 29.802
be deducted 0.050

the true mean height is 29.752
The greatest height observed at noon was, Jan. 29, 1766 .... 30.72
The least, Dec. 23, 1769 .... 28.69

Limits of the motion of quicksilver 2.03

Hence it appears, that if the mean height of barometers, on a level with the surface of the sea, be supposed, with Dr. Scheuchzer, Phil. Trans. N° 405, 406,
.... 29.993 inches
and the mean height at Caën .... 29.752 ditto.

then the mean difference .... 0.241 will be the greater mean weight of the atmosphere at the surface of the sea, than at Caën: and if, with the doctor, we allow, for each 10th of an inch depression of the quicksilver, 90 feet elevation, Mr. P.'s room, which is in the highest part of the town, will be about 217 feet above the level of the sea.

Mons. de Luc, of Geneva, has given a method to measure the different elevation of places by barometers, founded on his own observations, far more exact than any other before him: his rule is, 'the difference of the logarithms of the height of the quicksilver, in the two places, reduced into French lines, and the logarithms carried to 5 places, including the characteristics, will give the difference of elevation in toises, if Fahrenheit's thermometer be nearly at 66°; but about $\frac{1}{18}$ must be deducted from the elevation so given, if the thermometer be at 55° or temperate.'

29.993 English inches	= 337.824 French lines,	log. 25286.8
29.752 ditto	= 335.110 ditto	log. 25251.9
	difference	34.9 toises, or

209.4 French feet = 223 English feet nearly; from which if $\frac{1}{18}$ = $12\frac{1}{2}$ nearly be deducted, $210\frac{1}{2}$ feet remain for the difference of elevation of Mr. P.'s room and the surface of the sea; which differs $6\frac{1}{2}$ feet from the result given by the first hypothesis.

The greatest height observed, in these 5 years, with a good Fahrenheit's thermometer, screened from the sun in a s. w. room, was as follows at noon:

1765 August 23d }
1766 August 9th } 76°
1769 July 7th }

the least height of ditto, Jan. 6th, 1768 ...... 14. August 23, 1765, exposed the thermometer, at noon, to the sun, suspended on a thread between two sticks, in the middle of his garden at Caën, which may be about 2 English acres, so that the thermometer received the least reflected heat possible in that place; the quicksilver stood as follows, at 1h P. M. 97°, at 2h ditto 96. August 26, 1765, at a village, called les Isles Bardelles, 7 leagues from Caën, the same thermometer, in a south room, from which the sun was excluded by the window shutters, rose to 90°.

An Account of a remarkable degree of cold observed at Caen in Normandy.

1767	Ther.	Hours	1767	Ther.	Hours	1767	Ther.	Hours
December			December			December		
24	+ 12°	at 8h 0m	26	+ 12°	at 21h 00m	28	+ 15°	at 8h 20m
	+ 16	at 9 0			at 22 00		+ 14	at 11 00
							+ 13	at 19 00
25	+ 18	at 11 0	27	+ 17	at 7 00		+ 18	at 22 00
	+ 15	at 12 44		+ 16½	at 8 00			
	+ 18	at 21 45		+ 12	at 10 00	30	+ 23	at 7 00
		at 22 00		+ 11	at 11 00		+ 20	at 10 00
26	+ 15	at 10 00		+ 10	at 18 00		+ 11	at 19 00
	+ 11	at 19 00		+ 10	at 20 00		+ 14	at 21 00
					at 22 00			at 22 00

An Account of a remarkable degree of cold, observed at Caen in Normandy.

1768	Ther.	Hours	1768	Ther.	Hours	1768	Ther.	Hours
January			January			January		
3	+ 10°	at 11h 00m	4		at 19h 0m	5	+ 3°	at 19h 30m
	+ 9	at 20 00					+ 6	at 20 00
				− 2°½	at 5 30		+ 7	at 21 00
	+ 11	at 5 00		− 2	at 6 30		+ 8	at 21 30
	+ 10	at 6 00		− 2	at 8 00		+ 10	at 22 15
	+ 8	at 8 00		+ 1	at 9			
4	+ 6	at 9 15		0	at 9 30		+ 14	at noon
	0	at 12		− 3	at 10 00	6	+ 12	at 7 15
	− 8	at 19 30	5	0	at 10 35		+ 14	at 8 45
	0	at 21 0		0	at 11 00		+ 13	at 10 00

N. B. The sign + signifies, that the degrees marked by the quicksilver were above 0, or the beginning of the division.

The negative sign — signifies, that the degrees were below 0, or the beginning of the divisions.

The thermometer was exposed in the open air to the north.

The hours are astronomical hours.

XXXIII. *Nyctanthes Elongata, a New Indian Plant, respectfully communicated to the R. S. of London. By Peter Jonas Bergius,* M.D., &c. From the Latin.* p. 289.

The annexed figure, fig. 6, pl. 4, shows a sprig in half its natural size: the specimen itself was received from C. G. Ekeberg, lately arrived from a voyage to China.

Nyctanthes (elongata) foliis cordato-lanceolatis acutis elongatis minoribusque, ramis teretibus.

Description. Stem shrubby. Branches subprocumbent, opposite, cylindrical, the inferior smooth, the superior villose, branched: branchlets opposite. Leaves opposite, cordate-lanceolate, nearly two thumbs' breadth long, acuminate, perfectly entire, smooth on both sides, nerved, a little undulated at the margin, deep green: the lower ones of the branchlets gradually smaller, but the lowest of all cordate-ovate, small. Flowers terminal on the branchets, five or six together, subumbellated, shortly footstalked. Perianth of calyx monophyllous, tubular, very small, permanent; six or seven-cleft; divisions subulate, hairy. Corol monopetalous; tube cylindric, striated, long, measuring near a thumb's breadth, thickened at the upper part: border flat, eight or nine-parted: divisions ovate-oblong, acute. Stamens, filaments two, short; anthers linear, obtuse,

* Dr. Peter Jonas Bergius was professor of natural history in the university of Stockholm, and died in the year 1790. He was the author of a Description of several African Plants from the Cape of Good Hope, and of some other papers; but his principal work is his Materia Medica, (Stockholm, 1778, 8vo.) In this work the several articles belonging to the vegetable kingdom are arranged according to the Linnæan system, and their medical qualities detailed with great accuracy and judgment; rendering it a truly meritorious publication.

sulcated on each side, hid within the tube of the corol. Pistil. Germ roundish, truncate, retuse, smooth. Style filiform, length of stamens. Stigma thickened, bifid.

XXXIV. Account of a Mole from North America. By the Hon. Daines Barrington, F.R.S.* p. 292.

This species of mole much resembles that of Europe in its general appearance, except in point of colour: to show however that there is a very material and specific difference between the two animals, Mr. B. sent the head of the common English mole, which contains all the teeth belonging to each jaw. The American specimen was not indeed so perfect in this respect; but a sufficient number of teeth remained, to show the distinction between these two sorts of moles. In the European, there are 6 cutting teeth in the upper jaw, which are followed by 2 canine ones. In the American, on the other hand, there are 2 very long and large cutting teeth in the centre, calculated to fill the vacancy in the lower jaw, which contains only 2 short cutting teeth, followed immediately by 2 long canine ones. In the lower jaw of the European mole, however, there are 8 small cutting, without the intervention of any canine teeth.

XXXV. Experiments made in North Wales, to ascertain the Different Quantities of Rain, which fell in the Same Time, at Different Heights. By the Hon. Daines Barrington, F.R.S. p. 294.

Mr. B. having procured 2 proper rain-gages, he sent them to Mr. Meridith Hughes, of Bala, in Merionethshire, a very ingenious land surveyor; and directed him to place one of the rain-gages at the top of Rennig, which is about 4 miles west of Bala, and is commonly considered as the 5th mountain of North Wales, in point of height.† Mr. B. directed the other rain-gage to be fixed near a house, called Bochyrhaidr, about half a mile distant from Rennig; and so as that the rain might not be impeded, when the wind blew over the mountain towards the point where the lower rain-gage was placed. Proper precautions were also taken, that neither cattle, nor any other accident, should interfere with the experiment.

Being desirous to know, with some degree of precision, the height of this

* It is hardly possible from this very brief description to particularize the animal intended; but it should seem, from the account of the teeth, to be some species of *Mus*, and not of *Talpa*.

† I rather suppose it however to be only the 6th, and should range them thus, according to their comparative heights: Carnedd Llewelin, Snowdon, Cader Idrys, Arran Mowddy, Glider, and Rennig. I place Carnedd Llewelin before Snowdon, because I carried a water level to the top of the latter, and conceived Carnedd Llewelin to be higher; perhaps the difference may be only a few yards. B.—Orig.

mountain, Mr. B. directed Mr. Hughes to ascertain it in the common method, by examining the fall of the mercury in the barometer, at the top, when compared with its state at the bottom. Having made this experiment, Mr. H. reported that the difference was one inch and $\frac{6}{10}$; which, according to Dr. Halley's method of computation, would give about 450 yards in height, from the adjacent plain.

By the following table it will appear, that the quantities of rain, which had fallen in the two rain-gages were weighed 6 several times; in 3 of which the contents of the upper receiver exceeded those of the lower; and in the 3 others, the quantity in the lower exceeded that of the upper. On the whole, however, the contents of the lower rain-gage exceeded that of the upper above half an inch. This trifling difference therefore seems to arise from a shower's lasting perhaps a little longer on the bottom of the mountain, and not from any permanent cause, as in Dr. Heberden's observations.

The inference to be drawn however from them seems to be, that the increase of the quantity of rain depends on its nearer approximation to the earth, and scarcely at all on the comparative height of places, provided the rain-gages are fixed at about the same distance from the ground. Possibly also a much controverted point between the inhabitants of mountains and plains may receive a solution from these experiments; as in an adjacent valley, at least, very nearly the same quantity of rain appears to fall within the same period of time as on the neighbouring mountains.

1770.	Bochyraidr. Grains.	Inches.	The top of Rennig. Grains.	Inches.
From July 6th to 16th.	5080 =	0.709	4643 =	0.648
July 16th to 29th.	15654 =	2.185	15217 =	2.124
July 29th to Aug. 10th.	4370 =	0.610	4698 =	0.656
Sept. 9th both bottles had run over.				
Sept. 9th to 30th	23167 =	3.234	17648 =	2.464
Oct. 17th both bottles had run over.				
Oct. 17th to 22d	5353 =	0.747	6336 =	0.885
Oct. 22d to 29th	9179 =	1.281	9944 =	1.388
Nov. 20, both bottles were broken by the frost.	Total	8.766		8.165

Note by Dr. Heberden.—It may not be improper to subjoin to the foregoing account, that, in the places where it was first observed that a different quantity of rain would be collected, according as the rain-gages were placed above or below the tops of the neighbouring buildings, the rain-gage below the top of the house, into which the greater quantity of rain had for several years been found to fall, was above 15 feet above the level of the other rain-gage, which in another part of London was placed above the top of the house, and into which the lesser quantity always fell. This difference therefore does not, as Mr. Barrington justly remarks, depend on the greater quantity of atmosphere, through which the rain descends: though this has been supposed by some, who have thence concluded, that this appearance might readily be solved by the accumulation of more drops, in a descent through a greater depth of atmosphere.—Orig.

XXXVI. A Disquisition concerning certain Fluents, which are Assignable by the Arcs of the Conic-Sections; where are Investigated some New and Useful Theorems for Computing such Fluents. By John Landen, F. R. S. p. 298.

Mr. Mac Laurin, in his Treatise of Fluxions, has given sundry very elegant theorems for computing the fluents of certain fluxions by means of elliptic and hyperbolic arcs; and Mr. D'Alembert, in the Memoirs of the Berlin Academy, has made some improvement on what had been before written on that subject. But some of the theorems given by those gentlemen being in part expressed by the difference between an arc of an hyperbola and its tangent, and such difference being not directly attainable, when such arc and its tangent both become infinite, as they will do when the whole fluent is wanted, though such fluent be at the same time finite; those theorems therefore in that case fail, a computation thereby being then impracticable, without some further help.

The supplying that defect Mr. L. considered as a point of some importance in geometry, and therefore he earnestly wished, and endeavoured, to accomplish that business; his aim being to ascertain, by means of such arcs as above-mentioned, the limit of the difference between the hyperbolic arc and its tangent, while the point of contact is supposed to be carried to an infinite distance from the vertex of the curve, seeing that, by the help of that limit, the computation would be rendered practicable in the case wherein, without such help, the before-mentioned theorems fail. And having succeeded to his satisfaction, he presumes the result of his endeavours, which this paper contains, will not be unacceptable to the Royal Society.

1. Suppose the curve ADEF, pl. 4, fig. 7, to be a conic hyperbola, whose semi-transverse axis AC is $= m$, and semi-conjugate $= n$. Let CP, perpendicular to the tangent DP, be called p; and put $f = \frac{m^2 - n^2}{2m}$, $z = \frac{p^2}{m}$. Then, as is well known, will DP — AD be = the fluent of $\frac{-\frac{1}{2}m^{\frac{1}{2}}z^{\frac{1}{2}}\dot{z}}{\sqrt{n^2 + 2fz - z^2}}$, p and z being each = to m when AD is $= 0$.

2. Suppose the curve adefg fig. 8, to be a quadrant of an ellipsis, whose semi-transverse axis cg is $= \sqrt{m^2 + n^2}$, and semi-conjugate ac $= n$. Let ct be perpendicular to the tangent dt, and let the abscissa cp be $= n\sqrt{\frac{z}{m}}$. Then will the said tangent dt be $= m\sqrt{\frac{mz - z^2}{n^2 + mz}}$; and its fluxion will be found $= \frac{1}{2}mn^2\dot{z} - \frac{1}{2}\dot{z} \times \frac{\sqrt{m - z}}{\sqrt{n^2 + mz}} - \frac{\frac{1}{2}\dot{z}\sqrt{mz}}{\sqrt{n^2 + 2fz - z^2}}$.

3. In the expression

$\frac{y^{q-1}\dot{y}}{(a + by)^r \times (c + dy)^s}$, let $\frac{c + dy}{a + by}$ be supposed $= z$. Then will $\frac{az - c}{d - bz}$ be $= y$, and the proposed expression will be $= \frac{(ad - bc)^{1-r-s} \times z^{-s}\dot{z}}{(az - c)^{1-q} \times (d - bz)^{1+q-r-s}}$.

1

4. Taking, in the last article, r and s each $= \frac{1}{2}$, $q = \frac{3}{2}$, $a = -d = \frac{n^2}{m}$, $b = 1$, and $c = n^2$, we have

$$\frac{y^{\frac{1}{2}}\dot{y}}{\sqrt{\frac{n^2}{m} + y} \times \sqrt{n^2 - \frac{n^2}{m}y}} = \frac{n^{-1}\dot{y}\sqrt{my}}{\sqrt{n^2 + 2fy - y^2}} = -mnz^{-\frac{1}{2}}\dot{z} \times \frac{(m-z)^{\frac{1}{2}}}{(n^2 + mz)^{\frac{3}{2}}}.$$

It appears therefore, that y being $= n^2 \times \frac{m-z}{n^2 + mz} - \frac{\frac{1}{2}\dot{y}\sqrt{my}}{\sqrt{n^2 + 2fy - y^2}} - \frac{\frac{1}{2}\dot{z}\sqrt{mz}}{\sqrt{n^2 + 2fz - z^2}}$ is $= \frac{1}{2}mn^2z^{-\frac{1}{2}}\dot{z} \times \frac{(m-z)^{\frac{1}{2}}}{(n^2 + mz)^{\frac{3}{2}}} - \frac{\frac{1}{2}\dot{z}\sqrt{mz}}{\sqrt{n^2 + 2fz - z^2}}$; which, by art. 2, is = the fluxion of the tang. dt.

Consequently, taking the fluents by art. 1, and correcting them properly, we find DP — AD + FR — AF = L + dt.

CP, in fig. 7, being $= \sqrt{m^2z^2}$; cp, in fig. 8, $= n\sqrt{\frac{z}{m}}$;
CR, perpendicular to the tangent FR, $= \sqrt{m^2y^2}$;
DP — AD = the fluent of $\frac{-\frac{1}{2}\dot{z}\sqrt{mz}}{\sqrt{n^2 + 2fz - z^2}}$;
FR — AF = the fluent of $\frac{-\frac{1}{2}\dot{y}\sqrt{my}}{\sqrt{n^2 + 2fy - y^2}}$;
and L the limit to which the difference DP — AD, or FR — AF, approaches, on carrying the point D, or F, from the vertex A ad infinitum.

5. Suppose y equal to z, and that the points D and F then coincide in E, the points d and p being at the same time in e and q respectively. Then cv being perpendicular to the tangent ev, that tangent will be a maximum and equal to cg — ac $= \sqrt{m^2 + n^2} - n$; the tangent EQ, in the hyperbola, will be $= \sqrt{m^2 + n^2}$; the abscissa BC $= m\sqrt{(1 + \frac{n}{\sqrt{m^2 + n^2}})}$; the ordinate BE $= n \times \sqrt{\frac{n}{\sqrt{m^2 + n^2}}}$; and it appears that L is = 2EQ — 2AE — ev $= n + \sqrt{m^2 + n^2} -$ 2AE! Thus the limit proposed to be ascertained, is investigated, m and n being any right lines whatever!

6. The whole fluent of $\frac{\frac{1}{2}\dot{z}\sqrt{mz}}{\sqrt{n^2 + 2fz - z^2}}$, generated while z from 0 becomes $= m$, being equal to L; and the fluent of the same fluxion (supposing it to begin when z begins) being in general equal to L + AD — DP = FR — AF — dt; it appears, that, k being the value of z corresponding to the fluent L + AD — DP, $\frac{mn^2 - n^2k}{n^2 + mk}$ will be the value of z corresponding to the fluent L + AF — FR, and FR — AF will be the part generated while z from $\frac{mn^2 - n^2k}{n^2 + mk}$ becomes $= m$. It follows therefore that the tangent dt, together with the fluent of $\frac{\frac{3}{4}\dot{z}\sqrt{mz}}{\sqrt{n^2 + 2fz - z^2}}$, generated while z from 0 becomes equal to any quantity k, is equal to the fluent

of the same fluxion generated while z from $\frac{mn^2 - n^2k}{n^2 + mk}$ becomes $= m$; cp being taken $= n\sqrt{\frac{k}{m}}$.

Suppose $k = \frac{mn^2 - n^2k}{n^2 + mk}$, its value will then be $\frac{n}{m}\sqrt{m^2 + n^2} - \frac{n^2}{m}$. Consequently the fluent of $\frac{\frac{1}{2}\dot{z}\sqrt{mz}}{\sqrt{n^2 + 2fz - z^2}}$, generated while z from 0 becomes $= \frac{n}{m}\sqrt{m^2 + n^2} - \frac{n^2}{m}$, together with the quantity $\sqrt{m^2 + n^2} - n$, is equal to the fluent of the same fluxion generated while z from $\frac{n}{m}\sqrt{m^2 + n^2} - \frac{n^2}{m}$ becomes $= m$: and these two parts of the whole fluent being denoted by M and N respectively; M will be $=$ n — AE, and N $= \sqrt{m^2 + n^2}$ — AE.

7. The fluent of $\frac{\frac{1}{2}\dot{z}\sqrt{mz}}{\sqrt{n^2 + 2fz - z^2}}$ being L + AD — DP, the fluent of $\frac{\frac{1}{2}\dot{z}\sqrt{mz}}{\sqrt{n^2 + 2fz - z^2}}$ + DP — AD — L will be $= 0$. Therefore, the fluent of $\frac{\frac{1}{2}\dot{z}\sqrt{mz}}{\sqrt{n^2 + 2fz - z^2}}$ + the fluent of $\frac{\frac{1}{2}n^2\dot{z}\sqrt{\frac{1}{mz}}}{\sqrt{n^2 + 2fz - z^2}}$ being $=$ the fluent of $\frac{1}{2}z^{-\frac{1}{2}}\dot{z}\sqrt{\frac{n^2 + mz}{m - z}}$, it is obvious, that the fluent of $\frac{\frac{1}{2}n^2\dot{z}\sqrt{\frac{1}{mz}}}{\sqrt{n^2 + 2fz - z^2}}$ is $=$ DP — AD — L + the fluent of $\frac{1}{2}z^{-\frac{1}{2}}\dot{z} \times \sqrt{\frac{n^2 + mz}{m - z}}$ = DP — AD — L + the elliptic arc dg (fig. 8) whose abscissa cp is $= n\sqrt{\frac{z}{m}}$.

Consequently, putting E for $\frac{1}{4}$ of the periphery of that ellipsis, it appears that the whole fluent of $\frac{\frac{1}{2}n^2\dot{z}\sqrt{\frac{1}{mz}}}{\sqrt{n^2 + 2fz - z^2}}$, generated while z from 0 becomes $= m$, is equal to E — L = E + 2AE — n — $\sqrt{m^2 + n^2}$.

8. By taking, in art. 3, q, r, and s, each $= \frac{1}{2}$; and $a = -d = \frac{n^2}{m}$, $b = 1$, and $c = n^2$; we find, that, if y be $= \frac{mn^2 - n^2z}{n^2 + mz}$, $\frac{\dot{z}\sqrt{\frac{1}{z}}}{\sqrt{n^2 + 2fz - z^2}} + \frac{\dot{y}\sqrt{\frac{1}{y}}}{\sqrt{n^2 + 2fy - y^2}}$ will be $= 0$.

It is obvious therefore, that the fluent of $\frac{\dot{z}\sqrt{\frac{1}{z}}}{\sqrt{n^2 + 2fz - z^2}}$, generated while z from 0 becomes equal to any quantity k, is equal to the fluent of the same fluxion, generated while z from $\frac{mn^2 - n^2k}{n^2 + mk}$ becomes $= m$.

Now, supposing $k = \frac{mn^2 - n^2k}{n^2 + mk}$, its value will be $\frac{n}{m}\sqrt{m^2 + n^2} - \frac{n^2}{m}$. Consequently, the fluent of $\frac{\dot{z}\sqrt{\frac{1}{z}}}{\sqrt{n^2 + 2fz - z^2}}$, generated while z from 0 becomes $= \frac{n}{m}\sqrt{m^2 + n^2} - \frac{n^2}{m}$, is equal to half the fluent of the same fluxion, generated while z from 0 becomes $= m$; which half fluent is known by the preceding article.

9. It appears, by art. 4, that $\frac{\frac{1}{2}\dot{y}\sqrt{my}}{\sqrt{n^2 + 2fy - y^2}} + \frac{\frac{1}{2}\dot{z}\sqrt{mz}}{\sqrt{n^2 + 2fz - z^2}}$ is $=$ — the flux. of the tang. dt; and it appears, by the last article, that $\frac{\frac{1}{2}n^2\dot{y}\sqrt{\frac{1}{my}}}{\sqrt{n^2 + 2fy - y^2}} + \frac{\frac{1}{2}n^2\dot{z}\sqrt{\frac{1}{mz}}}{\sqrt{n^2 + 2fz - z^2}}$ is $= 0$; $mn^2 - n^2y - n^2z - myz$ being $= 0$.

Therefore, by addition, we have

$\frac{1}{2}\dot{y}\sqrt{\frac{1}{y}} \times \sqrt{\frac{n^2 + my}{m - y}} + \frac{1}{2}z^{-\frac{1}{2}}\dot{z} \times \sqrt{\frac{n^2 + mz}{m - z}} = -$ the fluxion of the tangent dt.

Consequently, by taking the correct fluents, we find the tang. dt (= the tang. fw) = the arc ad — the arc fg! the abscissa cp being = $n\sqrt{\frac{z}{m}}$, the abscissa cr = $n\sqrt{\frac{y}{m}}$, and their relation expressed by the equation $n^6 - n^4u^2 - n^4v^2 - m^2u^2v^2 = 0$, u and v being put for cp and cr respectively. Moreover, the tangents dt, fw, will each be $= \frac{m^2uv}{n^4}$; and ct $\times$ cw = cv^2 = ac $\times$ cg.

If for the semi-transverse axis cg we substitute h instead of $\sqrt{m^2 + n^2}$, the relation of u to v will be expressed by the equation $n^6 - n^4u^2 - n^4v^2 - (h^2 - n^2) \times u^2v^2 = 0$, and dt (= fw) will be $= \frac{h^2 - n^2}{n^3} \times uv$.

If u and v be respectively put for fr and dp, their relation will be expressed by the equation $h^6 - h^4u^2 - h^4v^2 + (h^2 - n^2) \times u^2v^2 = 0$, and dt (= fw) will be $= \frac{h^2 - n^2}{h^3} \times uv$.

10. Suppose y = to z, that is, $v = u$, and that the points d and f coincide in e. In which case the tangent dt will be a maximum, and = cg — ac. It appears then that the arc ae — the arc eg is = cg — ac. Consequently, putting E for the quadrantal arc ag, we find that the arc ae is $= \frac{E + h - n}{2}$!

the arc eg $= \frac{E - h + n}{2}$!

There are, Mr. L. is aware, some other parts of the arc ag, whose lengths may be assigned by means of the whole length ag, with right lines; but to investigate such other parts is not to his present purpose.

11. Taking m and n each = 1; that is, ac = AC = 1, and cg = $\sqrt{2}$; let the arc ag be then expressed by e: put c for $\frac{1}{4}$ of the periphery of the circle whose radius is 1; and let the whole fluents of $\frac{\frac{1}{2}\dot{z}\sqrt{z}}{\sqrt{1 - z^2}}$ and $\frac{\frac{1}{2}\dot{z}\sqrt{\frac{1}{z}}}{\sqrt{1 - z^2}}$, generated while z from 0 becomes = 1, be denoted by F and G respectively. Then, by what is said above, F + G will be = e; and, by his theorem for comparing curvilineal areas, or fluents, published in the Philos. Trans. for the year 1768, it appears that F $\times$ G is = $\frac{1}{2}c$. From which equations we find F = $\frac{1}{2}e - \frac{1}{2}\sqrt{e^2 - 2c}$, and G = $\frac{1}{2}e + \frac{1}{2}\sqrt{c^2 - 2c}$.

But m and n being each = 1, L is = F; therefore 1 + $\sqrt{2}$ — 2AE, the value of L, from art. 5, is in this case = $\frac{1}{2}e - \frac{1}{2}\sqrt{e^2 - 2c}$. Consequently, in the equilateral hyperbola, the arc AE, whose abscissa BC is = $\sqrt{(1 + \sqrt{\frac{1}{2}})}$, will be $= \frac{1}{2} + \sqrt{\frac{1}{2}} - \frac{1}{4}e + \frac{1}{4}\sqrt{e^2 - 2c}$, by what is said in the article last mentioned. Hence the rectification of that arc may be effected by means of the circle and ellipsis!

The application of these improvements will be easily made by the intelligent reader, who is acquainted with what has been before written on the subject. But there is a theorem, demonstrable by what is proved in art. 8, so remarkable, that he cannot conclude this disquisition without taking notice of it.

12. Let *lpqn*, fig. 9, be a circle perpendicular to the horizon, whose highest point is *l*, lowest *n*, and centre *m*; let *p* and *q* be any points in the semicircumference *lpqn*; draw *ps*, *qt* parallel to the horizon, intersecting *lmn* in *s* and *t*; and, having joined *lp*, *pt*, make the angle *lpv* equal to *ltp*, and draw *rv* parallel to *qt*, intersecting the circle in *r*, and the diameter *lmn* in *v*. Let a pendulum, or other heavy body, descend by its gravity from *p* along the arc *pqrn*: the body so descending will pass over the arc *pq* exactly in the same time as it will pass over the arc *rn*; and therefore, *qt* and *rv* coinciding when *lt* is equal to *lp*, it is evident that the time of descent from *p* to *q* will then be precisely equal to half the time of descent from *p* to *n*!

And it is further observable, that, if *pqn* be a quadrant, the whole time of descent will be $= \sqrt{\frac{a}{b}} \times (\frac{1}{2}e + \frac{1}{2}\sqrt{e^2 - 2c})$; the radius *lm*, or *mn*, being $= a$; and *b* being put for ($16\frac{1}{12}$ feet) the space a heavy body descending freely from rest falls through in one second of time.

In general *ns* being denoted by *d*, and the distance of the body from the line *ps*, in its descent, by *x*, the fluxion of the time of descent will be expressed by $\frac{\frac{1}{2}a\dot{x}\sqrt{\frac{1}{bx}}}{\sqrt{[2ad - d^2 - (2a - 2d).x - x^2]}}$; the fluent of which, corresponding to any value of *x*, may be obtained by art. 7. By which article it appears, that the whole time of descent from any point *p* will be $= \frac{a}{\sqrt{bd} \times (2a - d)} \times (\text{E} + 2\text{AE} - pn - ps)$.

The semi-transverse AC (fig. 7) being $= ns$;
the semi-transverse cg (fig. 8) $= np$;
and the semi-conjugate in each figure $= ps$.

After writing the above, Mr. L. discovered a general theorem for the rectification of the hyperbola, by means of two ellipsis; the investigation of which he purposed to make the subject of another paper.

XXXVII. On the Management of Carp in Polish Prussia. By J. Reinhold Forster, F. A. S. p. 310.

Though the carp is now commonly found in ponds and rivers, and generally thought to be a fresh water fish,* the ancient zoologists ranged the same among

* I have great reason to think, that many other fish, which, it is commonly conceived, can only live in the sea, may also exist, at least for several years, and perhaps breed, in fresh water. The smelt or sparling (salmo eperlanus Linnæi) never comes up our rivers, but for a short time; and then does not penetrate much farther than where the water continues to be brackish. I have however been informed by Sir Francis Barnard, the late governor of New England, that in a large pool

the sea-fish: and Mr. F. knew instances of its being caught in the harbour of Dantzig, between that city and a little town called Hela; which is situated at the extremity of a long, narrow, sandy promontory, projecting eastwards into the sea, and forming the gulf before Dantzig, of about 30 English miles diameter. These carp were forced probably by a storm, from the mouth of the Vistula, which here enters the Baltic, into the sea: and as the other two branches of the Vistula or Weixel disembogue into a large fresh water lake, called the Trish-Haff, which has a communication with the sea at Pillau; it is equally probable, that these fish came round from Pillau, to the harbour of Dantzig; especially as they are frequently found in the Trish-Haff.

The sale of carp makes a part of the revenue of the nobility and gentry in Prussia, Pomerania, Brandenburg, Saxony, Bohemia, Mecklenburgh, and Holstein; and the way of managing this useful fish is therefore reduced in these countries into a kind of system, built on a great number of experiments, made during several generations, in the families of gentlemen well skilled in every branch of husbandry.

The first thing which must be attended to, in case a gentleman chooses to have carp-ponds, is to select the ground where they are to be made: for on the soil, water, and situation of a pond, the success in the management greatly depends. The best kind of ponds ought to be situated in a well manured, fertile

which he rented not far from Boston, and which had not the least communication with the sea, several of these fish, originally introduced from the salt water, had lived many years, and were to all appearance very healthy.

I have also the following well attested fact with regard to the common grey mullet, which it is believed was never before taken in fresh water. Mr. Kymer has made, near Kidwelly in Carmarthenshire, a communication between his collieries and an arm of the sea, by means of a canal. Before this canal was completed, the salt water filled it at every tide, and several mullets were by this means introduced. For these 3 or 4 years, the sea has been entirely excluded; and the canal, from the constant influx of fresh water, has ceased to be brackish for more than 2 years. The mullets however continue to live in this canal; though Mr. Kymer informs me, they do not look in so good condition, as when fresh from the sea.

We are so much in the dark about the natural history of fish, particularly those of the salt water, that it is to be wished sea stews were made on some of our coasts, as I am told is very commonly practised in North America, and at a very trifling expence. Nothing more is requisite, than either to find or dig a proper cavity, perhaps a yard below the low water mark, at spring tides, from which the sea should be excluded, except at a narrow entrance, where large stones should be piled from the beach to above the high water mark. Through such an inlet, the stew would be, every 12 hours, supplied with fresh salt water, at the same time that the fish would not be able to make their escape. By this very easily contrived reservoir, sea-fish, when caught in too great numbers, might be kept for the supply of the table or market, when perhaps the weather will not permit them to be taken: and many ingenious experiments might be tried. It is not impossible, for example, that the fish of the fresh water might be improved, by continuing in such a stew for 2 or 3 weeks, as horses are said to thrive by feeding on the salt marshes. DAINES BARRINGTON.—Orig.

plain, surrounded by the finest pastures and corn fields of a rich black mould, having either mild or soft springs on the spot, or a rivulet that runs through the plain; the water ought to be mild and soft, by no means too cold, or impregnated with acid, calcareous, selenitic, or other mineral particles. The exposure must be sheltered against the cold blasting easterly or northern winds, by a ridge of hills, situated at some distance from the pond, enjoying fully the benign influence of the sun, far from any thick shady wood, that might intercept the beams of the sun, or where the leaves of trees might cause a putrefaction, or impregnate the water with astringent particles.

Such ponds as are surrounded by poor, cold, and stiff soils, or are open to the east and north winds, or have a wood on one or two sides, and hard or cold water, or such as issues from mines, moors, or mosses, are inferior in goodness. Ponds in a poor, dry, or sandy soil, surrounded by pines or firs, with the just-mentioned inconveniencies, are considered as the worst of all. The ground towards the pond ought to have a gentle slope: for deep valleys are subject to great floods, and will then endanger the dykes in a wet rainy season, and often the expectations of many years are carried away. The soil cannot be altered: it is therefore a chief qualification of a pond, to be contrived in a good soil. The sun is a less material article: provided therefore a pond can enjoy the morning and noon-tide sun, it matters not much if the wood be on one or two of its sides. The water is a material point; but in case the springs that supply the ponds are very cold and hard, it may be softened and tempered by exposing it to the sun and air in a large reservoir above the pond, or by leading it for a long way in an open exposure, before it enters the pond. The quantity of water to supply the pond with, is another requisite; too much water makes too great a canal necessary for carrying its superfluity off; and this is very expensive: too little water has another inconvenience, viz. that of keeping the water too long in the pond, and to cause a stagnation, without any sufficient fresh supplies; and often, in a dry season, the scantiness of fresh water distresses the fish, and causes diseases and mortality among them.

The above remarks are general, and must be applied to all kinds of ponds. It is found by experience most convenient, to have 3 kinds of ponds for carp. The first is called the spawning pond: the nursery is the second; and the main pond is the third and largest. There are two methods for stocking the ponds with carp; either to buy a few old fish, and to put them into a spawning pond; or to purchase a good quantity of one year's old fry, for the nursery. A pond intended for spawning, must be well cleaned of all other kinds of fish, especially such as are of a rapacious nature, viz. pike, perch, eel, and trout; and also of all the newts or larvæ of lizards, and the dytisci or water-beetles, which frequently destroy quantities of the fry, to the great loss of the owner. A pond

of the size of about one acre, requires 3 or 4 male carp, and 6 or 8 female ones; and so in proportion to each acre, the same number of males and females.

The best carp for breeders are 5, 6, or 7 years old, in good health, in full scale, without any blemish or wound (especially such as are caused by the lernæa cyprini Linn. a kind of cartilaginous worm) with fine full eyes and a long body. Such as are sickly, move not briskly, have spots as if they had the small-pox, have either lost their scales, or have them sticking but loosely to the body, whose eyes lie deep in their heads, are short, deep, and lean, will never produce good breed. Being provided with a set of such carp as are here described, and sufficient to stock a pond with, it is best to put them, on a fine calm day, the latter end of March or in April, into the spawning pond. Care must be taken, that the fish be not too much hurt by being transported in a hogshead, nor put into the pond on a stormy day; for they are easily thrown upon the shallows on the sides, being weak and harassed by being caught, removed, and not yet acquainted with the deep holes for their retreat, in the new habitation.

Carp spawn in May, June, or July, according as the warm season sets in earlier or later. The warm weather expands and swells gently the bodies of the fish; and their bellies being distended with roe and milt, they feel an itching about those turgid parts, and therefore swim to a shallow, warm, sheltered place, where the bottom of the pond is either somewhat sandy or gritty, where some grass and aquatic plants grow, or where some ozier branches and roots hang in the water; they gently rub their bodies against the ground, the grass, or oziers, and by this pressure, the spawn issues out; and as the milter, by a natural instinct, follows the spawner, and feels the same itching, the calls of nature are gratified in the same manner, and the soft roe or milt is spread over the spawn, and thus impregnated. Carp in this season are frequently seen swimming, as if it were in a circle, about the same spot, which is merely done with an intention of repeating the rubbing of their expanded bellies. The finest and calmest summer days are commonly those on which carp spawn; Providence having thus made a provision for the greater security of the fry of so useful a fish; as otherwise, in a stormy day, the spawn would be washed towards the banks, where it would be eaten up by birds, or trampled by men and quadrupeds, or dried up by the heat of the sun, and a whole generation of carp entirely destroyed. In a pond of his uncle's, Mr. F. frequently found the carp in a warm summer evening, round a large stone, rubbing their bellies against the hard sandy ground; he often approached with as much silence as possible, put his hands and feet among the sporting carp, and had the satisfaction to see them pass and repass through his hands, without being in the least disturbed; but at the least noise or quick motion occasioned by him, they moved away with suprizing velocity.

About the spawning season, great care must be taken, to keep out all aquatic fowl, wild and tame, from the ponds; for geese and ducks not only swallow the spawn, but destroy still more of it, by searching the weeds and aquatic plants. It is therefore a general rule, to send twice a day, a man round the ponds, to scare all wild fowl, viz. swans, geese, ducks, cranes, and herons.

Sometimes crusians and carp, or tench and carp, being put together in a pond, and the males and females of each kind not being in a just proportion to one another, the different species mix their roe and milt, and thus produce mules or mongrel breeds. The mules, between carp and crusians, seldom and slowly attain the size which carp are capable of; they are very deep, and shorter in proportion than carp, but of a very hardy nature. The mules between carp and tench, partake of the nature of both fish, come to a good size; but some part of their body is covered with the small slimy scales of a tench, and some other part has the larger scales of carp; their flesh approaches nearer to that of a tench, and they are likewise of a less tender nature than the common carp: this latter kind of mule is called in Germany spiegel karpe, i. e. the mirror-carp, the blotches with large scales among the smaller ones being considered as mirrors. Whether these mules are capable of propagating their species, he cannot affirm; never having made any experiments on that subject; nor has he heard any thing said on that head with any degree of precision, or founded on experience. In some ponds in Lancashire, he was told, by a gentleman of great worth and honour, both these kinds of mules are now and then found. He thinks it however not advisable to put carp and tench, or carp and crusians, in one pond, unless it be done for experiment's sake; in which latter case, a small pond, free from other fish, with one or two fish of each kind, will be sufficient to gratify curiosity, without debasing a generation of carp in a large pond.

The young fry being hatched from the spawn, by the benign influence of the sun, they are left the whole summer, and even the next winter, in the spawning pond; in case the pond be so deep, that the suffocation of the young tender fry under the ice, in a severe winter, is not to be apprehended, for it is by no means advantageous to take them out in the first months of their existence. However, if the shallowness of the pond, its cold situation and climate, make it necessary to secure the fry against the rigours of the ensuing winter, the water of the pond must be let off; the fry and old fish will gradually retire to the canal and ditches, which communicate with the hole in the middle of the pond, and a net, with small meshes, is then employed to catch both the fry and old ones. The old breeders are then separated from the fry, and both kinds put in separate ponds, that are warmer and more convenient for the wintering of these delicate fish. Care must be taken to fix on a calm, mild day, at the latter end of September, for catching the fry out of the spawning-pond.

The nurseries are the 2d kind of ponds, intended for bringing up the young fry. The best time to put them into the nursery, is in March or April, on a fine and calm day. A thousand or 12 hundred of this fry may be allotted to each acre of a pond. The choice of the fry must be made according to the above enumerated characters of good and healthy fish, and must be carefully removed from one pond to another. It is likewise requisite to send people with long sticks, all the first day, round the pond, to drive the tender and weak fry from the sides into the pond, because they are bewildered in a strange place, and often become the prey of rapacious birds.* In case the pond be good, and not overstocked before, and the fry well chosen and preserved, it is almost certain, they will grow within 2 summers so much as to weigh 4, 5, and sometimes 6 pounds, and to be fleshy and well-tasted. Many Prussian gentlemen make a good profit, by selling their carp, after 2 years standing in the nursery, and export them even to Finland and Russia.

The main ponds are the last kind. In these carp are put that measure a foot head and tail inclusive. Every square of 15 feet in the pond is sufficient for one carp, and will afford food and room for the fish to play in. The more room carp have, and consequently the more food the pond affords, the quicker will be the growth of the fish. The longer the pond has been already in use, the longer you intend to keep the carp in it, the more you desire to quicken the growth of them, the more you ought to lessen the number of fish destined for the pond. Spring and autumn are the best seasons for stocking the main ponds. The growth of the fish will always be in proportion to the food they have; for carp are observed to grow a long time, and to come to a very considerable size, and a remarkable weight. Mr. F. has seen carp above a yard long, and of 25 pounds weight; but he had no opportunity to ascertain their real age. In the pond at Charlottenburg, a palace belonging to the King of Prussia, he saw more than 2 or 3 hundred carp between 2 and 3 feet long; and he was told by the keeper, they were between 50 and 60 years standing: they were tame, and came to the shore to be fed; they swallowed with ease a piece of white bread, of the size of half a halfpenny roll.

During winter, ponds ought to have their full complement of water; for the deeper the water is, the warmer lies the fish. In case the pond be covered with ice, every day some holes must be opened, for the admission of fresh air into the pond, for want of which carp frequently perish. In the summer, observe to clean the rails and wire-works in the water-courses, of the weeds and grass, which frequently stop them up. Birds that feed on fish must be carefully kept

* I have reason to think that the common carrion crow should be added to the list of birds, which Mr. Forster has before supposed destroy fish when in shallow waters, as I once saw this bird taken by a trap, which was baited with a fish for a heron. D. Bar.—Orig.

out of the ponds. In a great drought, provision ought to be made, to keep the water at the same height as it commonly stands in the pond, i. e. between 4 and and 5 feet. If the water stagnate and grow putrid, it must be let off, and a supply of fresh water be introduced from the reservoirs. If the weeds, especially reed and flags, and some of the aquatic grasses, over-run too much the pond, scythes fixed on poles of 16 or 20 feet, with a lead fastened to them to keep the scythes on the bottom of the pond, are thrown out, and then again drawn to the person that works with them, by which the weeds will all be cut; after which operation, they must be drawn up by long harrows, and laid in heaps on the shore for putrefaction, and in length of time, for manure. This cleaning of ponds must never be done in a spawning-pond, where it would be the destruction of thousands of fish.

Autumn is the best season for catching such carp as are intended for the market. After the pond has been for 5 or 6 years in constant use, it is likewise time to let the water entirely off, and clear the pond of the mud, which often increases too much, and becomes a nuisance. When the pond is dry, it may be ploughed before the frost sets in, and next spring oats or barley should be sown in it, after a new ploughing; and it will repay the trouble to the owner with a rich and plentiful crop. When the loose superfluous mud is carried off out of the pond, care ought to be taken not to take the soil below the original level of the pond. Some people sow a pond, which has been laid dry for some months, with oats; and when growing, they fill the pond with water, and introduce carp for spawning, and think, by this contrivance, to procure food for the fish and something to rub their bellies against. But this practice seems to be more noxious than beneficial; for the growing oats will putrefy, and communicate putridity to the water, which can by no means be salutary to the fish.

The epicures sometimes feed carp, during the colder season, in a cellar. The following method is the best that can be observed for that purpose. A carp is laid on a great quantity of wet moss, spread on a piece of net, which then is gathered into a purse, and the moss so contrived, that the whole fish is entirely wrapt up in it: however, care must be taken to give the fish ease, and not to squeeze it, so that it may have room to breathe in this confined attitude. The net with the fish and moss is then plunged into water and hung up to the ceiling of the cellar. In the beginning, this operation must be very frequently repeated, at least every 3 or 4 hours; by length of time the fish will be more used to the new element, and will bear to be out of water for 6 or 7 hours.* Its

* It is known to every one that a carp will live a great while out of water; but perhaps it may not be so notorious, that the keeping him several hours in the common air, without any precautions, may be repeated from day to day, without any apparent inconvenience to the fish. There is a fishmonger near Clare-market, who in the winter exposes for sale a bushel at least of carp and tench,

food is bread soaked in milk, which in the beginning must be administered to the fish in small quantities; in a short time the fish will bear more and grow fatter. Mr. F. saw the experiment tried in a nobleman's house, in the principality of Anhalt-Dessau; and during a fortnight he visited the fish every day. After the fish had been kept in the above manner during a fortnight, it was dressed and served up at dinner, when every one present found it excellent in its flavour.

XXXVIII. Of the Remarkable Cold observed at Glasgow, in the Month of January 1768. *By Mr. Alexander Wilson, Professor of Astronomy at Glasgow.* p. 326.

While in bed, on Sunday morning, Jan. 3, 1768, about 8 o'clock, Mr. W. felt unusually cold. A little while after, on reaching out for a decanter which he placed near him the preceding night, with some water in it, he was surprized to find the surface of the water frozen over, the like not having happened before in that place. On this he desired his son to try the cold by a thermometer. The experiment was soon after made, by exposing a thermometer at a high north window, and free from the walls of the house; in which situation it had not remained a quarter of an hour, when they found the mercury had fallen to 5° of Fahrenheit's scale. Being satisfied, by another thermometer, that there was no fallacy in this preliminary observation, it naturally occurred, that the cold, however intense it now was, might have been much more so at some earlier hour of the morning. But how to ascertain this, and to recover the lost observation, was the difficulty. In the eagerness of disappointed curiosity, they were disposed to magnify this golden opportunity, which had now escaped them, and to reflect on it with regret, when luckily a little invention helped them out. A notion suggested itself, that if they went very warily to work, they might perhaps surprize those imagined colds still lurking under the surface of the snow, which at that time lay thick upon the ground.

Mr. W. immediately repaired to the fields, and sought out a low place, on which the sun had not then risen; here he laid the thermometer in the snow, almost on the very surface, when presently the mercury sunk from + 6 deg. to — 2 deg., which therefore he concluded to have been pretty nearly the coldest temperature of the air over night. The next thing was, to make regular observations with the thermometer, so long as the cold promised to continue remarkable. The instrument was hung upon a pole near the observatory, and to

in the same dry vessel: but a small proportion of these can be sold in a day; and I have frequently been informed, that the fish continue in good health, notwithstanding their being thus exposed to the air 6 or 7 hours for several successive days. D. Bar.—Orig.

the windward of it, care having been also taken to keep it under a proper shade, so long as the sun shone out.

Register of the Thermometer, kept at the college of Glasgow, on Sunday, Jan. 3, and Monday Jan. 4, 1768.

Sunday morning	10 o' clock	+ 5 deg.
	11	7
	12	9
afternoon	1	10
	2	11½
	3	9½
	3½	6½
	4	3½
	4½	2
	5	1½
	5½	2½
	6	1½
	6½	0½
	7	— 1
	7½	— 0½
	8	— 0½
	8½	— 1
	9	— 2
	9½	— 1
	10	— 2
	10½	— 2
	11	— 2
	11½	— 1
	12	— 0
	12½	— 0½
Monday morning	1	— 1
	2	— 0
	2½	+ 3
	3	6
	3½	7
	4	9
	4½	10
	5	12

The temperature of the snow on Sunday morning, at about ten inches below the surface, was near to 30 deg.

It was observable, that after sun setting, the atmosphere had a tendency sometimes to turn a little foggy, and again quickly to clear up, balancing as it were between these two different states. It is worthy of notice, that the minute variations of the thermometer, as set down in the above register, seemed to depend on these different constitutions of the air; the mercury always rising in the thermometer a small matter, when the mistiness came on, and vice versa.

In the intervals of observations, they made some other experiments, which the present intensity of the frost suggested; particularly one relating to the evaporation of ice, which was tried in the following manner. Mr. W. took a square reflecting metal belonging to his 2-foot telescope, and exposed it on the ballustrade of the observatory, till it had acquired the temperature of the place, which was then at 0 deg.: after it was thus cooled, he breathed on it repeatedly, till its polished surface was covered over with an incrustation of ice or frozen vapour, of a very palpable thickness. In this condition the speculum was replaced in its former situation, having its incrusted surface exposed to the still open air, when, in a little time, they found the frozen pellicle beginning to disappear at the outer edge, all around, leaving the metal quite clear. Gradually more and more of the speculum was bared in a regular progression, from the circumference towards the centre; and at last, in about 50 minutes, the whole surface had parted with its ice. This experiment was repeated when the speculum was defended from the open air, by a large thin box, with a cloth over it. The event turned out the same as before, only it required longer time.

This progress of the evaporation, from the outer parts towards the centre of the speculum, was probably owing to the original plate of ice being thickest towards the centre, a circumstance which might arise from the manner of fixing it at first breathing on it. Or perhaps it may be imputed to some more curious

cause, and may be some effect of the repulsive force belonging to the polished surface; but this point they did not sufficiently examine into, by a due repetition of experiments. Mr. W. just mentions, that partly with a view to this matter, they exposed as above, a set of bodies, having their surfaces of different degrees of polish, and as equally covered with frozen moisture as they could judge. The result of which experiments seemed to favour the idea of the ice being less attached to the more polished surface than to the coarser. This appeared particularly in the case of a comparison made between the speculum above mentioned, and the brass end or cover of the same telescope; for the ice was found still to cleave to its surface a good while after the speculum was entirely cleared.

XXXIX. Some Experiments on Putrefaction. By F. L. F. Crell, M. D., and Professor of Chemistry at Brunswick. p. 332.

The celebrated Lord Bacon (Nat. Hist. Cent. 4.) has doubtless shown a very great sagacity, in pointing out to posterity, putrefaction, as a subject worthy of being further inquired into; and as there happen daily so many changes, not only in the inanimate, but also in the animate world, carried on by its means; the knowledge of every thing relating to it must clear up a great many points in natural philosophy, not thoroughly understood before. But these inquiries ought to be of still more consequence to mankind, as health depends greatly on keeping in due bounds putrefaction, which the body naturally tends to. For these reasons, Sir John Pringle deserves, besides his other eminent merits, very great praise, on his having made many experiments on this subject; and medicine is indebted to him for considerable improvements resulting from them. He has besides opened the way to many other gentlemen, among whom excel Dr. Gaber, and Dr. M'Bride, whose numerous experiments show the ingenuity and sagacity they are possessed of: but the subject is not yet exhausted, nor will it be very easily. I have made some experiments relating to it; and should be very glad if they threw a new light on some points of the greatest importance to medicine.

Dr. Gaber has proved, by his experiments, the presence of a volatile alkali produced by putrefaction; but as he did not discover by the same proceedings * any in its beginning or end, though there was a very putrid smell, he denies its existence in these states, and concludes that this volatile alkali is not a necessary product of putrefaction.† This doctrine seemed not quite conformable to the

* Acta Taurinens. vol. i. p. 78. Cum attegerint summum effervescentiæ gradum, continuato ejusdem loci calore effervescentiæ vim amiserunt. P. 79. Citius plerumque prodiit foetor, quam alkali, idemque tardius desiit. P. 82. Massam inde relinqui foetentissimam, sed emisso alkali ad effervescentiam ineptam.—Orig.

† Id. p. 83, 15. Quum foeteret gravissime residuum destillationis, quamquam omni alkali orbatum, manifestum videtur, ab alkali foetorem exaltari quidem posse, et magis penetrantem effici, non

phenomena: for as all smell, as much as we know at least till now, depends on a saline matter, joined with a phlogiston, and the saline matter producing the putrid stench, was not very likely an acid; I supposed it to be a volatile alkali, which involved in phlogistic matter might fly off, before the alkali was developed. I wanted to know, by experiment, if I was right; for this purpose I put, the 19th of June (the thermometer being 58° of Fahrenheit, and continuing between 58° and 62° all the time I observed), in a pretty large receiver, some beef cut in very small pieces; I covered the bottom with it thinly, and poured upon it water, about 2 inches high. The 22d, the putrid smell was very sensible: but I let it stand till the 24th, when I poured off the fluid,* adding again about the same quantity of water to the flesh. I filtrated then the fluid through a piece of fine linen, and mixed with some of it the syrup of violets, which it did not alter; neither did it effervesce with the spirit of vitriol, diluted to a sharpness near that of the vegetable acid. I thought of keeping it in digestion for some days; but for fear that some little solid particles might have passed through the linen, and by that means, in growing putrid, might give some alkali, and render the trial inaccurate, I distilled the fluid by a heat of about 160°, after which I repeated the trial with the syrup of violets and the spirit of vitriol; but it produced no change. I then put it, the 25th, into a retort, fitted to it a receiver, applied to the jointure a ring of paste made of flour and water, covered it with a piece of wet bladder, and exposed it in a balneum arenæ to a heat of 108° to 116°, till the 29th of June, when the whole fluid was distilled over. I perceived during this operation, that the liquor, from being quite transparent, became turbid; the first distilled transparent fluid grew also turbid in the receiver, and at the bottom of the retort there was a small settlement of a whitish earth. The liquor had a particular smell, but quite different from a putrid one, inclining to the volatile alkali; and showed a slight but sensible degree of effervescence with the spirit of vitriol; and the syrup of violets was turned evidently green by it.

In the mean time, the flesh with the water continued to emit a putrid stench; and the 28th of June I found the fluid colouring the syrup of violets greenish, and showing a kind of effervescence with the acid. Both these qualities were

autem ab eodem produci, quandoquidem superest eo sublato—16. Videtur is odor a volatilibus admodum particulis proficisci, sed quæ ab alkali dissimiles sunt, plerumque citius gignantur, tardiusque dissipentur—alcalescentia adesse potest modico foetori conjuncta—vicissim maximus foetor absque alcali—Ex quibus differentia inter foetidas alcalinasque partes confirmari videtur.—P. 84, 17. Videtur alcali non esse productum necessarium putrefactionis neque gradum alcalescentiæ gradui putrefactionis respondere.—Orig.

* It requires some attention to find out the proper time when to pour off the liquor; if it be done too soon, it will give too little volatile alkali to be much sensible by experiments; for though it smells strongly, it is known how little matter is required to produce a strong smell. If it is delayed too long, it shows already signs of an alkali. For that reason, I made many experiments in vain.—Orig.

increasing every day, till the 8th of July, when, on account of a journey, I could not observe it any longer. I had left the mouth of the receiver open; and on my return the 1st of August, I found an exceedingly putrid smell; I covered the vessel; and the 2d, examined the fluid, but it did not effervesce any more. I then filtrated the liquor; but the flesh was so rotten, that a great many particles passed through the linen, and rendered it turbid. I put it into a retort, adapted a receiver, and luted it, as before mentioned; the heat was also the same, between 108° and 116°. In this warmth it continued for about 4 days, when the fluid was distilled over. On opening the vessels, the smell was again entirely changed, not near so disagreeable as before. In the receiver I obtained a fluid, which turned the syrup of violets green, effervesced very smartly with the very same spirit of vitriol I had used before; gave the smell of a volatile alkali, on adding to this the fixed alkali; precipitated the calces of metals dissolved in acids, and showed itself by all proofs a true volatile alkali. In the retort remained a yellowish matter, almost without any smell. I put to it some water; and after 24 hours it gave the herbaceous smell, but showed no signs of any alkali. I let it stand 4 days longer: the herbaceous smell continued; but there was no alkali to be discovered. I distilled it with a gentle fire: but neither then did there appear an alkali;* and by applying a stronger fire, I got nothing but a kind of empyreumatic oil.

The 3d of August, I had poured some fresh water on the putrid matter; its putrid smell continued; the 7th I decanted the fluid, filtrated it, and made it undergo the same operation, with exactly the same effect as before; which I did again the 11th, with the very same effect. I did not repeat it oftener, as I had occasion for this putrid flesh for some other purpose.

These experiments show I think that the volatile alkali is present, as long at least as the putrid smell continues; and that this volatile alkali is the basis of it, because, as this was distilled over, the residuum, being still in intestine motion, got only the herbaceous smell. The reason why the volatile alkali has been distinctly observed at a certain period of putrefaction, and not in others, is I believe this; the volatile alkali has it seems a tendency to disentangle itself, by intestine motion, of all such matter as it is involved with; but if it is not combined with such fixed matter as retains it till it has gone through all its evolutions, it is, being itself volatile, carried off by the still more volatile phlogistic matter with which it is commonly joined. For this reason, I suppose the putrefying matter shows in its beginning no sign of a volatile alkali; because its smell depends only

* What this herbaceous smell depended on, I did not inquire any further, as not relating to medicine, since a living body never was found in such a state: but very likely it depends on some volatile alkali, which is perhaps in so very small a quantity, as not to be perceptible by experiments.—Orig.

on those particles which have been on the surface, without any strong cohesion with the substance. In the further progress of putrefaction, the matter involving the alkali, or forming it, is intermixed, and in cohesion with the solid particles of the substance, and is by these means retained till the alkali is come to its purer state. Towards the end of putrefaction, the cohesion of the particles being almost entirely taken off, the volatile alkali is carried off before it can go through all its states.

If it is therefore true, that the volatile alkali is essential to, or at least always present in putrefaction, it seems to follow, that the alkalies never can be used in living bodies as antiseptics,* for setting aside their stimulating quality, which must prevent their use in most of the putrid diseases, they would increase the morbific matter, by being intimately mixed by circulation with phlogistic matter, which they find in abundance in such bodies. It has been objected to this that the exhalation of stale urine, though showing a great quantity of volatile alkali, is inoffensive to health:† and that some persons have taken the volatile alkali in very great quantity, without its bringing on a putrid disease:‡ but there are however some examples,§ where it has been hurtful. It is urged further, that a person, being only for a short time exposed to really putrid exhalations, may be infected with putrid diseases; and therefore that this effect of putrid exhalations does not depend on the volatile alkali, as it may be taken pure, in very large doses, without producing such effects. To this I reply, by an analogous instance; a small quantity of ferment will bring on fermentation in a large mass of fermentable matter, and yet as much acid as could be obtained from the fer-

* It appears very difficult to account for the antiseptic power of the volatile alkali, and other salts, on dead animal substances: I once thought, that as the ammoniac salt, nitre, &c. bring down the thermometer several degrees, perhaps all these salts acted by instantly absorbing the heat produced by the beginning intestine motion; and that, as a certain degree of warmth is necessary to putrefaction, in preventing this degree from coming on, it might hinder the whole operation. To see by experiment how far this might be true, I put into phials a certain quantity of water, with that proportionate quantity of alkalies, fixed and volatile, sal ammoniac, &c. which Sir John Pringle had found (Append. p. 16, 17) to be antiseptic; and in one as much pure water as a standard. I stopped every one of them with a cork, in which I had made a hole for a thermometer of Fahrenheit. I exposed all these phials to the same heat; Sir John had used about 112°; but I found, that both those with the salts, and that without it, marked the same degree of heat; and that therefore the absorption of heat can by no means be the reason of the putrefaction being stopped. May this phenomenon not depend on the salts penetrating the body, and giving to the particles more puncta contactus, according to their greater or less affinity? And may not these salts, in augmenting cohesion, hinder the fluids from separating themselves from one another, and in consequence prevent intestine motion? Is this not somewhat confirmed by the action of astringents? and by the most powerful actions of metallic salts, as being of the greatest specific gravity?—Orig.

† Sir J. Pringle, Append. p. 7.—Orig. ‡ Id. ibid. p. 92.—Orig.

§ Huxham on the sore throat, p. 67, 68. Ejusd. Essay on Fevers, p. 118, edit. 5.—Orig.

ment, far from exciting an intestine motion in the fermentable matter, would rather check it; but can it for all that be denied, that the involved acid in the ferment is the chief cause of setting the whole mass in fermentation? In the same way, the alkali combined with phlogistic matter may produce such intestine motion as the pure alkali cannot; and very likely the first would not produce it, if the volatile alkali in it could be changed.

To bring this about, the most powerful means seem to be the use of acids; and the most celebrated physicians agree in the good effect they have observed from acids in putrid diseases, and recommended them strongly. Dr. M'Bride thinks otherwise, and his reasons are these: first that if the acids came unchanged to the absorbent vessels, they would not admit of them;* 2dly, if they did, they would be dangerous;† and 3dly that they are quite changed, before they leave the primæ viæ.‡ As for the first, I do not know what reasons Dr. M'Bride founds his assertions on, as acids never are given in so concentric a state, as by their astringency to make these vessels shut up their orifice; and as metallic salts themselves are absorbed in their very compound state (which seems clear with regard to the corrosive sublimate, and other such saline preparations), I do not see why the simple acids could not be absorbed. The 2d reason seems to be founded on some of Dr. M'Bride's experiments (p. 132, 133), viz. that putrid flesh, sweetened by distilled vinegar and spirit of vitriol, was firm; but on being boiled went quite to pieces, whereas that sweetened by volatile alkali did not. But I conceive these experiments are not applicable to a living body: for the acid being there mixed with the fluids, cannot act in this way on the solids, till the fluids are (if I may use that expression) supra-saturated with the acid, § which in putrid diseases cannot be the case. And further, a heat of 212° of Fahrenheit never can increase the action of the acids in living bodies, as it did in the experiments; for though Dr. M'Bride denies this consequence, and will prove the contrary, as the flesh with the alkali did not dissolve; yet this circum-

* Experimental Essays, edit. sec. p. 20. The austere acid (generated in the first passages of weakly persons) is exactly in the same state with a foreign acid, for the lacteals will admit none of it.—Orig.

† Ibid. p. 134, the acids dissolve the elementary earth, and thus destroy the texture of that soundness they are supposed to restore.—P. 148, we are not to expect that they are to pervade the minute branches of the vascular system; when indeed it is evident, that they ought not to be allowed to pass into the blood in their acid form; since it is plain, that from their dissolvent nature, the body must be destroyed, and its most solid parts melted down to a jelly, if naked acids were to be received into the general mass of fluids.—Orig.

‡ Ibid. p. 148, acids are neutralized during the alimentary fermentation; and therefore they cannot act as acids, by saturating any thing of the alkaline kind that they meet with in their course of circulation.—Orig.

§ This has, it seems, happened in some rare cases quoted by Dr. M'Bride, and Dr. Haller, p. 148.—Orig.

stance proves nothing more, than that the volatile alkali has not such power of dissolving the gluten of animal fibres as acids have; for if the effect depended only on the action of the acids by themselves, the flesh would rather have been dissolved when immersed in them, than when boiled in water. The doctor besides seems not quite consistent on this head; for, p. 151, he says, 'Adstringents can only be of importance in those cases, where, from extreme relaxation and resolution of the solids, the dissolved fluids are suffered to transude, and either form spots of different hues, or run off by actual hæmorrhage; here indeed the acid of vitriol, as an astringent, not as an acid, is found of great use in gaining time.' As the acid could not exert its astringent power on the vessels, without coming to the secundæ viæ (p. 153), he seems not afraid, in this case, of its melting down the most solid parts to a jelly.

In proof of his 3d reason, he alleges some experiments; viz. the 3d, p. 40, where a mixture of flesh, bread, lemon-juice, and saliva, did not effervesce, after fermentation with an alkali; and the 5th, p. 42, where a mixture of bread, water, saliva, and spirit of vitriol effervesced smartly, before the intestine motion; but not at all after it. I could object against these experiments, and especially the 5th, that perhaps the proportion of the saliva to the acid was too great, and that a person in a putrid disease ought to take more acids than could be neutralized by the inquiline liquors. However, I will not insist on this; and suppose these experiments to be quite applicable to the case: but if these mixtures do not effervesce any more, does it follow, 'that they are neutralized, and therefore act as acids, by saturating any thing of the alkaline kind, that they meet with in their course of circulation?' There are some saline bodies, which do not effervesce when mixed together; which will however change each other's nature. Thus f. e. brimstone, mixed with a strong fixed alkali, does not effervesce,* but changes, on being dissolved, the nature of the alkali. A solution of soap does not effervesce on the addition of an acid, but joins with the acid, and neutralizes it. These instances made me suspect the conclusion drawn by Dr. M'Bride from his experiments; and to clear up these doubts, in this particular case, I referred to experiments. For this purpose, the 4th of August, the thermometer being at 64°, I mixed 3 oz. of saliva, a dram of the liquor of putrid flesh, and a very small quantity of bread: and added as much of the diluted spirit of vitriol, as to make it sour, and effervesce definitely with the alkali. There was not any sign of intestine motion till the 7th of August, when from time to time some air-bubbles, and also some solid particles, rose to the top; and this continued till the 8th. Not perceiving any further motion, I poured off the

* This applies also to the solution of brimstone in lime-water, out of which the lime particles have been precipitated, by the introduction of fixed air.—Orig.

clear liquor, which did not effervesce any more with the alkali. The 9th, I mixed 6 drams of the putrid liquamen, with about double of this liquor, and put in besides 4 solid pieces of flesh, which had lain 3 days in the liquamen: these pieces had a prodigious stench, and so rotten, that with the least force they were torn to pieces. There appeared no signs of intestine motion: the 10th, the putrid smell was very much abated: the 11th, it was changed, and there remained only a smell much like that of sound flesh: the pieces were without any smell, and had acquired again some degree of firmness. In this condition they remained for a week, and I did not observe them any longer.

This experiment proves, I believe, that acids, though changed in the alimentary canal so far, as not to effervesce with alkalies, may notwithstanding check putrefaction; and that therefore their use is of great consequence, and ought not to be omitted in putrid diseases. Though Dr. M'Bride believes that these diseases may be cured with fermentable substances only; I must own that I do not agree with him, and am not quite convinced of his opinion, that putrefaction depends only on the loss of fixed air. I rather believe this an effect, than the cause, of putrefaction; but I shall refer this subject to another occasion.

XL. Observations on Five Ancient Persian Coins, struck in Palestine, or Phœnicia, before the Dissolution of the Persian Empire. By the Rev. J. Swinton, B.D., F.R.S. p. 345.

These coins, as well as several others similar to them, were undoubtedly struck, in some of the cities of Syria, Palestine, or Phœnicia, before the reduction of those provinces, and the conquest of the Persian empire, by Alexander the Great.

1. The first of these 5 medals was brought to England, out of the East, by the Rev. Tho. Crofts, chaplain to the British factory at Aleppo. On one side is Atergatis, Adergatis, or Derceto, taken by several learned men, for the Dagon of Scripture, nearly as we find that pagan divinity described by Diodorus Siculus, and Lucian, with a pigeon before her, and a fish in her right hand. On the other, we perceive a galley, or small vessel, on the sea, with rowers in it; under which there appears a sea-horse, or rather a sea-monster, of a very particular form. Near the face of Adergatis, the two Phœnician letters, answering to MA, present themselves. The piece is in good conservation, having suffered very little from the injuries of time.

That this silver medal must have been anterior to the dissolution of the Persian empire, we may fairly collect from the reverse; which agrees in every particular, but the sea-horse, with the reverse of a Daric, that undoubtedly preceded the above-mentioned event, and exhibits the very same Phœnician letters with which it is adorned.

That this piece was struck at Ascalon, a very ancient and celebrated city of Palestine, there is, Mr. S. thinks, little reason to doubt. Dagon, or Atergatis, was a deity of the Philistines, to whom Ascalon appertained, as we learn from Scripture; and therefore may very naturally be supposed to have been worshipped there, as well as in the other principal cities belonging to that people. We are assured by Diodorus Siculus, and Lucian, that Ascalon was famous for the worship of Atergatis, or Derceto, and the superb temple of that deity there. The coins of Ascalon not unfrequently exhibit Atergatis, with a pigeon, as here; pigeons as well as fishes having been considered as sacred animals, bearing a near relation to Atergatis, if not as objects of religious worship, in that city.

As no chronological characters on the piece in question present themselves to our view, it will be extremely difficult, if not impracticable, to ascertain, with any precision, the time when it first appeared. However, Mr. S. thinks it probable, that the coin was struck about 351 years before the birth of Christ, when the provinces of Palestine and Phœnicia were subdued by Artaxerxes Ochus, soon after they had revolted from him.

With regard to the two Phœnician letters exhibited by this coin, they seem either to form the word מא, MA, which in Phœnician not improbably denoted WATER, or the SEA, as in Arabic, or to be the first 2 elements of the word MAIUMA, in Syriac, also signifying WATER, the name of the port and place of the magazine of naval stores, such a port and place having formerly appertained to Ascalon and Gaza.

2. The second medal also Mr. Crofts brought with him from Syria. Atergatis, or Derceto, on this silver plate, holds a concha-marina, or sea-shell, in her left hand; but in all other respects it is so similar to the former as sufficiently appears from the draughts of them both, that it may almost, if not absolutely, pass for a duplicate of the same coin. The piece however has been but indifferently preserved; so that without the assistance of the medal already described, it would have been of no great service to the learned world.

3. The 3d medal is a very small silver piece, and was also, by Mr. Crofts, brought with the other two, above described, out of the east. The reverse, which exhibits the two Phœnician elements MA, and a galley, or small vessel, full of rowers, on the water, almost entirely agrees with that of two Persian Darics. This indicates the piece to have been struck in Palestine, or Phœnicia, before the dissolution of the Persian empire, probably at the same time that the two former first appeared. On the other side is a laureated ancient head, which he takes to represent Jupiter Marnas, a deity worshipped at Gaza, a celebrated ancient city, at no great distance from Ascalon.

4. The 4th is a small brass medal, that may pass for an inedited coin, though one not unlike it has been published by M. Baudelot. On one side is a human

figure, probably representing a King of Persia, with a Persian tiara on its head, in a triumphal car, drawn by two horses, and driven by a similar figure, with a Persian tiara likewise on its head. On the other, a vessel navigated by rowers, resembling that exhibited by the 3 foregoing coins. The piece has been well preserved, and was undoubtedly anterior to the reduction of Syria and Phœnicia by Alexander the Great. For that the person in the car is a Persian, we may infer from the tiara on his head, which occurs on the heads of several Persian figures in the ruins of Persepolis; and that he was a royal personage, appears from hence, that the kings of Persia only had their effigies impressed on the Persian coins.

That the piece then was struck in Palestine, or Phœnicia, while under the domination of the Persians, there is he thinks little reason to doubt; though it may perhaps be not altogether so easy to ascertain, with any precision, the time when it first appeared. There is however one period, and one only, he apprehends, in the Persian history, to which this may, with the strictest propriety, be referred; and that is, immediately after the reduction of Sidon, by Artaxerxes Ochus, when the Phœnicians, who had before entered into an alliance with Nectanebus, King of Egypt, and asserted their independency, made their submission to him. This happened in the year of the Julian period 4363, about 351 years before the birth of Christ.

5. The fifth medal is extremely similar to the 4th, but very ill preserved. The former however differs from the latter in this, that it exhibits a lacquey, or slave, as it should seem, following the triumphal car. This renders it still more probable, that the figure in that car was intended to represent a person of the first distinction, or rather a Persian monarch.

XLI. On some Plants found in Several Parts of England. By Richard Hill Waring, Esq. F. R. S., p. 359.

Mr. W. here gives a catalogue of some indigenous plants, in places not heretofore mentioned, in the counties of Salop, Stafford, Chester, Flint, Denbigh, Carnarvon, and Merioneth, that are scarce in this island, or have been generally supposed to be so, or not indigenous; and occasionally of such as are scarce in other counties; and some, that though common in some other counties, are scarcely or not at all to be found in these; and also of such as may be doubtful, perhaps originally foreign, though generally supposed to be natives of Britain.

After the catalogue (which it was deemed unnecessary to reprint, as the plants therein enumerated are found described in the systems of botany published by Hudson, Withering, and Smith) Mr. W. subjoins the following remarks:

" Upon the whole, it may be difficult to determine what plants, if any, are originally British. With regard to biennials, if there has been immemorially a

constant annual flowering in waste places, or in ground that does not appear, or is not known to have been cultivated for the purpose, it may perhaps be reasonably presumed that they are the natural and spontaneous product of such places; for, in this case, I understand natural and spontaneous, according to common acceptation, to be synonymous, and applicable to any seminal production, however happening, or effected, without the assistance of art, whether from seeds deposited there, or in that soil, at the creation, or from such as are conveyed by the wind, by birds, or any other casual means. Otherwise, in strictness, there may be no such thing in nature as a spontaneous production; for as to the old doctrine of equivocal generation, I suppose it to be universally exploded; though I do not dispute that the stamina, or first rudiments, have existed, in the parent plants, from the beginning of the things, the vegetative principle being latent, till prepared and at liberty to exert itself.

And on the first conjecture, a difficulty may arise. It is perhaps not easy to conceive that the fecundity of seeds, once perfected, can be retained inert through many ages. Experience seems to show, that there are some kinds of seeds, that at a certain age, or nearly so, either vegetate or perish; that if kept out of their proper matrix, or in at too great a depth, beyond that time, whatever we do with them afterward, will not grow; and if there be really such (so deposited ab origine) those kinds cannot, even in that sense, and in that case, be said to come up spontaneously. Besides, if the seeds were so deposited in the earth, and in a perfect state, so numerous as they must be; the larger kinds especially could not escape our notice. As to the antediluvian nuts, cones, and stone-fruits, that, we hear, are sometimes found at vast depths within the earth, however they may suit the cabinets of the curious, I fear they are too antique to be prolific. But in the other way, the seeds may be conveyed, from whatever distance, in different years, (for aught we know they are in every year), to the places where we see the plants; and not only thither, but to many places that are not proper to receive and cherish them.

It is evident, that the oak, ash, and other our most common trees, are not naturally increased in any other way, except such as are productive of suckers at a considerable distance from the stems; and many of these do not generally perfect their seeds: to say nothing of inferior plants, that sometimes, in the phrase of gardening, lay themselves. But those suckers, till parted from the parent trees, and removed from the place, are not often better than underwood, which may be one reason why these kinds do not increase so extensively as the former. And if our forefathers had not industriously raised and increased (if not previously introduced) the most common and most useful trees, perhaps we should not observe them to increase naturally more, or have found them more numerous, than many that we know to be exotic, and yet are as easily increased,

and do of themselves increase as fast, proportionally, and are as hardy, as any trees we have. Yet it is not to be expected, that these of exotic origin, more than those that have been long familiar to us, should increase alike in all soils, or in all counties, since there are some soils that are far from being general.

Mr. Da Costa, in his Nat. Hist. of Fossils, observes, that "Chalk is found chiefly in the south-east part of this island," so that, "if a straight line were drawn from Dorchester (in Dorsetshire) to the coast of Norfolk, it would almost include our chalky strata;" and I believe his observation to be just, except that, though the line be drawn even to the most western part of that coast, this soil extends considerably beyond it, into Wiltshire. We know that, of all soils, this is the most favourable to beech, white-beam, juniper, viburnum, traveller's-joy, and to many of the herbaceous tribe; though not only such, but many foreign plants will increase in soils that are not the most suitable to them.

In the woods here, and at a distant place, I find, not unfrequently, seedlings of the Scotch-pine (which whether indigenous of Scotland, or not, may be doubtful), spruce-fir, horse-chestnut, walnut, and perhaps more than I can at this time recollect. Of the 4 kinds mentioned, some trees, notwithstanding the tread and the browsing of cattle, now grown to a considerable height, I am certain were not planted. Of the first 3 there are many not far off, that were planted, and probably may in most seasons bear perfect seeds: but of the walnut I do not know that there is, or has been, within half a mile of the first-mentioned woods, a tree that has produced a nut mature enough for vegetation. It is, however, easy to conceive, that the nuts may have been brought from a much greater distance by birds, or other animals, and dropped accidentally, or hoarded and forgotten, or perhaps not needed. In a shrubbery, many years left to nature, I have observed very numerous progenies of various foreign shrubs, both from the seeds and roots; and it is well known to gardeners, that many of their once choice flowering herbs are apt to multiply in the way of suckers, while the seeds of others sow themselves so plentifully, as not easily to be kept within bounds. It therefore seems to me not unlikely, that all these kinds, and many more perhaps yet unimported, may in future ages be so far naturalized as to be deemed indigenous of this land."

XLII. A Catalogue of the Fifty Specimens of Plants, from Chelsea Garden, presented to the Royal Society, for the Year 1770, pursuant to the Direction of the late Sir Hans Sloane, Bart., from the Society of Apothecaries, London: By Stanesby Alchorne, Member of the said Society. p. 390.

The 49th presentation, making 2450 different plants.

XLIII. Astronomical Observations made, by Appointment of the Royal Society,

at King George's Island in the South Sea; by Mr. Charles Green, formerly Assistant of the Royal Observatory at Greenwich, and Lieut. James Cook, of His Majesty's Ship the Endeavour.* p. 397.

Of these observations, the first is a series of equal altitudes of the sun, for the time, made with the astronomical quadrant: from the whole of these it is inferred,

* This gentleman was Capt. James Cook, the celebrated circumnavigator, who made, by authority, three voyages round the earth; in the last of which he was killed by the natives of one of the Sandwich Islands, the 14th of Feb., 1779, at 51 years of age. An ample account of the life of this extraordinary man is given in the Biographia Britannica. The following are a few particulars of him. He was born at Marton, in Yorkshire, in 1728, of parents in humble circumstances; and at an early age he was apprenticed to a shopkeeper at Snaith; but afterwards bound himself to a ship owner in the coal trade at Whitby, in which line he served many years. But, on the breaking out of the war in 1755, he entered on board a man of war; where distinguishing himself, by his good conduct, in 1759 he obtained a master's warrant. In that capacity he served at the reduction of Quebec, where he took the soundings of the river St. Laurence, and made an accurate chart of it. He next served at the retaking of Newfoundland, where also he made a survey of the coast, with other curious researches, and observed there an eclipse of the sun, Aug. 5, 1766, printed in the Philos. Trans., vol. 57. In 1768, with the rank of lieutenant, he was appointed to the command of the Endeavour, accompanied by Mr. Green, astronomer, to observe the transit of Venus at Otaheite, in the South Seas; and an account of their observations on that occasion is given in the article above. Along with them also sailed Mr., now Sir Joseph Banks, and Dr. Solander. After the transit was discovered, Mr. Cook sailed on a voyage of discovery, in which he discovered and visited a number of new lands; as the Society Islands, New Zealand, New Holland, Botany Bay, &c. In June, 17, 1771, he arrived in England, and was appointed a commander in the navy, an account of the voyage being published by Dr. Hawksworth.

In 1772 he was again sent out on another voyage, with two ships, the Resolution, commanded by himself, and the Adventure, by Capt. Furneaux. In this voyage he explored the southern hemisphere as far as latitude 71° 10′, amidst immense fields and mountains of ice. Capt. Cook then touched at Otaheite to refresh, and hence sailed to the westward, and visited several groups of islands; as, what he called the Friendly Isles, also the islands discovered by Quiros, called by Capt. C. the New Hebrides; also New Caledonia, and Norfolk Island, which has since been colonized. After many other additions to our geographical knowledge, but without attaining the main object, the discovery of a southern continent, he arrived in England, July 1775, having lost only one man, out of 118 on board his ship, owing to the excellent means he employed for preserving the health of the crew. Of these means he gave an account in a paper sent to the R. S., where he was chosen a member of their body, and his paper honoured with the prize medal in 1776. He was also, by the Government, raised to the rank of a post captain in the navy, and appointed to an office in Greenwich Hospital; and an account of his voyage was drawn up by himself and Mr. Wales.

The government having resolved to ascertain whether there be a northern communication between the Atlantic and Pacific oceans, Capt. C. volunteered his services on the occasion, and in July 1776, he sailed in the Resolution, accompanied by another vessel. After touching at some of the South Sea islands, he proceeded northwards, and discovered the group which he named the Sandwich Islands; hence proceeding to the northwest coast of America, he traced along all that coast, as far as latitude 74°, where their progress was stopped by an impenetrable mass of ice, extending between the north-east point of Asia and the north-west point of America. Hence he returned to the southward, and in November 1778, he revisited the Sandwich Islands, where he was unfortunately killed in a quarrel with the natives.

hence the daily rate of the clock's losing on mean time, by a mean of 40 results, is 20.8 seconds. By the first and last days observations compared together, the clock lost $19^m\ 49^s.9$ on mean time in 57 days, which is at the rate of $20^s.88$ or $20^s.9$ per day. The swing of the pendulum on each side of the perpendicular during this time, varied between 1° 50′ and 1° 55.

Remark.—The same clock, when fixed up at the Royal Observatory at Greenwich, before the voyage, with the pendulum of the same length, got at the rate of $1^m\ 45.8^s$ per day, on mean time, between April 19 and July 18, 1768. Therefore the force of gravity at Greenwich is to that at King George's Island, as 1000000 to 997075. *N. Maskelyne.*

Next follows a series of observations of meridian zenith distances of the sun and fixed stars, for finding the latitude of the observatory: from which it is found, that the mean of all the results from the sun and six stars to the North, gives the latitude 17° 28′ 51″ s; and the mean of all the results from the stars to the south, gives the latitude 17° 29′ 38″ s: the mean of these two means is 17° 29′ 15″ s. which may be taken for the latitude of the observatory.

Remark.—It must be confessed, that the results of these observations (most of which were made by Mr. Green) differ more from one another than they ought to do, or than those do made by other observers, with quadrants of the same size, and made by the same artist, the cause of which, if not owing to want of care and address in the observer, I don't know how to assign. *N. Maskelyne.*

The next is a series of lunar observations, that is, of the moon's distances from the sun and stars, for the longitude; the mean from all these give the longitude of the observatory, on George's Island, 149° 36′ 38″ west of Greenwich observatory. To these succeed a set of observations of the eclipses of Jupiter's satellites, for the same purpose; from which it is inferred that the longitude of the same place is 149° 32′ 30″.

Next, the observations of the main object, the transit of Venus, is given at considerable length, by the different observers. And,

1st. By Mr. Green, with a reflecting telescope of 2-feet focus, magnifying power 140 times.

	Apparent time. June 2.		
Light thus on the ☉'s limb, pl. 4, fig. 1	21h	25m	40s
Certain, as in fig. 2	21	25	55
First internal contact of ♀'s limb and the ☉, fig. 4	21	43	15
Penumbra and ☉'s limb in contact, fig. 5	21	43	55
First contact of penumbra, undulating, but the thread of light visible and invisible alternately	June 3, 3	14	3
Second internal contact of the bodies	3	14	51
Second external contact	3	31	28
Total egress of penumbra, ☉'s limb perfect	3	32	14

2. Transit of Venus by Capt. Cook, with a reflecting telescope of 2 feet focus, and the magnifying power 140.

	June 2,		
The first visible appearance of ♀ on the ⊙'s limb, see fig. 1....	21	25	45
First internal contact, or the limb of ♀ seemed to coincide with the ⊙'s, fig. 2	21	43	15
A small thread of light seen below the penumbra, fig. 3........	21	44	15
Second internal contact of the penumbra, or the thread of light wholly broke..........	June 3, 3	14	13
Second internal contact of the bodies, and appeared as in the first	3	14	45
Second external contact of the bodies..........	3	31	22
Total egress of penumbra, dubious..........	3	32	2

The first appearance of Venus on the sun, was certainly only the penumbra, and the contact of the limbs did not happen till several seconds after, and then it appeared as in fig. the 4th; this appearance was observed both by Mr. Green and me; but the time it happened was not noted by either of us; it appeared to be very difficult to judge precisely of the times that the internal contacts of the body of Venus happened, by reason of the darkness of the penumbra at the sun's limb, it being there nearly, if not quite, as dark as the planet. At this time a faint light, much weaker than the rest of the penumbra, appeared to converge towards the point of contact, but did not quite reach it, see fig. 2. This was seen by myself and the two other observers, and was of great assistance to us in judging of the time of the internal contacts of the dark body of Venus, with the sun's limb. Fig. the 5th is a representation of the appearance of Venus at the middle of the egress and ingress, for the very same phenomenon was observed at both: at the total ingress, the thread of light made its appearance with an uncertainty of several seconds; I judged that the penumbra was in contact with the sun's limb 10^s sooner than the time set down above; in like manner at the egress the thread of light was not broke off or diminished at once, but gradually, with the same uncertainty; the time noted was when the thread of light was wholly broke by the penumbra. At the total egress I found it difficult to distinguish Venus's limb from the penumbra, whi h of course made the second external contact a little doubtful, and the precise time that the penumbra left the sun could not be observed to any great degree of certainty, at least by me. Some of the other gentlemen, who were sent to observe at different places, saw at the ingress and egress the same phenomenon as we did; though much less distinct, which no doubt was owing to their telescopes being of a less magnifying power; for the penumbra was visible through my telescope during the whole transit; and Dr. Solander, whose telescope magnified more than ours, saw it, I have reason to think, distincter than either Mr. Green or myself; though we both of us saw enough to convince our senses, that such a phenomenon did in-

disputably exist, and we had a good opportunity to observe it, for every wished for favourable circumstance attended the whole of that day, without one single impediment, excepting the heat, which was intolerable: the thermometer, which hung by the clock, and was exposed to the sun as we were, was one time as high as 119°. The breadth of the penumbra appeared to me, to be nearly equal to ⅛ of Venus's semidiameter.

3. Transit of Venus by Dr. Solander, with a 3-feet reflecting telescope.

	App. time.		
First external contact plainly convex, a wavering haze seen some seconds before			
Ingress, light seen glimmering under Venus	21^h	43^m	28^s
♀'s free from the ☉'s limb	21	44	2
♀'s true limb out	3	31	49
♀'s atmosphere out	3	32	13

4. Observations of the transit of Venus, made by Mr. Charles Green, with Dollond's micrometer fitted to a reflecting telescope of 2-feet focus, gave on the day of the transit, for the diameter of Venus, 54″.9, on a medium of the whole, and that of the sun 31′ 27″.4. At these observations the thermometer stood at 113°.

5. Observations on the transit of Venus, June 3, 1769, by Dollond's micrometer fitted to a reflecting telescope of 18 inches focus, by Capt James Cook. The mean of all these give 56″.4 for the diameter of Venus.

Observations on the Dipping Needle.

Time when.		Place where.	Dip of the north or south point.
1768			
September	13	In Funchal Bay, dip of N. end of needle	77° 18′
October	25	Crossing the line in long. 30° 18′ W. of Greenwich	26 to 28 N. point
1769			
January	10	At sea in lat. 52° 54′ S. and long. 63° 10′ W.	63 S. point
	20	Good Success Bay in Straits Le Maire	68 51 ditto
	24	On board the ship at anchor in the above bay	65 0 ditto
	30	At sea in lat. 60° 4′ S. long. 74° 10′ W.	65 17 ditto
March	3	Ditto ditto, 36 49 S. ditto 111 54 W.	65 52 ditto
	13	Ditto ditto, 30 46 S. ditto 125 28 W.	64 25 ditto
April	5	Ditto ditto, 18 25 S. ditto 140 51 W.	30 0 ditto
May	30	George's island, lat. 17° 29′ S. long. 149° 34′ W.	30 43 ditto

N. B. Each of the above observations is the mean of 10, 12, or more; with the face of the instrument turned alternately east and west: those made at sea are a little dubious on account of the motion of the ship; but, by means of a swinging table we had made to set the compass on, we could, in a tolerable smooth sea, be certain of the dip to a degree, or at the most 2, by taking the mean of a great number of trials.

Lastly are given a set of observations on the tides at George's Island, by which

it is inferred by the astronomer royal, that the mean height of the sides is about 10 inches, and the greatest height scarcely exceeds one foot, in the middle of this wide extended ocean; which falls far short of what might have been expected from physical principles. The cause of this remarkable difference deserves further inquiry. The time of high water also appears to precede the moon's passing the meridian by 45 minutes at a medium, and the time of low water to precede the same, by $6^h\ 31^m$. But the mean difference of high and low water, should be $6^h\ 12^m$, which subtracted from $6^h\ 31^m$, leaves 19^m, by which the time of high water should precede the moon's passing the meridian; the mean of this and 45^m is 32^m, by which the time of high water precedes the moon's passing the meridian, by a medium of all the observations. The times of high and low water seem to be subject to great irregularity on particular days; no doubt owing to the small rise of the water, and the smallness of its force in consequence, which renders it more liable to be disturbed by the action of the winds and other causes; part of the irregularity may be attributed to the difficulty of observing the time of the flood or ebb, with any degree of certainty. *N. Maskelyne.*

N. B. The island here named King George's Island, is called by the natives Ota-heite, by which name it will henceforth be called, the name of King George's Island having been given before to another island in lat. 14 s. discovered by Commodore Byron.

*** Mr. Green having died at sea in the passage home from Batavia, all the astronomical and other observations were partly arranged by Capt. Cook, and partly by the astronomer royal, from the original manuscripts, and calculated by the latter.

XLIV. Variation of the Compass, as observed on board the Endeavour Bark, in a Voyage round the World. Communicated by Lieut. James Cook. p. 422.

These observations were taken from the latter part of 1768, to the middle of 1771: so that the columns of variation may, near enough, be supposed to answer to the middle year 1770.

Latit.	Longit.	Variation.
North	West	
	West from Greenwich.	
English Channel		23° 0′ w
C. Finestre s. by ½ w. 6 leag.		18 42
42° 1′	9 50 w.	21 40
40 29	10 11	21 4
37 4	11 34	19 50
36 46	11 58	21 19

Latit.	Longit.	Variation.
North	West	20° 39′ w
35° 40′	13° 4′	18 32
34 58	13 50	17 27
34 20	14 20	
Funchal		16 0
32 33 33	16 49	18 30
29 40	15 30	17 30 71 00 17 15
		18 30

Latit.	Longit.	Variation.
North	West	
29° 7′	15° 50′	16° 30′
Pico Teneriff, N. 18 E. Distance 140 miles.		16 43
		15 46
26 50	17 12	14 58
25 20	18 50	15 1
20 56	20 40	12 46
19 33	20 50	12 43
18 38	21 (	12 33
15 40	22 0	10 37

Latit.		Longit.		Variation.	
North		West			
14°	35′	22°	8′	10	0′ W
12	24	22	22	8	49
11	53	22	0	6	10
9	45	22	20	8	52
9	42	22	19	9	0
8	46	22	4	8	0
8	12	22	4	8	21½
8	6	22	13	7	48
7	48	22	13	8	39
7	13	22	33	8	54
6	50	23	46	8	40
3	4	26	30	4	2
2	0	27	55	3	17
0	55	28	55	2	24½
South		West			
2	3	31	0	2	48
3	59	32	30	2	25
5	46	32	48	1	31
7	30	33	4	0	15
9	22	33	16	0	58
				0	18
10	3	33	0	0	34 E
12	27	33	0	0	47
15	25	33	40	1	23
18	30	36	10	4	41
20	4	37	18	5	26
21	16	37	50	7	52
Cape Forio W.N.W. dist. 12 leagues.				6	40
Entrance Rio de Janeiro W.N.W. dist. 5 leagues.				7	34
25	44	41	4	8	40
26	0	41	20	8	23
26	34	41	33	8	23
30	20	41	49	9	36
32	30	42	48	11	3
32	54	43	38	11	3
34	34	45	38	13	44
36	50	48	32	15	1
37	8	49	1	16	1
37	8	49	0	15	45
36	46	49	2	15	30
41	40	56	25	16	22
42	40	60	25	18	36
48	42	60	51	20	4½
51	30	63	30	22	24
52	40	65	20	21	57
54	0	67	30		
10 leag. from Terra del Feugo.				23	30
Strait le Mare, ditto.				24	9
56	7	65	45	25	4
55	40			21	0
55	40	C. Horne S.W. 3 W. 8 leag.		21	16
57	0	69	0	22	0

Latit.		Longit.		Variation.	
South		West			
60°	10′	74°	26′	27°	9′ E
59	23	76	45	24	53
58	30	80	58	24	4
49	13	89	36	17	0
48	56	91	27	12	0
48	10	92	0	11	0
44	39	103	0	6	30
39	43	105	52	5	34
39	43	110	26	2	20½
36	49	111	54	2	26
37	8	116	8	3	13
37	24	117	41	4	41
35	30	119	30	1	42
34	0	121	0	4	12
32	40	123	0	4	23
31	20	124	40	3	20
30	56	125	20	3	0
30	30	126	0	3	45
29	36	126	50	3	22
29	32	126	48	1	30
29	28	127	4	2	18
29	10	127	16	3	27
27	40	129	20	3	14
25	21	129	28	3	21½
25	21	129	32	3	10
21	14	127	38	3	56
20	29	127	44	2	27
19	30	129	10	2	25
19	7	131	40	2	32
18	46	138	0	2	54
18	36	139	10	2	54
18	36	139	40	3	30
17	48	143	50	6	32
17	36	145	30	4	54
17	42	146	16	5	41
18	0	147	59	6	30
17	29	149	30	4	45½
17	15	151	41	5	50
21	20	151	15	5	40
22	8	150	55	5	37
23	37	150	37	6	7
26	10	149	46	8	8
26	30	Ditto		7	58
30	43	148	0	7	30
32	40	147	14	7	18
33	8	147	25	6	40
38	3	147	6	7	9
38	29	145	32	7	0
33	0	153	0	8	8
29	0	159	42	8	36
29	0	159	25	8	29
33	30	163	40	10	48
36	50	173	46	13	22
37	6	174	46	12	48
38	33	179	0	12	59
39	0	180	0	14	2

Latit.		Longit.		Variation.	
South		West			
39°	11′	180°	30′	15°	4½
39	37	182	30	14	10 E
40	0	182	0	10	22
36	48	184	12	11	9
35	50	185	15	12	40
35	15	Ditto		13	10
35	0	185	30	11	45
34	42	185	30	12	51
34	40	186	15	12	40
34	40	186	45	12	20
34	0	188	0	11	25
35	8	188	0	12	26
38	12	185	3	15	0
39	0	1 Ditto		14	15
39	40	Ditto		13	0
40	30	86	0	13	5
41	0	183	0	14	0
41	26	184	0	14	0
42	8	184	15	15	4
44	0	186	30	14	24
45	0	186	15	15	36
47	34	187	30	16	34
46	30	189	0	16	16
46	54	191	0	15	10
47	0	1 Ditto		15	56
47	12	91	30	16	29
45	0	192	30	15	2
44	27	191	15	13	48
40	30	186	0	12	20
37	15	196	40	13	50
37	40	197	40	13	56
38	45	202	23	11	22½
39	15	203	40	12	25
39	23	204	0	12	29
39	24	204	4	11	28
39	23	204	15	11	30
37	0	210	0	10	40
36	35	210	0	10	42
35	35	209	23	9	50
35	35	209	0	7	41
35	18	209	11	9	15
34	0	208	50	9	21½
34	18	208	49	8	48
33	50	208	37	8	0
33	22	208	20	7	56
Ditto		Ditto		8	25
33	13	207	20	8	0
32	40	206	36	9	10
26	20	206	46	8	40
25	34	206	45	8	36
25	24	206	38	8	21½
25	12			8	45
24	34	207	40	8	3
24	25	208	0	7	50
23	24	209	10	7	28
20	20	211	20	6	57

Latit.		Longit.		Variation.	
South.		West			
19°	18′	212°	30′	5°	35 E
19	4	212	50	5	31
19	0	213	15	5	25
18	52	213	35	5	0
16	59	213	55	4	53
10	36	219	8	2	54
10	3	220	45	2	30
9	51	221	5	2	51
7	39	222	40	2	34
7	2	222	30	2	4
6	18	222	10	2	30
9	36	232	13	0	8¾ W
9	50	232	57	0	2
9	40	235	45	1	18½
9	50	235	45	2	4
10	8	236	0	1	49
11	10	241	30	2	44
11	10	246	50	3	10
10		256	32	2	51
15	52	264	36	2	56
18	34	274	50	3	24
21	56	287	10	4	10
23	20	297	18	10	20
24	57	304	31	12	15

Latit.		Longit.		Variation.	
South		West			
26°	59′	311°	28′	17°	30′ W
27	55	314	0	24	20
28	40	316	0	24	0
28	54	316	30	26	10
31	8	326	30	25	35
34	20	333	0	28	19
35	40	337	10	24	0
34	54	339	0	22	30
Table Bay, Cape of Good Hope.				20	30
27	12	349	30	17	40
26	34	350	32	18	37
26	12	350	46	17	0
25	26	351	16	17	30
19	50	357	0	14	0
18	30	359	6	13	53
15	25	7	0	13	10
12	30	9	45	12	50
10	24	12	0	11	00
3	18	17	46	10	00
North					
4	20	21	51	7	40
7	40	26	0	9	40
10	38	29	22	6	30

Latit.		Longit.		Variation.	
North		West			
18°	25	35°	30′	5°	9′ W
20	0	36	30	6	40
21	4	38	0	5	4
23	30	40	0	4	30
25	40	43	18	5	34½
27	22	43	43	5	20
28	30	43	42	5	24
29	51	44	9	7	17
30	26	44	15	9	9
32	16	45	14	7	0
32	40	45	0	6	55
33	16	44	53	8	23
33	53	44	25	8	14½
34	36	Ditto		8	14
38	26	40	20	9	1
39	12	39	0	14	15½
39	22	38	0	14	24
43	55	17	16	18	30
44	30	16	18	19	30
44	40	15	44	23	0
44	50	16	10	22	50
45	0	13	0	20	36
45	30	10	45	21	25½
45	20	9	37	21	10
45	45	8	38	22	30

Extract from Capt. Cook's Journal.

1771, Nov. 9, at 8 a. m. Mr. Green and I went on shore, to observe the transit of Mercury, which came on at $7^h\ 20^m\ 58^s$ apparent time, and was observed by Mr. Green alone; I at this time was taking the sun's altitude in order to ascertain the time.

Mr. Green	Internal contact	12^h	8^m	58	P. M.
	External contact	12	9	55	
C. Cook	Internal contact	12	8	45	
	External contact	12	9	48	

Lat. observed at noon 36° 48′ 28″, the mean of this and yesterday's observations gives 35° 48′ 5½″ south, the latitude of the place of observation. The variation of the compass was found to be 11° 9′ east. These observations were made by the help of a Graham's watch with a second hand; corrected by observed altitudes of the sun.

XLV. The Transits of Venus and Mercury over the Sun's Disk, June 4 *and Nov.* 10, 1769, *observed by John Maurits Mohr. Communicated by Captain James Cook. From the Latin.* p. 433.

These transits were observed at Batavia in the East Indies; the observer being furnished with good instruments made by the best English artists, Shelton, Graham, Dollond, &c. Mr. M. used all the proper methods to rectify his in-

struments, to ascertain the rate of the clock, his true time, and the latitude and longitude of his observatory, which were, viz. 6° 10′ south latitude, and 104° 30′ longitude, east of Paris observatory. The cloudy sky however prevented any observations of the planet's passage over the solar disk; so that the exits only could be distinctly observed, which were as follow: viz. 1769.

June 4, Venus's exit, before noon, true time.

Interior contact, or beginning of the exit	8^h	30^m	13^s
Exterior contact, or the total exit	8	48	31

Nov. 10, Mercury's exit, before noon.

Interior contact, or beginning of the exit	7	33	32
Exterior contact, or the total exit	7	35	11

XLVI. Kepler's Method of Computing the Moon's Parallaxes in Solar Eclipses, Demonstrated and Extended to all Degrees of the Moon's Latitude, as also to the Assigning the Moon's Correspondent Apparent Diameter; with a Concise Application of this Form of Calculation to those Eclipses. By the late H. Pemberton, M.D., F.R.S. Communicated by M. Raper, Esq., F.R.S. p. 437.

The calculation of solar eclipses having been generally reputed a very operose process, from the repeated computations required of the moon's parallaxes by their continually varying during the progress of the eclipse, Dr. P. was once induced to consider Kepler's compendium for performing this, delivered in his Rudolphine tables, of which he had given a demonstration in his treatise entitled Astronomiæ Pars Optica. But this demonstration is perplexed, and the method itself wants correction to render it perfect. Both these defects he endeavoured to supply by the following propositions, by which may be determined with sufficient exactness the moon's apparent latitude, not only in eclipses, but in all distances of the moon from the ecliptic. And to these propositions Dr. P. premises the method he generally used for computing the nonagesime degree, and its distance from the zenith; this form of calculation not being encumbered with any diversity from the difference of cases.

Lemma. To find the nonagesime, or 90th degree of the ecliptic from the horizon, and its distance from the zenith; the latitude of the place, and the point of the equinoctial on the meridian being given. In pl. 5, fig. 1, 2, 3, 4, let AB be the equinoctial, AC the ecliptic, D the zenith, DE the meridian, and DF perpendicular to the ecliptic; whence F is the nonagesime degree, and DF the distance of that point from the zenith. Then from DE, the latitude of the place, and AE the distance of the meridian from Aries, the arch of the ecliptic AF, and the perpendicular DF may be thus found. Let I be the pole of the equinoctial, and H the pole of the ecliptic. Then AE augmented by 90° is the

measure of the angle DIH, or of its complement to 4 right angles: and the square of the radius is to the rectangle under the sines DI, IH, as the square of the sine of half the angle DIH, or of half its complement to 4 right angles, to the rectangle under the radius, and half the excess of the cosine of the difference between DI and IH, above the cosine of DH, or the sine of DF.

In the next place, the arch AD being drawn, in the rectangular triangle AED, the radius is to the cosine of DE, as the cosine of AE to the cosine of AD; and in the rectangular triangle AFD, the cosine of DF is to the radius, as the cosine of AD to the cosine of AF; therefore, by equality, the cosine of DF is to the cosine of DE as the cosine of AE to the cosine of AF;* the arch AF, counted according to the order of the signs, being to be taken similar in species to AE: for when AE is less than a quadrant, as in fig. 1, AF will be less than a quadrant; and when AE shall be greater than 1, 2, or 3 quadrants, AF, counted according to the order of the signs, shall exceed the same number of quadrants. For, since DE and DF are each less than quadrants, when AE in the triangle DEA is also less than a quadrant, the hypothenuse AD is less than a quadrant, when in the triangle DFA the legs DF, FA are similar, that is, FA will be less than a quadrant; as in fig. 1: but if AE be greater than a quadrant, as in fig. 2, that is, dissimilar to DE, the hypothenuse DA will be greater than a quadrant, and the arches DF, FA likewise dissimilar, and AF greater than a quadrant; also in fig. 3 and 4, the arches AE, AF counted from A, in consequence, will be the complements to a circle of the arches AE, AF in the triangles ADE, ADF.

For an example, let the case be taken in Dr. Halley's astronomical tables, where an occultation of the moon with a fixed star is proposed to be computed, the latitude of the place being 65° 50′ 50″, and the point of the equinoctial culminating 25° 36′ 24″, from the first point of Aries. This case relates to fig. 1, and the computation will stand thus:

For the distance of the nonagesime degree from the zenith,

Distance of E in consequence from A, the equinoctial point	25°	36′	24″	
Add	90	0	0	
Gives the angle HID	115	36	24	L. Sines
				9.92749
Half HID	57	48	12	9.92749
HI, the obliquity of the ecliptic used by Dr. Halley	23	29	0	9.60041
ID, the complement of the latitude	24	9	10	9.61190
Natural number corresponding	0.11676			9.06729
Its double, to be deducted from the nat. cosine of ID ∽ IH (0° 40′ 10″)	0.99993			Sum thrice
leaves the nat. cosine of HD (39 58 0)	0.76641			d. deduct
Therefore DF is	50	2	0	

For the arch AF.

Cosine of DF, or sine of HD (co. arith.	0.19223
Cosine of the latitude, or sine of ID	9.61191
Cosine of AE	9.95510
Cosine of the long. of the 90th deg. (54° 56′ 24″)	9.75924

The arch HD might have been computed by the versed sine of the angle HID.* But, if a table of natural sines is not at hand, the arch AD may be found logarithmically thus:

Take half the sum of the 4 first logarithms in the preceding computation of HD, viz. 19.53364

Deduct the sine of half DI ∽ IH 7.76675

the remainder is 11.76689

This remainder sought in the table of logarithmic tangents gives the correspondent sine 9.99994

This sine deducted from the first number leaves the sine of half HD, that is, 19° 59′ 0″ 9.53370

Prop. 1.—In fig. 5, 6, let BCA be the ecliptic, E the moon appearing in the ecliptic in C, from the place of the earth, whose zenith is Z; B the nonagesime degree, the arch ZB being perpendicular to the ecliptic, ZEC the circle of altitude; ED the moon's latitude, the arch DE being perpendicular to the ecliptic CB; and DC the parallax in longitude; then DE is to the horizontal parallax, as the sine of ZB, the distance of the nonagesime degree from the zenith, or the altitude of the pole of the ecliptic, to the radius; also DC is to the moon's horizontal parallax, as sin. BC × cos. ZB to the square of the radius. The arch CE is to the moon's horizontal parallax, as sin. ZC to radius, and DE is to CE as sin. ZB to sin. ZC; whence by equality DE is to the horizontal parallax as sin. ZB to the radius. Again, sin. ZB is to radius, as the tangent of ZB to the secant of ZB; therefore DE is to the horizontal parallax, as tan. of ZB to sec. ZB: but DC is to DE as sin. BC to tan. ZB; whence by equality DC is to the horizontal parallax, as sin. BC to the sec. ZB, or as sin. BC × cos. ZB to the square of the radius.

Corol.—If the point S be taken 90 degrees from the apparent place of the moon, and the arch SZ be drawn, in the spherical triangle SBZ, the cs. ZB × cs. BCS, that is, cs. ZB × s. BC is equal to rad. × cs. ZS: therefore DC is to the horizontal parallax as cs. ZS, or the sine of the distance of S from the horizon, to the radius. And if the point S is taken in consequence of the moon, it will be above the horizon, when the nonagesime degree is also in consequence of the moon; otherwise below.

Prop. 2.—Let G be the apparent place of the moon out of the ecliptic in the circle of latitude CK, K being the pole of the ecliptic, and H her true place. Then EF, the distance of the moon from the circle of her apparent latitude, when she is seen in the ecliptic, is equal to HL, her distance from the circle of her apparent latitude, when her apparent place is G. If a great circle EHT be drawn through E and H, till it meet the circle of the apparent latitude in T, the 4 great circles

* The same may be concluded from the s. HD being to s. TD as s. HID to s. IHD.—Orig.

CZ, GZ, CT, ET, intersecting each other, the ratio of s. ZC to s. CE is compounded of the ratio of s. ZG to s. GH and of the ratio of s. SHT to s. ET.* But, CE and GH being the parallaxes in altitude at the respective distances from the zenith ZC, ZG, s. ZC is to s. CE as s. ZG to s. GH: therefore the sine of HT will be equal to the sine of ET, and the arches HT, ET together make a semicircle: whence ET is equal to HL.

Corol.—The arch KH being drawn, the parallax in longitude, when the moon is in H, will be to HL, as rad. to s. KH, or the cosine of the latitude; and EF, or its equal HL, to CD, as s. KE to the radius. Therefore the moon's parallax in longitude, when in H, is to the parallax in longitude, when she appears in the ecliptic, as the sine of KE to the sine of KH, that is, as the cosine of the latitude, when the moon appears in the ecliptic, to the cosine of her latitude in H.

Prop. 3.—When the moon appears out of the ecliptic, if her latitude is small, the difference of the moon's latitude, when the moon appears in the ecliptic under the same apparent longitude, if both latitudes are on the same side of the ecliptic; otherwise their sum, will be to the moon's apparent latitude, nearly as the sine of the moon's distance from the zenith, when appearing in the ecliptic under the same apparent longitude, to the sine of the corresponding apparent distance.

Fig. 6. When the moon appears out of the ecliptic in G, the four great circles CZ, GZ, CT, ET, intersecting each other as before, the ratio of s. CZ to s. ZE will be compounded of the ratio of s. CG to s. EH, or of CG to EH in these small arches, and of the ratio of s. HT to s. GT, which last ratio, when the latitude is small, and HT near a quadrant, is nearly the ratio of equality. Now, in the triangle EKH, the arch EH exceeds the difference of KE and KH, that is, the difference of the latitudes, when both the latitudes are on the same side of the ecliptic, and their sum, when the latitudes are on the opposite sides. But here the excess will be inconsiderable. Therefore if an arch X be taken, whose sine shall be to the sine of the difference, or sum of the latitudes, as s. ZC to s. ZE, X shall be nearly equal to CG, the apparent latitude in G.

Corol. 1. If the arches DE, BZ be continued to K, the pole of the ecliptic, the 4 great circles CB, CZ, DK, BK, will intersect each other, and s. BD will be to the sine of BC, in the ratio compounded of the ratio of s. ZE to s. ZC, and of s. DK to s. EK, the least of which ratios, the arch DE being small, and DK a quadrant, is nearly the ratio of equality: therefore s. BD is to s. BC nearly as s. ZE to s. ZC; so that s. BD will be to s. BC nearly as the difference of the moon's true latitude, when she appears in G, from her latitude DE, with which she would appear in the ecliptic, if the points H and E are both on the same side of the ecliptic, or as

* Ptolem. Almag. L. i. c. 12. Menel. Spheric. L. iii. pr. 1.—Orig.

the sum of those latitudes, when H and E are on different sides of the ecliptic, to the moon's visible latitude.

Corol. 2.—The moon's apparent diameter, is to her horizontal diameter, as the sine of her apparent distance from the zenith to the sine of her true distance. Therefore, when the moon is in C, her apparent diameter is to her horizontal diameter, as S. ZC to S. ZE, and S. ZC being to S. ZE nearly as S. BC to S. BD; the moon's apparent diameter in C will be to her horizontal diameter, nearly as S. BC to S. BD. Again, the ratio of S. CG to S. EH, is compounded of the ratio of S. ZG to S. ZH, and of the ratio of S. CT to S. ET; and is also compounded of the ratio of S. ZC to S. ZE, and of the ratio of S. GT to S. TH; but the sine of ET is equal to the sine of TH, the arches ET and TH composing a semicircle; also the sine of CT there differs little from the sine of GT; therefore S. ZG is to S. ZH, that is, the moon's apparent diameter, when in G, to her horizontal diameter, nearly as S. ZC to S. ZE, or nearly as S. BC to S. BD.

Corol. 3.—In all latitudes of the moon, EH will not greatly exceed the difference, or sum of the moon's latitude in H, and the latitude with which she would appear in the ecliptic. Therefore the ratio of S. ZC to S. ZE being compounded of the ratio of S. CG to S. EH, and of the ratio of S. HT to S. GT, if X be taken, that its sine be to the sine of the difference or sum of the latitudes, as S. ZC to ZE, S. X will be nearly to S. CG as S. HT to S. GT. Hence the difference of S. X and S. GC will be to S. CG nearly as the difference of S. HT and S. GT to S. GT, HT not sensibly differing from TL. Now FT and TL together make a semicircle, and the sum of FG and GL is twice the difference of TL from a quadrant, and the difference between FG and GL equal to twice the difference of TG from a quadrant, also the difference between the sines of TL and TG is equal to the difference of the versed sines of the difference of those arches from quadrants; and further the rectangle under the sines of two arches is equal to the rectangle under half the radius, and the difference of the versed sines of the sum and difference of those arches: therefore the difference of the sines of X and of CG will be to the sine of CG, as the rectangle under the sine of half FG and the sine of half GL, to the rectangle under half the radius and the sine of GT; and in these small arches the difference of X and CG, will be to CG, nearly as the rectangle under the sines of FG and GL to the rectangle under twice the radius and the sine of GT, or even twice the square of the radius; this difference being to be added to X, when the moon's apparent latitude, and that by which she would appear in the ecliptic, are on the same side of the ecliptic, otherwise deducted from X for the final correction of the apparent latitude. And in the last place, this correction will be always so small in quantity, that in computing it CF may be safely substituted for GL.

Corol. 4.—The excess of the moon's apparent diameter, when seen in G, above

her apparent diameter in C, bears a less proportion to her horizontal diameter, than the rectangle under the sine of her horizontal parallax and twice the sine of half the apparent latitude CG, to the square of the radius.

The sine of CE is to the sine of ZC as the sine of the horizontal parallax to the radius; and CE, the difference of ZC and ZE, being very small, the difference of the sines of those arches may be esteemed to bear to the sine of CE, the ratio of the cosine of ZC to the radius; and thus the difference of the sines of ZC and ZE, will be to the sine of ZC, as the rectangle under the sine of the horizontal parallax and the cosine of ZC, to the square of the radius. And in like manner the difference of the sines of ZG and ZH, will be to the sine of ZG, as the rectangle under the sine of the horizontal parallax and the cosine of ZG, to the square of the radius. But S. ZE is to S. ZC, as the moon's horizontal diameter to her apparent diameter in C, and S. ZH to S. ZG, as the moon's horizontal diameter to her apparent diameter in G. Therefore the difference of the apparent diameter in G from the apparent diameter in C, is to the horizontal diameter, as the rectangle under the sine of the horizontal parallax and the difference of the cosines of ZC and ZG, to the square of the radius. But in the triangle CZG, the difference of ZC and ZG is less than the third side CG: therefore the chord of the difference of those arches, and much more the difference of their cosines, will be less than the chord of CG, or twice the sine of half CG. Hence the ratio of the augmentation of the apparent diameter in G, to the apparent diameter in C, will be less than the rectangle under the sine of the horizontal parallax and twice the sine of half CG, the apparent latitude, to the square of the radius.

More accurately, the chord of the difference of ZC and ZG being to the difference of their cosines, as the radius to the cosine of half their sum, the difference of the moon's apparent diameters in C and G may be considered as nearly bearing to the horizontal diameter, the ratio of the parallelopipedon, whose altitude is the sine of the horizontal parallax, and base the rectangle under the chord of CG and the cosine of ZC, to the cube of the radius; the cosine of ZC being to the cosine of ZB, the distance of the nonagesime degree from the zenith, as the cosine of BC, the apparent distance of the moon from the nonagesime degree, to the radius. But this difference can never be any sensible quantity.

Corol. 5.—When the moon is in the longitude of the nonagesime degree, the parallax in longitude ceases, and the apparent latitude is the difference of the moon's apparent distance from the zenith, and the distance of the nonagesime degree from the same. But now since DC is to the horizontal parallax, as the rectangle under the sine of BC and the cosine of ZB, to the square of the radius; if an arch be taken to the horizontal parallax, as S. BD $\times$ CS. ZB to the square of the radius, this arch will differ but little from the parallax in longitude, and is

used by Kepler as such; however, it ought to be corrected by adding it to BD, and taking an arch to this, in the proportion of the sine of BD thus augmented, to the sine simply of BD; and this last arch will be equal to the parallax in longitude without sensible error.

Again, DE, taken to the horizontal parallax as the sine of ZB to the radius, is considered by Kepler as the moon's parallax of latitude in eclipses; but this being deducted or added, as the case requires, gives EH, which being augmented in the proportion of the sine of BD + DC to the sine of BD, gives truly the apparent latitude without sensible error, when the latitude is small: but, when greater, requires to be corrected by adding together the logarithmic sine of the latitude now found, the sine of EH and the logarithm of DE, the sum of which is the double of the correction required.

In the last place the moon's horizontal diameter augmented in the proportion of the sine of BC to the sine of BD exhibits the moon's apparent diameter. And here the calculation will proceed thus: in the example above chosen for computing the nonagesime degree,

The moon's longitude is given from ♈	62° 2′ 38″	
The longitude of the nonagesime degree as found above	54 56 24	
Therefore BD =	7 6 14, its sine	9.09226
BZ, as found above, 50° 2′ 0″, its cosine		9.80777
The horizontal parallax in seconds		3.52387
	0 4 25	2.42390
This added to BD gives	7 10 39, its sine	9.09673
Difference from the first sine		447
This added to the log. of 4′ 25″, gives the log. of 4′ 28″, for the moon's parallax in longitude, such as is derived from the parallax in altitude by the parallactic angle		2.42837
Again,		
The sine of ZB 50° 2′ 0″		9.88447
Horizontal parallax 55′ 41″ = 3341 seconds		3.52387
Their sum, rejecting the radius, gives DE = 42′ 40″		3.40834 A
The moon's latitude	4° 50′ 18″	
Their sum, (EH) the latitude being south	5 32 58, its sine	8.98546 B
From the preceding calculation		447
For the apparent latitude, were the moon's lat. small	5 6 25½	8.98993 C
But the moon's lat. being here great, the numbers marked A, B, C, being added together, give twice the correction ..	0 0 24	1.38373
Its half	0 0 12	
This deducted from N° C, the moon's lat. being south, gives for the apparent latitude	5 36 13½	
Lastly,		
From the moon's horizontal parallax her horizontal diameter is	0 30 37½ or 1837½	3.26423
The number from the first calculation		447
The moon's apparent diameter 1856½″ or	30 56½	3.26870

Now, in solar eclipses, the most regular method of treating them, would be to consider the visible way of the moon from the sun as a line of continued cur-

vature, which it really is; and as it differs not greatly from a straight line, an arch of a circle may safely be used for it. But to form a computation in the sphere on this principle, would require a process somewhat intricate; but all the particulars usually inquired into in solar eclipses, may readily be assigned graphically with scale and compass after this manner.

First, find the time nearly of the conjunction of the luminaries, without being solicitous to investigate the time with exactness. To this point of time assign in some crude manner the moon's parallax in longitude, by which a time may easily be assumed, not very distant from the visible conjunction. This may very commodiously be performed instrumentally by the following proposition. To this point of time compute the place of the sun and moon, also for an hour before and after, or rather for such an interval of time as may include the whole eclipse, and not too much exceed, of which an estimate may easily be made by the forementioned proposition here subjoined. But all these places of the luminaries may be deduced from the calculation for finding the true conjunction, by means of the horary motions. In the next place, to each of these points of time compute the distance from the zenith, and the place in the ecliptic of the nonagesime degree. Then from each position of the nonagesime degree, compute, by the method described, the moon's parallax in longitude, her apparent latitude, and apparent diameter.

Fig. 7. After this, assuming on any straight line, as AB, the point C for the sun, from it lay down, for the 3 points of the ecliptic for which the preceding computations were made, the 3 distances CD, CE, CF, which shall be the measures in seconds, taken from a scale of equal parts sufficiently large, of the distances of the moon from the sun in each, compounded with their respective parallaxes in longitude, so as to represent the respective apparent distances of the moon from the sun in longitude. On these points erect the perpendiculars DG, EH, FI, for the moon's correspondent apparent latitudes, and describe through these 3 points the arch of a circle, as representing the visible way of the moon from the sun during the eclipse.

Then if from C the line CK be drawn from the centre of this circle, K will be the place of the moon at the greatest obscuration. The best method for assigning this point K is to describe the arch of a circle with the centre C and any interval by which it may cut the arch GHI, as in N and O; for the point K bisects the intercepted arch NKO. Again, if CL, CM be applied from C to the arch IHG, each equal to the sum of the semidiameter of the sun, and apparent semidiameter of the moon, L will be the place of the moon's centre at the beginning, and M the same at the end of the eclipse.

In the last place, for finding the time, when the moon shall be in each of the points L, K, M, measure the chords of the arches HG, HL, HM, HI, as not sen-

sibly differing from the arches themselves. Then A denoting HL or HM, and B the sum of GH and HI, the time sought for the greater chord may be considered equal to $\left[\frac{2A}{B} - \left(\frac{2A}{B}\right)^2\right] \times \frac{GH \backsim HI}{B} \times$ the time of the moon's passing from G to H, or from H to I. The time for the lesser chord will be $\left[\frac{2A}{B} + \left(\frac{2A}{B}\right)^2\right] \times \frac{GH \backsim HI}{B} \times$ the time above named; and in the last place, the time of the moon's passage between H and K equal to $\left[\frac{2HK}{B} \mp \left(\frac{2HK}{B}\right)^2\right] \times \frac{GH \backsim HI}{B} \times$ the time specified.

This calculation Dr. P. deduced from Sir Isaac Newton's differential method; and in the last case $-$ or $+$ $\left(\frac{2HK}{B}\right)^2 \times$ &c. is to be taken, as K shall fall within the greater or lesser of the arches GH, HI: but for the most part the term may be wholly omitted.

If this method be applied to the occultation of a star, the distances CD, CE, CF must be the parallaxes in longitude computed according to the first of the preceding propositions, united with the respective distances of the moon from the star in longitude, contracted in the proportion of the cosines of the moon's latitudes, or at least of the star's latitude to the radius. Also the moon's apparent latitudes must, for the most part, be corrected by the 3d corollary of the 3d prop. and the apparent diameters, if the correction could amount to any sensible quantity, by the 4th corollary.

The proposition mentioned above, for estimating the distance of the true conjunction from the visible, is this. Fig. 8, in any circle, whose diameter is AB, let the arch AC measure twice the complement of the declination of any point in the ecliptic CD; in like manner measure twice the complement of the latitude, and, AD, BD being drawn, let DE be the versed sine of the distance in right ascension, of that point of the ecliptic from the meridian, taken to a radius equal to the perpendicular let fall from C on the chord AD; then BE will be the sine of the distance of the point assumed in the ecliptic from the horizon, to a radius equal to the diameter of the circle. Therefore, if the diameter of the circle be the measure, on any scale of equal parts of the moon's horizontal parallax, and the point taken in the ecliptic be 90° distant from the moon's apparent longitude; the right ascension and declination of this point being first taken from tables of right ascension and declination, BE, found as above, will be the measure of the parallax in longitude, as assigned in the corol. to prop. 1; and if the point assumed in the ecliptic be 90° distant from the moon's true place, BE will approach near enough to that parallax for the purpose intended.

After the same manner may the parallax in longitude be found for any other time assumed. Also if the arch AC be taken equal to twice the complement of the obliquity of the ecliptic, that is, BC equal to twice that obliquity, BE will be nearly equal to the parallax in latitude, provided DE be taken equal to the

versed sine, to the like scale, as before, of the complement of the right ascension, of the point of the ecliptic on the meridian. And thus may be found the fittest interval of time for the 3 calculations of the parallaxes, &c. I have above proposed in general an hour; but in great eclipses it would be best to assume this interval something greater, and in small eclipses less.

XLVII. Of Logarithms, by the late Wm. Jones, Esq., F. R. S. Communicated by John Robertson, Lib. R. S. p. 455.

The following paper on the nature and construction of logarithms, was communicated to Mr. R. many years before, by that eminent mathematician the late Wm. Jones, Esq. The familiar manner in which he explains their nature, and the great art with which he obtains the modes of computation, not being exceeded, if equalled, by any writer on this subject, may claim a place in the Philos. Trans.

1. Any number may be expressed by some single power of the same radical number. For every number whatever is placed somewhere in a scale of the several powers of some radical number r, whose indices are $m - 1$, $m - 2$, $m - 3$, &c. where not only the numbers r^m, r^{m-1}, r^{m-2}, &c. are expressed; but also any intermediate number x is represented by r, with a proper index z. The index z is called the logarithm of the number x.

2. Hence, to find the logarithm z of any number x, is only to find what power of the radical number r, in that scale, is equal to the number x; or to find the index z of the power, in the equation $x = r^z$.

3. The properties of logarithms are the same with the indices of powers; that is, the sum or difference of the logarithms of two numbers, is the logarithm of the product or quotient of those numbers. And therefore, n times the logarithm of any number, is the logarithm of the nth power of that number.

4. The relation of any number x, and its logarithm z, being given; to find the relation of their least synchronal variation $\dot{x}$ and $\dot{z}$. Put $1 + n$ for r, the radical number of any scale, and $q = \frac{n}{1+n}$.

Let $a = q + \frac{1}{2}q^2 + \frac{1}{3}q^3 + \frac{1}{4}q^4$, &c.: $f = \frac{1}{a}$.

Then $f\dot{x} = x\dot{z}$ shows the relation required. For $x = r^z = (1 + r)^z$.

Now, let x and z flow so, that x becomes $x + \dot{x}$, at the same time as z shall become $z + \dot{z}$,

Then $x + \dot{x} = (1 + n)^{z + \dot{z}} = (1 + n)^z \times (1 + n)^{\dot{z}} = x \times (1 + \dot{z}q + \frac{1}{2}\dot{z}q^2 + \frac{1}{3}\dot{z}q^3 + \frac{1}{4}\dot{z}q^4$ &c.)

Therefore $\dot{x} = x\dot{z} \times (q + \frac{1}{2}q^2 + \frac{1}{3}q^3 + \frac{1}{4}q^4$ &c.) $= x\dot{z}a = x\dot{z} \times \frac{1}{f}$. Consequently $f\dot{x} = x\dot{z}$.

5. If $1 + n = r = 10$, as in the common logarithms of Briggs's form,

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Then a will be found to be 2.302585092994 &c.
And $f = \ldots\ldots\ldots\ldots\ldots$ 0.4342944819 0325 &c.
If $a = 1 = f$, the form will be that of Napier's logarithms.

6. Let B, B′, be the logs. of the numbers x, x', in the form $f = \frac{1}{a}$,
And N, N′, the logs of the same numbers, in the form $\varphi = \frac{1}{\alpha}$.
Then $\text{B}\varphi = \text{N}f$; $\text{B}a = \text{N}\alpha$; $\text{BN}' = \text{NB}'$; $\text{B}'\varphi = \text{N}'f$.
For $\dot{\text{B}} : \dot{\text{N}} :: (f \times \frac{\dot{x}}{x} : \varphi \times \frac{\dot{x}}{x} ::) f : \varphi :: \text{B} : \text{N} :: \frac{1}{a} : \frac{1}{\alpha} :: \text{B}' : \text{N}'$.
If $x = 10$; $\text{B} = 1$; $a = 2.30258$ &c.; or $f = 0.43429$ &c.; $\alpha = \varphi = 1$.
Then $\text{N} = \text{B} \times \frac{a}{\alpha} = 2.30258$ &c.

$$\text{N}' = \text{B}' \times \frac{\text{N}}{\text{B}} = 2.30258 \text{ \&c.} \times \text{B}'.$$

$$\text{B}' = \frac{f}{\varphi} \times \text{N}' = 0.43429 \text{ \&c.} \times \text{N}'.$$

7. Putting $x = q \pm v$; $\text{N} = \frac{v}{q}$.
Then $z = $ log. of x, or the log. of $q \pm v$, will be

$$= \pm \frac{v}{q} - \frac{v^2}{2q^2} \pm \frac{v^3}{3q^3} - \frac{v^4}{4q^4} \pm \frac{v^5}{5q^5} - \frac{v^6}{6q^6} \text{\&c.} \times f;$$

$$= \pm \text{N} - \tfrac{1}{2}\text{N}^2 \pm \tfrac{1}{3}\text{N}^3 - \tfrac{1}{4}\text{N}^4 \pm \tfrac{1}{5}\text{N}^5 - \tfrac{1}{6}\text{N}^6 \text{\&c.} \times f.$$

For $\dot{z} = \dot{\text{L}}.\, x = \dot{\text{L}}.\, (q \pm v) = f \times \frac{\dot{x}}{x} = f \times \frac{\pm \dot{v}}{q \pm v}$,

$$= (\pm \frac{\dot{v}}{q} - \frac{\dot{v}v}{q^2} \pm \frac{\dot{v}v^2}{q^3} - \frac{\dot{v}v^3}{q^4} \pm \frac{\dot{v}v^4}{q^5} - \frac{\dot{v}v^5}{q^6} \text{\&c.}) \times f.$$

8. In 3 quantities p, q, r, increasing by equal differences, the logarithm of any one of them being given, the logarithms of the other 2 are also given.
For, let $v = q - p = r - q$; $\text{N} = \frac{v}{q} = \frac{q-p}{q} = \frac{r-q}{q}$; P, Q, R, the logs. of p, q, r.

1. $\text{L} = \frac{q}{p} = (\text{L}.\, \frac{q}{q-v} =)\, \text{Q} - \text{P} = f \times (\text{N} + \tfrac{1}{2}\text{N}^2 + \tfrac{1}{3}\text{N}^3 + \tfrac{1}{4}\text{N}^4 + \tfrac{1}{5}\text{N}^5 \text{ \&c.}) = f\text{V}$. For $\dot{\text{L}}.\, \frac{q}{q-v} = f \times \frac{\dot{v}}{q-v}$.

2. $\text{L}.\, \frac{r}{q} = (\text{L}.\, \frac{q+v}{q} =)\, \text{R} - \text{Q} = f \times (\text{N} - \tfrac{1}{2}\text{N}^2 + \tfrac{1}{3}\text{N}^3 - \tfrac{1}{4}\text{N}^4 + \tfrac{1}{5}\text{N}^5 \text{ \&c.}) = f\text{X}$. For $\dot{\text{L}}.\, \frac{q+v}{q} = f \times \frac{\dot{v}}{q+v}$.

3. $\text{L}.\, \frac{r}{p} = f \times (\text{V} + \text{X}) = \text{R} - \text{P} = 2f \times (\text{N} + \tfrac{1}{3}\text{N}^3 + \tfrac{1}{5}\text{N}^5 + \tfrac{1}{7}\text{N}^7 + \tfrac{1}{9}\text{N}^9 \text{ \&c.}) = 2f\text{Z}$. Where $\text{N} = (\frac{v}{q} =)\, \frac{r-p}{r+p}$.

Or $\text{L}.\, \frac{r}{p} = \text{L}.\, \frac{q+v}{q-v} = \text{R} - \text{P} = 2f\text{Z}$. For $\dot{\text{L}}.\, \frac{q+v}{q-v} = 2f \times \frac{q\dot{v}}{qq - vv}$.

9. Hence, in two quantities, r the greater, p the less.
Putting $\text{N} = \frac{r-p}{r+p}$; $\text{A} = 2f\text{N}$; $\text{B} = \text{AN}^2$; $\text{C} = \text{BN}^2$; $\text{D} = \text{CN}^2$, &c.
And $\text{S} = \text{A} + \tfrac{1}{3}\text{B} + \tfrac{1}{5}\text{C} + \tfrac{1}{7}\text{D} + $ &c. Then $\text{L}.\, \frac{r}{p} = \text{S}$; or $\text{R} - \text{P} = \text{S}$.

Or, putting $N = \frac{r-p}{r+p}$; $A = fN$, &c. Then $L.\frac{r}{p} = 2S$.

Where $p = 1$; $N = \frac{r-1}{r+1}$; let $A = 2fN$, &c. Then $L.\,r = S$.

Or, in this case, putting $N = \frac{r-1}{r+1} = A$; $B = AN^2$, &c. Then $L.\,r = 2fS$.

Where $p = 1$, and $f = 1$; $N = \frac{r-1}{r+1}$; let $A = 2N$, &c. Then $L.\,r = S$.

10. In 3 quantities p, q, r, increasing by equal differences, the logarithms of any two of them being given, the logarithm of the 3d is also given.

1. For $L.\frac{qq}{pr} = 2f \times (V - X) = 2Q - (P + R)$

$= 2f \times (\frac{1}{2}N^2 + \frac{1}{4}N^4 + \frac{1}{6}N^6 + \frac{1}{8}N^8\ \&c.) = 2fY$. Where $N = \frac{r-p}{r+p}$.

Or $L.\frac{qq}{pr} = L.\frac{qq}{qq - vv} = 2fY = 2Q - (P + R)$.

Because $L.\frac{qq}{qq - vv} = 2f \times \frac{vv}{qq - vv}$.

2. Putting $N = \frac{qq - pr}{qq + pr} = \frac{vv}{qq + rp} =$ (where $v = 1$) $\frac{1}{qq + pr}$; $A = fN$; $B = AN^2$, &c.

Then $L.\frac{qq}{pr} = 2S = 2Q - (R + P)$; or $Q - \frac{R + P}{2} = S$.

For since $vv = qq - pr = 1$; put qq for r; pr for q.

Then $r - p = qq - pr = vv = 1$; $r + p = qq + pr$.

3. Putting $N = \frac{v}{q} = A$, &c. $a = \frac{1}{2}$,

$b = \frac{1}{4} - \frac{1}{3}a$,

$c = \frac{1}{6} - \frac{1}{5}a - \frac{1}{3}b$,

$d = \frac{1}{8} - \frac{1}{7}a - \frac{1}{5}b - \frac{1}{3}c$,

$e = \frac{1}{10} - \frac{1}{9}a - \frac{1}{7}b - \frac{1}{5}c - \frac{1}{3}d$,

&c.

And $M = aA + bB + cC + dD$ &c.; $\Sigma = \frac{1}{2}(R + P)$; $\Delta = \frac{1}{2}(R - P)$. Then $Q = \Sigma + \Delta M$.

For $Q - P = fV = \alpha$; $R - P = 2fz = 2\Delta$; but $\frac{\alpha}{\Delta} = 1 + M = \frac{Q - P}{\frac{1}{2}R - \frac{1}{2}P}$; therefore, &c.

11. Any numbers p, q, r, &c. and as many ratios a, b, c, &c. composed of them, the difference of whose terms is 1; as also the logarithms A, B, C, &c. of those ratios, being given: to find the logarithms P, Q, R, &c. of those numbers, where the form is 1.

For instance, if $p = 2$, $q = 3$, $r = 5$,

$a = (\frac{9}{8} =) \frac{3^2}{2^3}$; $b = (\frac{16}{15} =) \frac{2^4}{3.5}$; $c = (\frac{25}{24} =) \frac{5^2}{3.2^3}$.

Now, the logs. A, B, C, of these ratios, a, b, c, being found, the log. of either 2, 3, 5, or of any number compounded of them, may be found directly, by making each successively equal to a^x, b^y, c^z. Thus, for the log. of $10 = 2.5$.

Let $a^x b^y c^z = \frac{3^{2x}}{2^{3x}} \times \frac{2^{4y}}{3^y.5^y} \times \frac{5^{2z}}{3^z.2^{3z}} = 3^{2x}.2^{-3x} \times 2^{4y}.3^{-y}5^{-y} \times 5^{2z}.3^{-z}.2^{-3z} = 2.5$.

Therefore $2^{4y-3x-3z-1} \times 3^{2x-y-z} \times 5^{2z-y-1} = 1$.
Consequently $4y - 3x - 3z - 1 = 0$; $2x - y - z = 0$; $2z - y - 1 = 0$.
Therefore $x = 10$; $y = 13$; $z = 7$; and $a^{10} \times b^{13} \times c^{7} = (2 \times 5 =) 10$.
Therefore $10\text{A} + 13\text{B} + 7\text{C} =$ log. of 10, to the form 1.
Or, since $a = \frac{3^2}{2^3}$; $b = \frac{2^4}{3.5}$; $c = \frac{5^2}{3.2^3}$.
Therefore $\text{A} = 2\text{Q} - 3\text{P}$; $\text{B} = 4\text{P} - \text{Q} - \text{R}$; $\text{C} = 2\text{R} - \text{Q} - 3\text{P}$.

$$\text{Consequently}\ \left.\begin{array}{l} \text{P} = 3\text{A} + 4\text{B} + 2\text{C} = \text{log. of } 2 \\ \text{Q} = 5\text{A} + 6\text{B} + 3\text{C} = \text{log. of } 3 \\ \text{R} = 7\text{A} + 9\text{B} + 5\text{C} = \text{log. of } 5 \end{array}\right\} \text{to the form } 1.$$

Therefore $\text{P} + \text{R} = 10\text{A} + 13\text{B} + 7\text{C} =$ log. of $(2 \times 5 =) 10$.
And $f\text{P}$, $f\text{Q}$, $f\text{R}$, are the logarithms of 2, 3, 5, respectively, in the scale of logarithms whose form is f.

XLVIII. An Inquiry into the Value of the Ancient Greek and Roman Money. By M. Raper, Esq., F. R. S. p. 462.

In an introduction, Mr. R. enumerates the various writers on the Greek and Roman coins, showing their respective endeavours and labours, and estimating their comparative methods, merits, and defects. In the following discourse, he has collected the most authentic evidence he could find, of the weights of the Attic drachm and the Roman denarius; part of which he had taken from that very valuable publication of the Pembroke collection of coins. In the year 1759, by the favour of the learned and ingenious Dr. Gowin Knight, principal librarian of the British Museum, he weighed a considerable number of the most perfect Greek and Roman coins in that noble repository. The scales he used were good workmanship, and his weights were most accurately sized; and, on comparing the Troy ounce he used, with that in the archives of the R. S., in an exquisite balance of Dr. Henry Pemberton, it was found to be $\frac{3}{8}$ of a grain heavier; which he therefore allowed for in the following discourse.

Mr. R. then proceeds to sect. 1, on the Attic drachm. And here he observes, that the Greek coins were not only money, but weights also. Thus their drachm was both a piece of money and a weight; their mina was 100 drachms as a sum, and the same number as a weight; and their talent contained 60 minas, or 6000 drachms, both by weight and tale. This way of reckoning 100 drachms to the mina, and 60 minas to the talent, was common to all Greece; and where the drachm of one city differed from that of another, their respective talents differed in the same proportion.

Of all the Greek cities and free states, both in Europe and the lesser Asia, that of Athens was the most famous for the fineness of their silver, and the justness of its weight: Xenophon tells us, that wherever a man carried Attic silver, he would sell it to advantage. And their money deserves our more

particular attention, both because we have the most unexceptionable evidence of its standard weight; and what little we know of the money of other Greek cities is chiefly by comparison with this. The current coin of Athens was the silver drachm, which they divided into 6 oboles, and struck silver pieces of 1, 2, 3, 4, and 5 oboles, of half an obole, and a quarter of an obole. Their larger coins above the drachm were, the didrachm, the tridrachm, and the tetradrachm; which last they called stater, or the standard.

It does not appear that they coined copper till the 26th year of the Peloponnesian war, when Callias was a 2d time archon. It was soon after publicly cried down; and the conclusion of the proclamation was to this effect, that silver is the lawful money of Athens. But they seem to have had copper money not long after; for Theophrastus, Demosthenes, and some of the comic poets, quoted by Athenæus and Pollux, mention the chalcus, which was the name of the copper coin. Many pieces of Attic copper are now in being; and Vitruvius says, they coined copper oboles, and quarter oboles. Authors differ in the value of the chalcus; some say, it was the 6th part of an obole, others the 8th; Pliny (speaking of it as a weight) the 10th; and Vitruvius says, some called the quarter of an obole dichalcon, others trichalcon. According to Polybius, it seems to have been the 8th part, for he makes a quarter of an obole equal to half a Roman as. But though, when Polybius wrote, the obole might pass for 8 chalci, it is not impossible that at different times, or in different places, it may have passed for 6, 10, and 12.

It is a common opinion, that the Athenians coined gold, for which Mr. R. can find no good authority; and from the best information he has been able to get, there does not appear to be any Attic gold coin now remaining, that was struck while they were a free and flourishing people. The lexicographers, indeed, tell us, the Χρυσοῦς Ἀττικὸς was equal to the daric, and speak of gold mines at Laurium; but no ancient writer mentions such a coin, and all agree that the mines at Laurium were silver. That they had no gold coin at the beginning of the Peloponnesian war, appears from the account Thucydides gives of the treasure then in the Acropolis, which consisted of silver in coin, and gold and silver bullion; but he would certainly have mentioned gold in coin, had there been any. Athenæus tells us that gold was extremely scarce in Greece, even in the time of Philip of Macedon; but that, after the Phocæans had plundered the Pythian temple, it shone forth among the Greeks. Philip conquered these Phocæans, and put an end to the holy war, as it was called. About the time this war broke out, he took the city Crenides, on the borders of Thrace, which he enlarged, and called Philippi, after his own name; and he so improved the gold mines in its district, which before were of small account, that they produced above 1000 talents yearly, and enabled him to coin gold, which he called Philippics.

What Athenæus says of the scarcity of gold, may be true, if confined to Macedon, and the poorer states of Greece; but must not be extended to Corinth or Athens; for though Thucydides does not specify the quantity of gold that was in the Athenian treasury at the beginning of the Peloponnesian war, it was probably not inconsiderable; for the gold about the statue of Minerva weighed 40 talents, which valued, according to Herodotus, at 13 times its weight in silver, will be found to amount to above 120,000 pounds sterling. There is a gold coin in the British Museum, of elegant workmanship, with the head of Minerva on one side, and the owl and oil bottle on the other, the inscription ΑΘΗ, and under the oil bottle the letters ΝΗ. It weighs 109½ troy grains; but being a little worn, it probably, when new, came up to the just weight of the Roman imperial aureus. Whence we may conclude, that when this piece was struck, the Athenians had reduced their money to the Roman standard, and that their drachm was then equal to the denarius. But Mr. R. cannot find there is any Attic gold now extant, that was coined before Greece became subject to the Romans.

The Persian daric seems to have been the gold coin best known at Athens in ancient times. This they called stater, probably because it was the standard to which their drachm was originally adjusted, which the lexicographers tell us was half its weight. Though Greaves says the daric is still found in Persia, it is certainly very scarce, and perhaps of doubtful antiquity. For want therefore of the daric, we must have recourse to the gold of Philip, who took either that coin or the Attic drachm for his standard. Philip and his son Alexander coined gold of 4, 2, 1, and half an Attic drachm. Mr. R. then gives an account of a great number of these Philippics, or Attic drachms, then in the possession or museums of the curious; and after selecting 24 of the most perfect specimens, then weighing them all very accurately, he added all their weights together, and divided the sum of all by their number 24, when the quotient came out 132.92 troy grains, for the medium weight. Mr. R. then adds, as none of these species can have increased their original weight, but, on the contrary, some may have lost a small part of it, we may fairly conclude, that the standard weight of the Philippic was not less than 133 Troy grains; but probably somewhat greater. And its half Mr. R. afterward shows was also the Attic drachm of 66½ grains.

In Sect. 2, Mr. R. next treats of the Eginean and Euboic Talents.

The Attic was not the only money-talent used in Greece. Historians and others mention the Eginean and the Euboic talents. The former weighed 10000 Attic drachms, but, like other talents, contained only 6000 of its own; which being so much heavier than the Attic, the Athenians called it παχεῖαν δραχμὴν, or the thick drachm. This talent was used at Corinth, as appears by a passage in A. Gellius, where the Corinthian talent is valued at 10000 attic drachms: and

as Corinth was a place of great trade, it was probably used in most of the cities of the Peloponnesus. If the Attic drachm weighed $66\frac{1}{2}$ Troy grains, the Eginean should weigh $110\frac{5}{6}$; which, to avoid fractions, and because the Attic drachm is rather undersized, he calls 111. And there are Macedonian coins, struck before Philip coined gold, that answer to this standard. Therefore the Eginean talent must have been the standard of the Macedonian money, till Philip changed it.

It appears likewise, from many specimens here noticed, to have been the standard of the Ptolemaic money in Egypt. And not only so, but that it was originally Egyptian. For what should induce Ptolemy, to relinquish the standard established by Alexander, and used all over Asia and the greater part of Greece, but that he found the Eginean talent established in Egypt, when he possessed himself of that opulent kingdom.

The Euboic talent certainly came from Asia; for Herodotus tells us, the Kings of Persia weighed their gold by that talent. In the same place he informs us, that the Babylonian talent weighed 70 Euboic minas. Pollux says, it weighed 70 attic minas. Therefore the Euboic talent should be equal to the Attic. But Ælian tells us, it weighed 72 Attic minas; and if so, the Euboic talent should be heavier than the Attic, in the proportion of 72 to 70. An article in the treaty between the Romans and Etolians, recorded by Polybius, by which the latter were to pay a certain number of Euboic talents, in silver of Attic fineness, seems to favour this inequality of the two talents: for had they been equal, there would have been no occasion to specify the quality of the silver by the standard of one country, and its weight by that of another.

By one passage in Xenophon, it appears probable that the Babylonian talent weighed above 70 Attic minas; by another, that it weighed above 70 Euboic minas; and if Pollux took his value of the Babylonian talent from Herodotus, as the text now stands, and Ælian his value of the same from a more correct copy of that author, or from some better authority, the Euboic talent must have been equal to the Attic.

In Sect. 3, Mr. R. treats of the Roman Money.

Pliny has given the following historical account of the Roman coinage: "Silver was first coined at Rome in the 485th year of the city, when Q. Ogulnius and C. Fabius were consuls, 5 years before the first Punic war. And the denarius was made to pass for 10 pounds of copper; the quinarius, for 5; and the sesterce, for two and a half. But the weight of the as was reduced in the first Punic war, when the republic, being unable to defray its expences, resolved to coin 6 asses out of the pound; by which they gained 5 parts, and paid their debts. The stamp of the as was a double-faced Janus on one side, and the prow of a ship on the other: on the triens and quadrans a boat. After

this, when they were pressed by Hannibal, Quintus Fabius Maximus being dictator [about the year 537], the as was reduced to one ounce, and the silver denarius made to pass for 16 asses; the quinarius, for 8; and the sesterce 4. And the republic gained one half [on the copper money]. But in the pay of the army, the soldier also received a silver denarius for 10 asses. The stamp of the silver money was a chariot and a pair, or a chariot and 4 horses; whence they were called bigati and quadrigati. The as was soon after reduced to half an ounce, by the Papirian law.——What is now called the victoriat, was coined by the Clodian law; before which, it was imported from Illyricum as merchandize: its stamp is a victory, whence it takes its name. The gold money was coined 62 years after the silver, and the scruple passed for 20 sesterces, which, as the sesterce was reckoned at that time [$2\frac{1}{2}$ asses], made the pound of gold worth 900 silver denarii [of 16 asses each]. It was afterwards thought proper to coin 40 pieces out of the pound of gold. And our Princes have, by degrees, diminished their weight to 45 in the pound."

The denarii now remaining are of various kinds. The most ancient are the bigati and quadrigati, having on one side the head of a woman in a helmet, with the inscription ROMA, and the mark of the denarius X or $\frac{V}{A}$, and some few XVI, and a biga or quadriga on the other. The next to these in antiquity have the head of Roma, or some other deity, on one side, and on the reverse, the name of the mintmaster, or mintmasters, with historical or emblematical figures. Many of these have the X or $\frac{V}{A}$, which continued to be the mark of the denarius long after it passed for 16 asses; whence some have concluded that it was reduced again to 10 asses, contrary to the express testimony of Vitruvius; and Tacitus tells us that the mutinous legions in Pannonia demanded, to have their pay raised from 10 asses, to a denarius. A 3d sort has the head of a consul or a general on one side, with an historical or emblematical reverse. Few, if any, of these have the mark X or $\frac{V}{A}$ on them. These 3 sorts are called consular denarii, as having been struck during the republican government by consuls. The imperial denarii have commonly the head of the reigning emperor, with his name and titles on one side, and some emblematical figures on the reverse, with a suitable inscription.

The Romans coined their first gold money by the scruple; as appears from Pliny's account, which is confirmed by the coins; for he tells us the scruple passed for 20 sesterces, and the rare gold coins now remaining with the numerals XX, and XXXX, which answer to the weight of 1, and 2 ancient Roman scruples. These have the head of Mars on one side, with the numeral letters denoting their value, and, on the reverse, an eagle standing on a thunderbolt. The latter coins of this scrupular standard are like the denarii of the age in which they were struck; as was the gold of the different standards that succeeded it.

The Romans did not use the denarius for a weight, as the Greeks did their

drachm; till the Greek physicians coming to Rome, and finding the 2 coins nearly equal, prescribed by it, as they had been accustomed to do by the drachm in their own country. Neither did the Roman pound depend on the weight of the denarius, as the Greek mina did on that of the drachm; but the weight of the denarius depended on the pound.

The ancient Roman pound was divided into 12 ounces, and the ounce into 24 scruples. And we learn from Celsus and Pliny, that 84 denarii were coined out of the pound of silver; therefore, if we knew the true weight of the Roman pound, we should thence know that of the denarius. There are many ancient Roman weights now remaining, from under an ounce to 100 pounds; some of them with inscriptions have the appearance of standards. Lucas Pætus, from an ancient weight of 10 pounds, another of 4 pounds, and a third of 1 pound, inscribed EX. AVC. D. CAS. in letters of silver, besides three smaller of 3, 6, and 9 ounces, all six perfect and agreeing together, determined the ancient pound to contain 11 ounces, 10 scruples, modern Roman weight. But where he gives the weight of Vespasian's congius, he makes 10 ancient Roman pounds to weigh 9 pounds 6 ounces 10 scr. 10 gr. modern weight. The modern Roman ounce contains, like the ancient, 24 scruples, the scruple 24 grains. Therefore, according to this determination, the ancient Roman pound should weigh 11 ounces, 10 scr. $15\frac{2}{5}$ gr. modern weight, which is equal to $5012\frac{1}{5}$ Troy grains, if the exact weight of the modern Roman ounce be 438 Troy grains, as Greaves reckons it. But Pætus used a steelyard, which is a very fallacious instrument. Many other weights are examined, from which are deduced a great variety of weights for the ancient Roman pound, from 5000 up to 5780 grains Troy.

The Roman congius contained 10 pounds weight of wine. Vespasian's standard is of brass; Pætus, Villalpandus, and Greaves, have given drawings of it; and Gruter tells us, the inscription was in letters of silver. Several authors have examined the capacity of this vessel, and the weight of the fluids it contains; the medium of which gives 51591 Troy grains for the weight of its contained water, the 10th part of which gives 5159 Troy grains for the Roman pound. After many other considerations on this point, Mr. R. adds: all the above circumstances considered, it seems more probable that this standard should give too great a Roman pound, than too small a one. But as nothing certain can be determined from it, we must have recourse to the coins, especially the gold, which, though not so correctly sized as the Greek Philippics, are much more so than the silver denarii.

Pliny tells us, that when the Romans first coined gold, they made the scruple pass for 20 sesterces. And from several good specimens, it appears that the mean weight of the gold coin called the aureus, of 40 to the pound, was 126 Troy grains; which gives 5040 such grains for the pound weight.

The weight of this coin was gradually diminished by the emperors, till in Pliny's time 45 were struck out of the pound. He died in the reign of Titus; and the mean

aureus of Greaves's table from Nero to that prince, inclusive, is under 112 grs. That of the Pembroke collection for the same period amounts to 113; but Nero's coins, (contrary to Hardouin's reading of Pliny's text) appear to have been heavier than those of Vespasian or Titus. By many specimens, it appears that the mean weight of these reduced aurei was 112 Troy grs; which multiplied by 45, gives again 5040 Troy grs. for the pound weight. Alexander Severus coined pieces of one half and one third of the aureus, called semisses and tremisses; whence the aureus came to be called solidus, as being their integer. Soon after the reign of this prince, the coinage became irregular, till Constantine entirely new modelled it, by coining 72 solidi of 4 scruples, out of the pound of gold, and for the denarius substituting the miliarensis. Greaves's 2d table exhibits 29 of these solidi, from Constantine to Heraclius, weighing from $67\frac{1}{2}$ grains to $70\frac{3}{4}$. The mean from the 29 pieces is 69 grains, which, multiplied by 72, gives but 4968 grains for the weight of the Roman pound. But if the standard weight of this coin amounted to 70 grains, the pound will weigh 5040, agreeable to what it was found from the aurei.

Having thus given as complete an account of the Roman gold, as he could collect, Mr. R. next proceeds to examine the evidence we have of the weight of their silver money. The consular silver is so unequal, that the Romans must have been very negligent in sizing their pieces. Villalpandus says, that weighing many denarii of the same form, inscription, and apparent magnitude, and so like to each other, that they seem to have been struck, not only in the same age, but even on the same day, he found them to differ in weight, 5, 9, or 10 grs. from each other. After noticing many instances of a difference in weight of this coin, Mr. R. exhibits, in a table, the weights of 46 of the fairest denarii in the British Museum, of all weights from 55 Troy grains, the lowest, up to $66\frac{1}{2}$; these being all added together, and the sum divided by the number of them, gives 60.95 grains for the medium weight. And he adds, but if we take 5040 Troy grains for the weight of the Roman pound, as determined from the gold coins; the scruple will weigh $17\frac{1}{2}$ grs; the consular aureus, 126; the imperial aureus, 112; and the solidus, 70: all which are probable weights of the several coins: and the consular denarius of 84 in the pound will weigh just 60 Troy grs. And this must be very near its true standard weight; for were we to add only half a grain to it, the consular aureus would exceed 127 grs., which is certainly too great a weight for that coin.

Though Pliny gives no particular account of any alteration in the weight of the denarius, it was doubtless diminished by the emperors as well as the aureus, though by what degrees is uncertain; for Galen tells us, that the writers on weights and measures differed in the number of drachms [denarii] they assigned to the oz.; most of them making it to contain $7\frac{1}{2}$, some but 7, and others 8. The later writers make it contain 8 denarii, of 3 scr. each. Greaves 'found, by examining many imperial denarii, that from Augustus's time to Vespasian they continually decreased, till, from

being the 7th part of the Roman oz., they came now to be the 8th part: and therefore 96 were coined out of the Roman libra, whereas before, under the consuls, 84. From Vespasian to Alex. Severus, as far as he had observed, the silver continued at a kind of stay in respect of weight, excepting only such coins as on some extraordinary occasion, both then, and in the first Emperors time, were stamped, either in honour of the Prince, or of the Empress and Augusta familia, or else in memory of some eminent action. These last most usually were equal to the denarii consulares, and many of them had these characters EX. S. C, or else S. P. Q. R. Under Severus and Gordianus, the denarii began to recover their primitive weight, but most commonly with a notable abasement, and mixture of allay." Eisenschmid has given the like account of the imperial denarius, and says, he found its weight from Nero to Sept. Severus, to be to the consular denarius, in the proportion of 7 to 8.

The denarius continued to be the current silver money of the empire, till Constantine substituted the Miliarensis in its stead. The price of gold had been increasing a considerable time before his reign, which made a new regulation of the money necessary. For this purpose, Constantine divided the pound of gold into 72 solidi, which was a more commodious number than either 40 or 45, as it divided the ounce and half ounce without a fraction. He likewise altered the weight of the silver coin, and fixed the price of the pound of gold at 1000 pieces of his new silver, which were thence called miliarenses. This he seems to have done in imitation of the ancient coinage; for when the aureus of 40 in the pound passed for 25 denarii, the pound of gold passed for 1000. But it was attended with this inconvenience, that his solidus could not be exchanged for its true value in silver; for 1000 divided by 72 is $13\frac{8}{9}$; but it passed for 14, which was more than it was worth, and made 2 prices of gold at the same time; one the legal price of 1000 miliarenses for the pound; the other, the current price, of 14 for the solidus, which must have occasioned disputes in the payment of small sums. To remedy this inconvenience, it was thought proper to alter the weight of the silver money, and having fixed the price of the pound of silver, at 5 solidi, to coin 60 pieces out of it; which retained the name miliarenses, though the pound of gold was worth only 864.

A scholiast on the basilics tells us, that "One siliqua [of gold] is worth 12 folles [of copper], or half a miliarensis: therefore 12 siliquas are half a solidus, for the whole solidus is worth 12 miliarenses, or 24 siliquas." The Roman pound contained 1728 siliquas, therefore there were 72 of these solidi in the pound; and each of them being worth 12 miliarenses, the pound of silver, which was valued at 5 solidi, must have contained 60 miliarenses. How many miliarenses Constantine coined out of the pound of silver is no where said; but if the price of gold was nearly the same in his reign, as when 5 solidi were worth a pound of silver, the pound must have been worth $14\frac{2}{5}$ pounds of silver; and

1000 divided by $14\frac{2}{5}$, gives $69\frac{4}{9}$ for the number of miliarenses coined out of the pound. Therefore it is probable Constantine's number was either 69 or 70. If the former, each piece should weigh $73\frac{8}{23}$ Troy grs.; if the latter, $72\frac{8}{10}$ Eisenchmid found the larger silver of Constantine to come up to 90 Paris grs., or $73\frac{83}{100}$ Troy; but the smaller, which should be its half, seldom amounted to 40 Paris grs., or $32\frac{4}{5}$ troy; which leaves it uncertain whether 69 or 70 of these miliarenses were coined out of the pound. If 69, the proportion of gold to silver was almost $14\frac{1}{2}$ to 1; if 70, $14\frac{2}{7}$ to 1.

§ 4. *Of the Value of Gold in Greece and Rome.*

Herodotus reckons the value of gold to silver in the proportion of 13 to 1. Plato, who wrote about 50 years after him, says it was 12 times the value of silver; and Xenophon, Plato's contemporary, relates, that Cyrus paid Silanus the Ambraciot 3000 darics for the 10 talents he had promised him; which being Babylonian talents, agrees with Plato's estimate.

After the conquest of Asia by Alexander, the immense treasures of the Kings of Persia circulating in Asia and Greece, reduced the price of gold to 10 times its weight in silver, at which it seems to have continued 200 years, or more.

The Romans did not coin gold till above 100 years after the death of Alexander. That the Romans kept their accounts in copper sesterces of $2\frac{1}{2}$ asses, long after the silver sesterce passed for 4, appears from what Pliny says of the pay of the army, that notwithstanding the silver denarius passed for 16 asses, it was paid to the soldier for 10: which implies that the quæstor's accounts were kept in copper money, as all the public accounts probably were. Cæsar is said to have doubled the pay of the soldiers, and it appears from the account Tacitus gives of the mutiny of the legions in Pannonia, that at the accession of Tiberius to the empire, their pay was but 10 asses a day; and they demanded a denarius, not on pretence that the legionary soldiers had ever received so much, but that 10 asses were not an equivalent for the dangers and hardships a soldier underwent. Hence 5 asses appear to have been their pay before Cæsar raised it; but if this was their pay on the quæstor's book, they actually (according to Pliny) received a quinarius of 8 asses, and Cæsar only nominally doubled it; which is more probable than that their pay at the time he raised it, should be under two pence three farthings English a day. Polybius tells us, that in his time the pay of a Roman foot soldier was two oboles a day; that of a centurion twice as much; and that of a horseman a drachm or denarius. This must be understood of what they received, not of their nominal pay on the quæstor's book. The foot soldier, therefore, was paid at the rate of $5\frac{1}{3}$ asses a day, which, in a country where a traveller might have his lodging and all necessaries on the road for half an as, would be great pay, had not their cloathing, arms, and tents, been deducted out of it, as they were. But both the public and private riches of the

Romans were increasing very fast when Polybius wrote, and the prices of all the necessaries of life must have increased in proportion, therefore it is probable that the soldier's pay was raised to 5 asses on the quæstor's book, for which they received a quinarius, before Cæsar augmented it.

If the pound weight of gold was worth 900 denarii, 84 of which were coined out of the pound of silver, the value of gold to silver must have been in the proportion of 900 to 84, or as $10\frac{5}{7}$ to 1. And if this was the value of gold at Rome 62 years after their first coinage of silver, it proves that no fewer than 84 denarii were then coined out of the pound. Now by an article in the treaty with the Etolians, about 18 years after this first coinage of gold at Rome, that people were permitted to pay one-third of their tribute in gold, at the rate of one pound of gold for ten of silver. Therefore gold was then but 10 times the value of silver in Greece; and it could not be much higher at Rome, where silver was esteemed the more useful metal, as appears by the limitation of the sum to be paid in gold, to one-third of the whole; and Pliny observes, that the Romans always required the tribute they imposed on conquered countries should be paid in silver, not in gold; therefore it is not probable that gold should bear a much higher price at Rome than elsewhere, as it would, according to this account of its first coinage, if fewer than 84 denarii were coined out of the pound of silver.

From a passage in Tacitus, compared with Suetonius, we learn that in Galba's time the aureus passed for 25 denarii. But 100 nummi were equal to 25 denarii; therefore when 40 aurei were coined out of the pound of gold, and 84 denarii out of the pound of silver, the pound of gold, passing for 1000 denarii, was worth $11\frac{19}{21}$ pounds of silver. When the aureus of 45 in the pound passed for 25 denarii of 96 in the pound, the proportional value of gold to silver was as 375 to 32, or a little under $11\frac{3}{4}$ to 1. Suetonius tells us, that Cæsar brought so great a quantity of gold from Gaul, that he sold it throughout Italy and the provinces for 3000 nummi the pound. 3000 nummi make 750 denarii; and 750 is to 84, as $8\frac{13}{14}$ to 1. This was its price as merchandise, when the market was overstocked, and the seller in haste to dispose of his goods; but what effect it had on the coin, is not known. By the diminution of the aureus for above half a century before the reign of Constantine, the price of gold appears to have been rising, till it came to above 14 times its weight in silver; for 5 solidi of 72 in the pound, being valued at a pound of silver, the proportion between the two metals was as $14\frac{2}{5}$ to 1.

§ *V. Of the Value of the Ancient Greek and Roman Money.*

It does not appear that either the ancient Greeks or Romans allayed their money, but coined the metals as pure as the refiners of those times could make them: for though Pliny mentions two instances of the contrary at Rome, the example was not followed, till the late Emperors debased the coin: and his

expression, miscentur æra falsæ monetæ, shows he thought the practice illegal. Though the ancients had not the art of refining silver, in so great perfection as it is now practised, yet, as they mixed no base metal with it, and esteemed what they coined to be fine silver, Mr. R. values it as such.

Sixty-two English shillings are coined out of 11 ounces 2 dwt. Troy of fine silver, and 18 dwt. of allay. Therefore, the Troy grain of fine silver is worth $\frac{62}{111}$ of a farthing. Hence the Attic drachm of $66\frac{1}{2}$ grains will be found worth a little more than 9 pence farthing; the obole, a little more than 3 half-pence; and the chalcus, about $\frac{7}{9}$ of a farthing. But, for the reduction of large sums to English money, the following numbers are more exact.

The Attic drachm	0*l.*	0*s.*	9*d*.286
The mina	3	17	4. 6
The talent	232	3	0.

Hence the mina expressed in pounds sterling and decimals of a pound will be 3.869*l.* the talent 232.15*l.*

The Romans reckoned by asses before they coined silver, after which they kept their accounts in sesterces. The word sestertius is an adjective, and signifies 2 and a half of any substantive to which it refers. In money matters its substantive is either as or pondus; and sestertius as, is 2 asses and a half; sestertium pondus, 2 pondera and a half, or 250 denarii. When the denarius passed for 10 asses, the sesterce of $2\frac{1}{2}$ asses was a quarter of it; and the Romans continued to keep their accounts in these sesterces long after the denarius passed for 16 asses; till, growing rich, they found it more convenient to reckon by quarters of the denarius, which they called nummi, and used the words nummus and sestertius, indifferently as synonymous terms, and sometimes both together, as sestertius nummus; in which case, the word sestertius, having lost its original signification, was used as a substantive; for sestertius nummus was not 2 nummi and a half, but a single nummus of 4 asses.

They called any sum under 2000 sesterces so many sestertii, in the masculine gender; 2000 sesterces they called duo or bina sestertia, in the neuter; so many quarters making 500 denarii, which was twice the sestertium; and they said dena, vicena, &c. sestertia, till the sum amounted to 1000 sestertia, which was a million of sesterces. But, to avoid ambiguity, they did not use the neuter sestertium in the singular number, when the whole sum amounted to no more than 1000 sesterces, or one sestertium. They called a million of sesterces decies nummum, or decies sestertium, for decies centena millia nummorum, or sestertiorum, in the masculine gender, omitting centena millia, for the sake of brevity: they likewise called the same sum decies sestertium (in the neuter gender), for decies centies sestertium, omitting centies for the reason above-mentioned; or simply decies, omitting centena millia sestertium, or centies sestertium; and with

the numeral adverbs, decies, vicies, centies, millies, and the like, either centena millia, or centies, was always understood. The Constantinopolitans kept their accounts in solidi, which are reduced to pounds sterling, by multiplying the given number by 58648, and cutting off 5 figures on the right hand for decimals.

Conclusion.—The Greeks had no money at the time of the Trojan war; for Homer represents them as trafficking by barter, and Priam, an Asiatic, weighs out the 10 talents of gold, which he takes to ransom his son's body of Achilles.

This ponderal talent was very small, as appears from Homer's description of the games at the funeral of Patroclus, where 2 talents of gold are proposed as an inferior prize to a mare with foal of a mule. Whence Mr. R. concludes it was the same that the Dorian colonies carried to Sicily and Calabria; for Pollux tells us, from Aristotle, that the ancient talent of the Greeks in Sicily contained 24 nummi, each of which weighing an obole and a half, the talent must have weighed 6 Attic drachms, or 3 darics; and Pollux elsewhere mentions such a talent of gold. But the daric weighed very little more than our guinea; and if 2 talents weighed about 6 guineas, we may reckon the mare with foal worth 12*l*; which was no improbable price, since we learn from a passage in the Clouds of Aristophanes, that, in his time, a running horse cost 12 minas, or above 46 pounds sterling. Therefore this seems to have been the ancient Greek talent, before the art of stamping money had introduced the greater talents from Asia and Egypt.

Herodotus tells us that the Lydians were reputed to be the first that coined gold and silver money; and the talent, which the Greeks called euboïc, certainly came from Asia. Therefore the Greeks learned the use of money from the Asiatics. The Romans took their weights and their money, either from the Dorians of Calabria, or from Sicily; for their libra, uncia, and nummus, were all Doric words, their denarius was the Sicilian Δεκάλιτρον; and Pollux tells us, from Aristotle, that the Sicilian nummus was a quarter of the Attic drachm; and the Romans called a quarter of their denarius by the same name.

The great disproportion between the copper and silver money, when the Romans first coined the latter, has induced many to believe that the first denarii must have been heavier than the 84th part of their pound; thinking it incredible that silver should ever be valued at 840 times its weight of copper. But they can produce no ancient author of credit, in support of this opinion. But we are little interested in the weight of the denarius for the first 60 years after it was coined; and it has been shown that when the Romans began to coin gold, it did not exceed the 84th part of their pound.

XLIX. Description of a Method of Measuring Differences of Right Ascension

and Declination, with Dollond's Micrometer; with other New Applications of the same. By the Rev. N. Maskelyne, B. D., F. R. S., &c. - p. 536.

The divided object glass micrometer, as happily applied by the late Mr. John Dollond to the object end of a reflecting telescope, and now with equal advantage adapted by the present Mr. Dollond, his son, to the end of an acromatic telescope, is so easy of use, and affords so large a scale, that it is generally considered by astronomers as the most convenient and exact instrument for measuring small distances in the heavens. But, as the common wire micrometer is peculiarly adapted for measuring differences of right ascension and declination of celestial objects, and is not near so convenient or exact for measuring their absolute distances; so on the contrary the object glass micrometer is peculiarly fitted for measuring distances, and has generally been supposed incapable of or unfit for measuring distances of right ascension and declination. Thus the 2 micrometers, as mutually supplying each other's defects, have been esteemed both equally necessary in their turn to be used by the practical astronomer, and consequently to have a place in every well-furnished observatory. Every astronomer, who has time and inclination for making a variety of observations, would undoubtedly wish to be supplied with, and to make use of both. But, as every person desirous of making observations for his own amusement, or public utility, may not happen actually to be furnished with, nor chuse to be at the expence of providing himself with both, it is certainly a very desirable thing, if he could be enabled to make that use of the instrument he has, which might supply, in some measure at least, the want of the other which he has not. Therefore, as the object glass micrometer may be applied with little trouble, and but small additional expence, to the measuring differences of right ascension and declination, with an exactness little, if at all, inferior to what they can be obtained with the common micrometer, Mr. M. gives here the directions necessary to be followed when it is used in this manner. He afterwards shows how differences of right ascension and declination between the limbs of the sun and Venus or Mercury, and distances of the limbs both in lines parallel and perpendicular to the equator, may also be observed in the transits of these planets over the sun.

A small addition will be necessary to be made to the apparatus of the object glass micrometer, to enable it to answer these purposes, viz. a cell, containing 2 wires intersecting each other at right angles, placed in the focus of the eye-glass of the telescope, and moveable round about, by turning a button. Let ENWS, pl. 5. fig. 9, represent the field-bar of the telescope, EW and NS two wires intersecting each other at right angles at C, and moveable about the same as a centre, in manner above-mentioned. Suppose it be required to measure the difference of right ascension and declination of two stars, whose difference of declination does not exceed the extent of the scale of the micrometer, and the distance of

the meridians passing through the stars does not exceed CW, the semidiameter of the field of the telescope. Turn the wires EW, NS about, till one of the stars (the westernmost star will generally be best for this purpose) runs exactly along the wire EW, by the diurnal motion. Then separate the 2 segments of the divided object glass to a convenient distance, and turn the micrometer round about, by means of the proper handle, till the two images of the same star, formed by the two segments of the object glass, pass the horary wire NS at the same instant. Lastly, partly by separating the glasses, and partly by touching the rack-work screws of the stand of the telescope, cause the southernmost image of the northernmost star, and the northernmost image of the southernmost star, to appear both upon and run along the wire EW, as A, B. The numbers standing on the scale of the micrometer will show the difference of declination of the stars; and if the times be noted when they pass the horary wire NS, the difference of the times will give the difference of their right ascension. For EW, on account of the star's running along it, is parallel to the equator; and consequently NS, which is perpendicular to it, represents a meridian or horary circle. And because the two images of the same star pass the horary wire NS at the same instant, it follows that the centres of the two semicircular glasses lie in the same meridian, and consequently when A, B, the two contrary images of the two stars are brought to the same parallel of declination EW, the scale will show the difference of their declinations. And, for the same reason, the times of the images of the two stars passing the meridian wire NS will not be affected by the separation of the glasses of the micrometer, and consequently the difference of the times will give the difference of their right ascension. It will be easily understood that in performing the operations above described, it will be necessary from time to time to turn the screws of the rack-work which move the whole telescope together. These operations will be much facilitated and rendered more exact, if the telescope be supported by and moveable on a polar axis; for the wires and micrometer may be thus more brought into the requisite positions, and the turning of the telescope about in order to follow the diurnal motion will not disturb those positions: which will afford this further advantage to the observer, of being able to repeat the observations without loss of time.

If two additional horary wires FG, HI parallel to NS be placed near E and W, the two extremities of the wire EW, the adjustment of the wires and micrometer may be more readily performed, and the observation may be made on two stars, though their meridian distance from one another should be almost equal to EW, the diameter of the field of the telescope. It is evident, that if two stars be thus observed whose difference of declination is well settled, the value of the scale of the micrometer may be thence determined.

In the foregoing directions it has been supposed that the images of the two

stars can be brought to appear within the field of the telescope on the wire EW at the same time; but this is not absolutely necessary. For if the micrometer be set to the difference of the declination nearly, and then the star which passes first through the telescope be made to run along the wire EW, by touching one of the handles of the rack-work of the telescope, and afterwards the other star, when it comes into the telescope, be brought to the wire EW by altering the opening of the glasses of the micrometer, the difference of the declination will be had, by taking half the sum of the numbers shown by the micrometer, at the two separate observations of the two stars on the wire EW. This will be true, in case it can be depended on that the two semicircular glasses recede equally in contrary directions; which may indeed be doubted, the work on which the motion of the glasses depends not being designed for such a purpose, and therefore probably not made sufficiently accurate for it.

The manner in which Mr. Dollond has contrived the motion of the glasses, in his new improvement of the object glass micrometer, entirely obviates this difficulty, and the difference of right ascension and declination of any two stars, or other points in the heavens, may be thus accurately measured, let the difference of right ascension be what it will, provided the difference of declination does not exceed the extent of the scale of the micrometer; and thus the object glass micrometer is put pretty much on a footing with the common micrometer, even with respect to the measuring right ascensions and declinations.

The difference of right ascension and declination between Venus or Mercury and the sun's limb, in their transits over the sun, are to be observed nearly in the same manner as the difference of right ascension and declination of two stars. But the process will perhaps be rendered clearer by the following description. 1. Turn the moveable wires EW, NS, into such a position, that the sun's north limb n, fig. 10, or the planet's north limb v, may run along the wire EW, which thus becomes a tangent to the peripheries of their disks. 2. The semicircular glasses being separated to a convenient distance, turn the micrometer about, till the two images of the planet v, v, pass over the horary wire NS at the same instant. 3. Separate the glasses of the micrometer to such distance, that the north limb v of the northernmost image of the planet may touch the wire EW, at the same time that the northernmost limb n of the southernmost image of the sun touches the same wire; and the scale of the micrometer will show the difference of declination of the northern limbs of the sun and planet. In like manner, if the glasses of the micrometer be opened to a greater or less distance, according as the planet is nearer the north or south limb of the sun, every thing else remaining unmoved, the difference of declination of the southern limbs of the sun and planet may be observed, by bringing the southernmost limb of the southernmost image of the planet to run along the wire EW, at the same time

that the southernmost limb of the northernmost image of the sun runs along the same. Half the difference of these two measures, if taken immediately after one another, is equal to the difference of the declination of the centres of the sun and planet at the intermediate time, without any regard to the quantities of the diameters of the sun or planet, or the error of adjustment of the micrometer. The difference of the transits of the eastern or western limbs of the sun and planet will give the difference of right ascension, as in the common micrometer.

Instead of differences of right ascension, distances of the planet from the sun's limb, in lines parallel to the equator, may be more accurately observed as follows. The glasses being separated to a convenient distance, turn both the wires and micrometer about, so that the two images of the planet may both run along the wire EW, fig. 11, and separate the glasses so that V, one of the images of the planet, may touch the limb of the sun to the east or west, or rather both alternately. Or perhaps the following method may be preferable: separate the two images of the sun to any convenient distance, so as to produce a considerable angle of intersection of the circumferences at I and T; turn the wires about, so that the planet's centre, north, or south limb, may run along the wire EW; then turn the micrometer about till the two intersections I, T, pass the horary wire NS at the same instant, and the micrometer will be in a proper position for measuring distances in a line parallel to the equator; and the distance of the planet from the sun's limb in a line parallel to the equator will be obtained by only bringing the glasses nearer together, or separating them further, till the planet's limb is in contact with the sun's limb. If distances of the planet's near limb from the sun's limb be thus taken to the east and west alternately, and reduced to a given time, by allowing for the motion of the planet by calculation, half the difference of the two reduced measures will be the distance of the planet's centre from the middle of the chord of the sun's disk passing through the planet's centre parallel to the equator at the given time, without any regard to the quantities of the diameters of the sun or planet, or the error of the adjustment of the micrometer. It may be proper to remark, that when the planet is brought to touch the sun's limb, the point of contact will be north or south of the planet's centre, according as the planet itself is north or south of the sun's centre.

In like manner, distances of Venus or Mercury from the sun's limb may be measured in lines perpendicular to the equator, see fig. 12, (the micrometer being brought into the proper position, in the very same manner as for measuring the difference of declination from the sun's north or south limb, before described); and if the planet be brought into contact with the sun's limb to the north and south alternately, half the difference of the measures, reduced to a given time, by allowing for the motion of the planet by calculation, will be the

difference of declination of the centres of the sun and planet at that time, without any regard to the diameters of the sun or planet, or the error of adjustment of the micrometer. And this would be a better observation than measuring the difference of declination of the limbs of the sun and planet by bringing them both in contact with the same wire parallel to the equator described above; as the measuring distances from the sun's east or west limb, in lines parallel to the equator, is a better observation than measuring differences of right ascension of the limbs by time.

By these two observations of distances, of an inferior planet from the sun's limb, in lines parallel and perpendicular to the equator, its true place with respect to the sun's centre may be accurately ascertained during any part of its transit over the sun's disk; and consequently its nearest approach to the sun's centre, and the time of the ecliptic conjunction, may be deduced with great exactness, though the middle of the transit should not be seen, and the sun should be visible only for a small space of time sufficient for taking these observations.

The following order of making the several observations with Dollond's micrometer, in the late transit of Venus, was recommended to the observers who went on the part of the Royal Society to the North Cape and to the South Sea, which may serve to elucidate their observations. See Phil. Trans., vol. 59, p. 266, 267, and this vol. 61, p. 397, 418. Instructions to the like effect were also given to the other observers, sent by the Royal Society to Hudson's Bay and the north of Ireland, on the same occasion. See Phil. Trans., vol. 59, p. 480—482, and vol. 60, p. 488.

1st. Immediately after the first internal contact, you are to observe several diameters of Venus, suppose 12, with 0 of the vernier placed alternately to the right and left hand of the beginning of the divisions of the scale. 2d. You are to observe several differences of declination of the northern limbs of the sun and Venus, and the southern limbs of the sun and Venus alternately. 3d. If there be considerable time left before the middle of the transit, you are to observe distances of Venus from the sun's limb to the east and west alternately, in lines parallel to the equator. 4th. If there still remain considerable time before the middle of the transit, you are to observe several times the horizontal diameter of the sun. 5th. You are to begin at least half an hour (an hour would be better) before the middle of the transit, to measure the nearest distance of Venus from the sun's limb, and the farthest distance of Venus from the sun's limb, alternately. N.B. The same position of the micrometer will serve for both, without turning it about. These observations are to be continued till the very middle of the transit, when the distance will continue the same for a little space of time; but it will be better to continue them for some time longer. 6th. The same

observations which were taken before the middle of the transit, or such as could not, through some impediment, be observed before, may be proper to be observed after the middle of the transit. 7th. It will be adviseable to practise observations similar to those here recommended, previous to the transit of Venus, by means of spots in the sun.

L. A Supplement to a former Paper, concerning Difficulties in the Newtonian Theory of Light. By the Rev. S. Horsley, LL.B., F.R.S. p. 547.

PROB. I.—A parcel of equal circles being disposed on a plane surface, of any figure whatever, in the closest arrangement possible; to determine the ultimate proportion of the space covered by all the circles, to the space occupied by all their interstices, when each circle is infinitely small, and the space, over which they are disposed, is of a finite magnitude.

The closest manner in which a parcel of equal circles can be disposed on a plane, is when the centres of every 3 contiguous circles are situated at the angles of an equilateral triangle, which has each of its sides equal to a diameter of any one of the circles. A number of circles, thus disposed, may be divided, as fig. 13, pl. 5, shows, into several rows of circles, having their centres ranged on parallel right lines, AG, HP, QX, ΓΣ, &c. Every circle, which is not in an outermost row, or at the extremity of any other row, touches 6 others, namely, 2 in its own row, and 2 in the row on either side of its own: and each adjacent pair of these 6 do also touch each other. In the outer rows, every circle, which is not at one extremity of its row, touches 4 others, 2 in its own row, and 2 in the row next beside it: which last 2 do likewise touch each other. A circle at either extremity of an outer row, touches only a single circle in its own row, but either 1 or 2 in the row next beside it. The bare inspection of the figure will make these assertions manifest.

Now, imagine the equal circles, exhibited in the figure, to be each infinitely small, the number of them being infinitely great, and the whole space over which they are disposed being of a finite magnitude. The ultimate proportion of the space covered by all the circles, to the space occupied by all their interstices, is that of half the area of one of the circles to the whole of one interstitial area, i. e. the proportion of 39 to 4 very nearly.

Demonst. The circles ranged along the parallel right lines AB, HP, form 2 rows of interstices; the row marked a, b, c, d, &c. and the row marked α, β, γ, δ, &c. and, in like manner, 2 rows of interstices are formed by every 2 contiguous rows of circles. Now, the numbers of the circles ranged along the several parallel right lines, AG, HP, QX, &c. are either equal or unequal, according to the figure of the space over which they are disposed.

Case 1. First suppose, that an equal number of circles is ranged along each

of the parallel lines; in which case, the figure, in which they are included, must be a parallelogram. The number of circles, ranged along the parallel right lines AG, HP, being equal, the number of interstices in each of the rows, a, b, c, d, &c. α, β, γ, δ, &c. is less by unity than the number of circles on either line, AG, or HP, be that number what it will. Thus the 2 circles A, B, on the line AG, with the 2 circles HK, on the line HP, have the single interstice a, in the row a, b, c, d, &c. and the single interstice α, in the row α, β, γ, δ, &c. Again, the 3 circles A, B, C, on the line AG, with the 3, H, K, L, on the line HP, have the 2 interstices a, b, in the row a, b, c, d, &c. and the 2 α, β, in the row α, β, γ, δ, &c. And universally, if the number of circles in each row be m, the number of interstices, in each of the 2 rows of interstices, will be $m - 1$. Consequently, the whole number of interstices formed by these 2 rows of circles is $2m - 2$. In like manner, the 2 rows of circles HP, QX, form 2 more rows of interstices. And the number of circles on each line, HP, QX, being m, the number of interstices in each row is $m - 1$, and the whole number in both rows $2m - 2$. Therefore, the whole number of interstices formed by the 3 rows of circles, AG, HP, QX, is $2m - 2$ twice taken, or $(2m - 2) \times 2$. By the same reasoning if a 4th row of m circles, ΓΣ be added, the number of interstices formed by the 4 rows is $(2m - 2) \times 3$. And universally, if there be n rows of equal circles, and m circles in each row, the number of interstices formed by all the rows is $(m - 2) \times (n - 1)$. Now, when the circles are infinitely small, their diameters are infinitely small. Therefore, the space which they cover being of finite magnitude, it is necessary, that both the number of circles in each row, and the number of rows, that is, that each of the numbers, m and n, should be infinitely great. But when m and n are each infinitely great, $(2m - 2) \times (n - 1)$, that is, the number of interstices, becomes ultimately $2mn$; and the interstices being all equal one to another, if the area of one be called P, the sum of their areas will be $2mn \times$ P. But the number of circles in n rows, each row consisting of m circles, is mn; and the circles being equal, if the area of one be called A, the sum of their areas will be $mn \times$ A. Hence the space covered by all the circles, is to the space covered by all their interstices, when the magnitude of each circle is infinitely diminished, and the number of them so infinitely augmented, as that they shall cover a space of finite magnitude, ultimately, as $mn \times$ A to $2mn \times$ P, that is, as A to 2P, or as $\frac{1}{2}$A to P, that is, as half the area of one circle to the whole area of one interstice.

Case 2. Now, suppose that unequal numbers of circles are ranged along the several lines AG, HP, QX, &c. which must always be the case, if the figure of the space, in which they are contained, be any other than a parallelogram; and let the number on AG be the greatest of all, and call that number, as before, m. If from the row HP, the extreme circle P be taken away, all the rest being left,

the interstice ζ will be taken away, and all the other interstices, formed by m circles on HP, with m circles on AG, will remain. If again the circle O be taken away, besides the interstice ζ already taken away, the two ε, f will disappear; and every circle more that is taken away, of those remaining on HP, from the extremity of the line, 2 more interstices will disappear. If from the row of circles HP, the extreme circle H be taken away, the 2 interstices a, α, will disappear. And if the circles K, L, M, be taken away successively, every new circle that is taken away, 2 more interstices will disappear, of those formed by the 2 rows AG, HP. Again, if the 2 circles P and H be taken away, the 3 interstices ζ, a, α, will disappear; and every circle more that is taken away, from either extremity, 2 more interstices will disappear. Hence, whatever number of circles be taken away, out of m circles on HP, provided they be taken successively, from either or both ends of the row (and when the number of circles on HP is supposed less than that on AG, the deficiency must be at the end, not in the middle of the row, otherwise the circles remaining would not be in the closest arrangement), it is evident that the number of interstices which disappear, of those which would be formed by m circles on HP, with m circles on AG, must be either double the number of circles taken away, or less than the double of that number by 1. That is, if $m - a$ be the number of circles left on HP, the number of interstices formed by them, with m circles on AG, is less than the number which would be formed by m circles on HP, with m circles on AG, either by $2a$, or by $2a - 1$. The number of interstices formed by m circles on each row would be, as has been shown in the preceding case, $2m - 2$. Therefore the number formed by m circles on AG, with $m - a$ circles on HP, is either $2m - 2 - 2a$, or $2m - 2a - 1$. That is, ultimately (when the number $m - a$ is infinitely increased) $2m - 2a$. Now suppose the number of circles on QX to be $m - a - b$. The number of circles on the 2 rows AG, HP, is $2m - a$. On the 3 rows AG, HP, QX, the number is $3m - 2a - b$. And if the number of circles on ΓΣ be $m - a - b - c$, the number of circles on the 4 rows AG, HP, QX, ΓΣ, will be $4m - 3a - 2b - c$. And, universally, the number of rows being n, and the number of circles on the several rows, m, $m - a$, $m - a - b$, $m - a - b - c$, $m - a - b - c - d$, &c. successively, the whole number on all the n rows will be $nm - a \times (n - 1) - b \times (n - 2) - c \times (n - 3)$, &c. But as it has been shown that m circles on AG, with $m - a$ circles on HP, form $2m - 2a$ interstices, if the number $m - a$ be infinite; in the same manner it may be shown, that $m - a$ circles on HP, with $m - a - b$ circles on QX, form $2m - 2a - 2b$ interstices, when the number $m - a - b$ is infinite. Therefore the whole number of interstices formed by the 3 rows on AG, HP, QX, is $(2m - 2a) \times 2 - 2b$. And, in like manner, the number of interstices, formed by the circles of 4 rows, will be $(2m - 2a) \times 3 - 2b \times (2 - 2c)$. And, uni-

versally, n being the number of rows, the number of the interstices will be $(2m - 2a) \times (n - 1) - 2b \times (n - 2) - 2c \times (n - 3)$, &c. That is, $2m \times (n - 1) - 2a \times (n - 1) - 2b \times (n - 2) - 2c \times (n - 3)$, &c. By comparing this expression with the former expression of the number of the circles, it will appear, that when n, the number of the rows of circles, is infinitely augmented, the number of interstices is to the number of circles, ultimately as 2 to 1. For the two expressions always consist of an equal number of terms. The same numerical terms in both are affected with the same signs. The first term of the latter, $2m \times (n - 1)$, is ultimately double the first term of the former, mn, when n is infinitely increased, and each succeeding term of the latter is double the corresponding term of the former. Therefore the whole of the latter expression, is ultimately to the whole of the former, as 2 to 1. That is, the number of interstices is ultimately double the number of circles: whence it follows, as in the former case, that the whole space covered by the circles, is to the whole space occupied by the interstices, as half the area of one circle, to the whole area of one interstice.

In this demonstration, I have supposed the number of circles on the several lines AG, HP, QX, &c. to decrease continually. Had I supposed them to decrease by fits, and in any manner imaginable, still the conclusion would have been the same.* Therefore let the figure of the finite space, including the circles thus closely arranged, with their interstices, be what it will, the proportion of the space covered by all the circles, to the space taken up in interstice, is ultimately that of half the area of one circle to the whole area of one interstice.

Now, that this is the proportion of 39 to 4, very nearly, will appear by computing one of the interstitial areas. The method of computing the interstitial area is obvious. Let A, B, H be the centres of the 3 circles, which close the interstice ϒΦϒ. Join AB, AH, BH. The right lines AB, AH, BH, pass through the

* Suppose the number of circles on the first row to be m, on the 2d, $m - a$, on the 3d, $m - a + b$, on the 4th, $m - a + b - c$, on the 5th, $m - a + b - c + d$, and so on, and each of these numbers to be infinitely increased. Then, n being the number of rows, the whole number of circles will be $nm - a \times (n - 1) + b \times (n - 2) - c \times (n - 3) + d \times (n - 4)$, &c. Number the interstices formed by every 2 contiguous rows, and add them all together, and the whole number of interstices will be found to be $2m \times (n - 1) - 2a \times (n - 1) + 2b \times (n - 3) - 2c \times (n - 3) + 2d \times (n - 5)$, &c. Now, by comparing these two expressions, it appears that both consist of the same number of terms: that the same numerical terms in order from the first, have the same signs: that the first term of the latter, $2m \times (n - 1)$, is ultimately the double of the first term of the former, when n is infinitely increased: that of the terms following the first, the negative terms of the latter are each double the corresponding negative terms of the former: and each positive term of the latter differs from the double of the corresponding positive term of the former, by a number which vanishes with respect to either of those corresponding terms, when n becomes infinite. Therefore, when n becomes infinite, the whole of the latter expression becomes the double of the whole of the former. Hence the conclusion is as before.—Orig.

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points of contact Υ, Φ, Ψ, respectively. The area of the triangle, AHB, is equal to the areas of the 3 sectors ΑΥΦ, ΒΦΨ, ΗΨΥ, added to the interstitial area ΥΦΨ. But the triangle AHB is equilateral. Therefore each of the sectors ΑΥΦ, ΒΦΨ, ΗΨΥ, is $\frac{1}{6}$ of the circle to which it belongs: and the circles being equal, the 3 sectors are equal to the half of any one of the circles. Therefore the area of the triangle AHB is equal to $\frac{1}{2}$ the area of one circle, (as of A) added to the interstitial area ΥΦΨ. Therefore, from the area of the triangle AHB, take $\frac{1}{2}$ the area of the circle A, and there will remain the interstitial area ΥΦΨ.

Now, if the radius ΑΦ be put = 1, each side of the triangle AHB will be 2.

Therefore, the area of the triangle AHB will be .... = 1.73205

But the radius being 1, $\frac{1}{2}$ the area of the circle A is .... = 1.5708

The difference is 0.1612

And this is the interstitial area ΥΦΨ, the half area of the circle A being 1.5708. Therefore the semicircle is to the interstice, as 1.5708 to 0.1612, or as 9.74 to 1, or as 39 to 4, very nearly.

Corol. If a parcel of equal circles be so disposed on a plane surface of any figure whatever, that the centres of every 3 adjacent circles are situated at the angles of equal equilateral triangles, having sides greater than the diameters of the circles, but greater in a finite proportion, the ultimate proportion of the space covered by all the circles, to the space occupied by all the interstices, when each circle is infinitely diminished, and the number of them so infinitely increased, that the space over which they spread is of a finite magnitude, is that of $\frac{1}{2}$ the area of one circle to the whole area of one interstice. And the area of any one interstice, is equal to the difference of the area of the equilateral triangle, formed by the right lines joining 3 adjacent centres, and $\frac{1}{2}$ the area of one of the circles.

PROB. II. To determine the greatest possible density of an infinitely thin crust composed of equal spherules, having their centres all in the same plane.

From the number 39 subtract its third part. To the number 4 add the third part of 39. The remainder is to the sum, that is, 26 is to 17, very nearly, as the space occupied by all the matter, to the space occupied by all the pore in an infinitely thin crust, of the greatest possible density, composed of equal spherules, having all their centres in the same plane.

Demonst. On a base of innumerable infinitely small circles, arranged in the closest manner possible, according to prob. 1, imagine right cylinders to be erected, each cylinder having one of the little circles for its base, and its altitude equal to the diameter of its base. These cylinders are in the closest arrangement possible for equal cylinders; and the spheres, which they circumscribe, are in the closest arrangement possible for equal spheres, which have their centres in the same plane. The solid space occupied by the cylinders, is to the solid space

occupied by their interstices, as the surface covered by their circular bases, to the surface covered by the interstices of their bases. That is, as 39 to 4, very nearly, by the first problem. But the spheres contained within these cylinders are each but $\frac{2}{3}$ of the containing cylinder. The solid content therefore of all the spheres is but $\frac{2}{3}$ of the solid content of all the cylinders; and the remaining 3d part of the solid content of the cylinders, together with the interstices between the cylinders, makes up the whole of the interstices between the spheres. Therefore the space occupied by the spheres, is to the space occupied by their interstices, as $39 - \frac{39}{3}$ to $4 + \frac{39}{3}$, or as 26 to 17, very nearly.

The spheres being in the closest arrangement possible, if each be a solid atom, or without pore within its own dimensions, then the infinitely thin crust, which these atoms compose, is plainly the most dense that can be composed of equal spherules, having their centres in one plane. And the space occupied by its matter, is to the space occupied by its pore, as 26 to 17, very nearly.

Scholium. If the component spherules, instead of being solid, be supposed to be each of the density of gold, in which one half of the bulk may reasonably be supposed to be pore; then only $\frac{1}{2}$ of the space, which they occupy, is filled with matter, and the other half is to be added to the pore. Hence spherules of the density of gold, arranged in the closest manner possible, having their centres in one plane, compose a crust, in which, $\frac{13}{43}$, or somewhat more than $\frac{12}{40}$, of its bulk, is matter. Therefore, the density of such a crust is somewhat greater than 12 times that of water, since $\frac{1}{46}$ only of the bulk of water is supposed to be matter, and $\frac{39}{40}$ is pore.

N. B. The first of these two problems, enabled me to determine the greatest possible number of spherical particles of a given magnitude, that could find room to lie at one time on the surface of the sun; and by the 2d, I found the density of the crust, which such particles, in the closest arrangement possible, with a given density of each particle separately, would compose.

LI. On the Going of an Astronomical Clock. By the Rev. Francis Wollaston, F.R.S. p. 559.

Mr. W.'s clock was made by Holmes. The pendulum rod is of deal, to which the ball is screwed fast; and it is adjusted by a smaller weight underneath. The clock beats dead seconds; and is fastened to a principal wall, independent of the floor. The room never has a fire in it. The transit telescope, with which he made the observations, has an achromatic object glass, of only 14 inches focal length, and magnifies about 15 times; its transverse axis is but 12 inches long, and it is mounted on a vertical axis of 18; being designed for an equal altitude instrument likewise, and so used in some of the observations. It is fastened to a large stone pillar, bedded on the wall of the house; and is

adjusted in the meridian, to a mark 700 feet distant. Mr. W. mentions these particulars, because the observations show that even so small an instrument is capable of tolerable exactness: and it is for that reason he set down the result of all the transits he had taken for a year past; though much fewer would have sufficed for showing the rate of the clock.

In the middle of February, when the first change was, the frost was intense; and the pendulum did not, for some days, throw out so far by about 7′ as it generally did; which was about 1° 37′ on one side, and 1° 40′ on the other. At the change in August, Mr. W. observed no difference. It appears by these trials as if the clock gained in warm and lost in cooler weather: but this is not clear. It began to gain before the weather grew warm. Whether this be owing to damp, or any other causes, longer experience and abler observers may discover. From the observations made by Mr. W., it appears that the rate of the clock was as follows:

1770		Clock + too fast — too slow	Gain or Loss	Numb. of Days	Rate per Day
Nov.	1	+ 0."5			
	17	— 16.9	— 17."4	16	— 1."1
	28	— 25.7	— 8.8	11	— 0.8
Dec.	22	— 1 6.4	— 40.7	24	— 1.7
	30	— 1 22.0	— 15.6	8	— 1.9
1771					
Jan.	13	— 1 45.6	— 23.6	14	— 1.7
Feb.	1	— 2 9.7	— 24.1	19	— 1.3
	14	— 2 15.4	— 5.7	13	— 0.4
March	9	— 2 5.5	+ 10.1	23	+ 0.4
	15	— 2 0.0	+ 5.5	6	+ 0.9
April	1	— 1 49.4	+ 10.6	17	+ 0.6
	13	— 1 37.5	+ 11.9	12	+ 1.0

1771		Clock + too fast — too slow	Gain or Loss	Numb. of Days	Rate per Day
May	5	— 59."5	+ 38."0	22	+ 1."7
	18	— 38.6	+ 20.9	13	+ 1.6
June	1	— 5.5	+ 33.1	14	+ 2.4
	18	+ 36.1	+ 41.6	17	+ 2.4
July	3	+ 58.7	+ 22.6	15	+ 1.5
	21	+ 1 30.9	+ 32.2	18	+ 1.8
Aug.	3	+ 1 57.1	+ 26.2	13	+ 2.0
	16	+ 2 20.9	+ 23.8	13	+ 1.8
	30	+ 2 24.7	+ 3.8	14	+ 0.3
Sept.	15	+ 2 22.4	— 2.3	16	— 0.1
Oct.	1	+ 2 10.7	— 11.7	16	— 0.7
	15	+ 2 3.4	— 7.3	14	— 0.5
	31	+ 1 38.1	— 25.3	16	— 1.6

Mr. W. here adds a few other observations made since he settled in Chislehurst, the lat. of which is 51° 24′ 33″ north, and the longitude is $18^{s}.5$ in time, east of the observatory at Greenwich. These additions consist of observations of the occultations of stars by the moon, and of the eclipses of Jupiter's satellites, not necessary to be here repeated.

LII. Of a Pure Native Crystallized Natron, or Fossil Alkaline Salt, found in the Country of Tripoli in Barbary. By Donald Monro, M.D., F.R.S., &c. p. 567.

It is well known that the nitre, or nitron, of the ancients, which they used for making of glass, * and in their baths,† and for other purposes, was not the

* See an account of the making of glass with nitre and sand in C. Plinii Secundi Hist. Natural. tom. iii. lib. xxxvi. cap. 26, and an account of its medicinal virtues, ibid. lib. xxxi. cap. 10. And

salt which now goes by the name of nitre, or saltpetre; but a salt of an alkaline nature, which at present is commonly called the natron of the ancients, or the fossil alkali. The knowledge of it was entirely lost for several centuries; but was revived in the last, by the Hon. R. Boyle. who, in his Short Memoirs for the Natural Experimental History of Mineral Waters, ‡ after telling us that it is of an alkaline nature, says, 'that he had some of it brought from Egypt, and a neighbouring country, whose name he did not remember.'

However, it was afterwards neglected, and its properties as a distinct species of alkaline salt not known for many years; for though chemists observed, that a Glauber salt and cubic nitre were formed by dislodging the marine acid from sea-salt, by means of the vitriolic and nitrous acids; and from thence suspected that there was something particular in the basis of this salt; yet its true nature was not discovered till Mons. du Hamel du Monceau gave an account, in the Memoirs of the French Royal Academy of Sciences for the year 1736, of his having obtained it pure, in two different ways. 1st. By dislodging the marine acid by means of the vitriolic, and then separating it by the addition of a phlogiston, and forming a hepar sulphuris, from which he precipitated the sulphur by means of the vegetable acid, and then separated this acid from the basis of sea-salt by the force of fire. 2d. By dislodging the marine acid from the sea-salt by the addition of the nitrous, and so forming a cubic nitre, from which he dislodged the acid, by deflagrating it with charcoal; and then he purified the remainder by dissolving it in water, and by filtrating and evaporating the liquor and crystallizing the salt.

After he had obtained the basis of sea-salt quite pure, he tried a number of experiments with it, and with the natron of Egypt; and found that they were entirely of the same nature, and that they were of a distinct species of alkaline salt, different in their properties from the potash, and other alkaline salts, commonly obtained by burning wood, and most other vegetable substances; and that they formed different neutral salts with the 3 mineral acids, and with the vegetable. This salt is likewise got from burning the barilla, the kali, and other marine plants; and all that is at present used in this country, by our manufacturers, has been prepared in this manner.

Hitherto it has not been found native in the western parts of Europe, except in mineral waters, and in the neighbourhood of volcanos, or at places where

Tacitus, in mentioning the river Belus in India, says, 'Circa cujus os collectæ arenæ, admixto nitro, in vitrum excoquuntur.' Lib. v. Hist. sect. 7.—Orig.

† Nitre is mentioned as used in baths, in several parts of the Holy Scripture, particularly by the prophet Jeremiah. See chap. ii. ver. 22. The nitre, or natron, is likewise taken notice of by many other of the ancient authors.—Orig.

‡ See his notes on title 26, p. 86, of the edition printed at London 1684-5.—Orig.

they are alleged to have existed formerly; but it has long been found in Egypt, and near Smyrna, and in other eastern countries, commonly mixed with earth, in a floury or concrete form; in some places pretty pure, in others more mixed.*

In the year 1764, Dr. Wm. Heberden gave an account of a salt of this kind, found on the Pic of Teneriff, where there is a volcano; and added several very ingenious experiments of the Hon. Henry Cavendish, to prove that the vegetable alkali has a greater affinity with acids than the fossil or natron. It is probable that this salt, got at the Pic of Teneriff, is the basis of sea-salt, whose acid has first been dislodged, either by the force of fire, or by the acid of decomposed sulphur, which has afterwards been attracted by a fresh phlogiston, and both separated by the force of fire; though it is not at all impossible but that there may be magazines of this fossil salt lodged native in the bowels of this mountain.

Hitherto there had been no account of its being found any where native in a crystalline form, and in large quantity; and therefore he imagined that the following history would be agreeable to the R. S. In the year 1765, Mrs. White, widow to the late consul White of Tripoli, on her return to this country, showed Dr. D. M. a substance which, she said, had a very particular property of bubbling up, or fermenting, when mixed with lemon juice. Immediately, on seeing and tasting it, he suspected it to be a pure native natron, or fossil alkali; and was confirmed in this opinion, by mixing it with different acids; and he afterwards had a few pounds of it sent home to him, and some gentlemen in the city had imported some hundred weight of it.

On inquiring into the history of this salt, he was told that it was brought yearly to Tripoli, in large quantities, from the mountains in the inland part of the country, and that it went by the name of Trona; that the inhabitants sometimes took an ounce, or more of it, by way of physic, and that it commonly operated both as an emetic and purgative medicine; that the principal use they made of it, was to mix it with their snuff, to give it, what they think, an agreeable sharpness; and that it was yearly sent to Constantinople, in large quantity, to be employed for the same purpose. But so far as he could learn, the Turks are entirely ignorant of its nature, and employ it for no other uses. It is well known that this salt does not run per deliquium, but falls down into a white floury powder, when exposed to the air; and that it makes a harder and firmer soap than the common vegetable alkali, and is alleged to make a purer and finer glass.

This salt, which he had the honour to present to the R. S., was extremely pure, dissolved entirely in water, leaving only a small quantity of a reddish earth behind. He tried what quantity of acid 1 oz. of this salt would saturate, and

* See Hoffman, Phys. Chem. lib. ii. obs. 1. Geoffroy, Mater. Medica, part i. cap. 2. Dr. Shaw's Travels, Excerpt. p. 55, and other authors.—Orig.

found that it saturated as much as near $2\frac{1}{2}$ oz. of the common gross barilla, in the form it is commonly imported. He had it likewise tried by calico printers, and it was found to answer all their purposes, and nearly in the same proportion with respect to the gross barilla, as above mentioned, and he was told that it was thought to answer better than any other salt they had ever tried. Most of the neutral salts made with this alkali and acids, except the cubic nitre, keep long without running per deliquium, even those made with vegetable acids; for most of the neutral salts made with vegetable acids, and with some of this salt, which he had the honour to present to the R. S. in the year 1767, still remained entire, though kept only in a close drawer, in the same tea-cups and small basins, without any cover, as they were shown to the Society.

He had not been able to learn in what particular place of the inland part of Tripoli in Barbary this salt is found, nor how it is disposed of in the bowels of the earth: but it should seem to run in thin veins, of about $\frac{1}{2}$ an inch, or a little more thick, in a bed of sea-salt; for all of it that has hitherto been imported into this country, is covered with sea-salt on each side. The one side is always smoother than the other, and appears as if it had been the basis on which it rested; the other, which should seem to be the upper side, is rougher, by the shooting of the crystals. The pieces of the thin veins appear almost as if the salt had been dissolved in water, and afterwards boiled up into thin crystallized cakes, only that the crystals are much smaller, and disposed in a manner that cannot easily be imitated by art; for when this salt is dissolved, and evaporated to a pellicle, and left to crystallize, it always shoots into crystals resembling those of Glauber salt.

Brown paper dipt into a solution of this salt, after it is dry burns almost as if it had been dipped in a solution of true nitre, as Dr. Heberden had observed of the salt got at the Pic of Teneriff; which shows that it contains more of an inflammable principle than the common vegetable alkali. There are great mines of sea-salt in the country of Tripoli, the salt of which should seem to contain a large proportion of this natron; for he was told that all the meat salted with it acquired a red colour.

This native alkaline salt having never been subjected to the force of fire, is perfectly mild, and contains no caustic parts, as the barilla, and the common potashes prepared by burning wood and plants, or the salts thrown out by volcanos commonly do; and therefore it will be found to be much more useful for bleaching and washing linens, and for clearing and scowering cotton or woollen stuffs, and for many other purposes, than any other alkaline salt hitherto known, at the same time that it will answer every purpose for which the other kinds of the fossil alkali are employed.

When this salt is to be used for making rochelle or other neutral salts, or for

washing or bleaching linen, it ought first to be dissolved in pure water, and the solution be allowed to stand for some time, till the reddish or brown earth has all precipitated to the bottom, and then the pure liquor ought to be poured off, and what remains at the bottom be thrown into a filter; for if this precaution is not taken, the reddish earth is in danger of giving a slight brown or reddish colour to the neutral salts, or to affect the colour of the linen.

LIII. The Quantity of the Sun's Parallax, as Deduced from the Observations of the Transit of Venus, June 3, 1769. By Tho. Hornsby, M.A., Prof. of Astron., Oxford, and F.R.S. p. 574.

The uncertainty as to the quantity of the sun's parallax, deduced from the observations of the transit of Venus in 1761 (whether it arose from the unfavourable position of the planet, so that a sufficient difference of time in the total duration of the transit was not, and indeed could not be, obtained from observations made at different places; or from the disagreement of the observations of different astronomers, which were to serve as terms of comparison) seems now to be entirely removed: and from the observations made in distant parts by the astronomers of different nations, and especially from those made under the patronage and direction of this society, the learned of the present time may congratulate themselves on obtaining as accurate a determination of the sun's distance, as perhaps the nature of the subject will admit.

The two following tables give not only the observations themselves, but also the computed differences of time from which the parallax was deduced.

TABLE I.

Places.	Latitude.	Oberver's Names.	Int. Cont. at Ing.	Int. Cont. at Egr.	Obs. Durat.
Wardhus	70° 22′ 36″ N	F. Hell	$9^{h}\ 34^{m}\ 10^{s}.6$	$15^{h}\ 27^{m}\ 24^{s}.6$	$5^{h}\ 33^{m}\ 14^{s}$
Kola	68 52 56	M. Rumonsky	9 42 4	15 35 23	5 53 19
Hudson's Bay	58 47 32	M. Wales	1 15 21.3	7 0 45.5	5 45 24.2
		M. Dymond	1 15 25.3	7 0 48.5	5 45 23.2
California	23 3 37	Abbè Chappe	0 17 27.9	5 54 50.3	5 37 32.4
K. George's Island	17 28 55 S	Capt. Cook	21 44 15.5	3 14 13	5 29 57.5
		Mr. Green	21 43 55.5	3 14 3	5 30 7.5
		Dr. Solander	21 44 2.5		

TABLE II.

	Observ. durat.	Diff. of comp. durat.	Diff. of observ. durat.	Sun's parallax.
King George's Island	$5^{h}\ 29^{m}\ 52^{s}.5$			
Wardhus	5 53 14	$23^{m}\ 31^{s}.36$	$23^{m}\ 21^{s}.5$	8″.639
Kola	5 53 19	23 41 .09	23 26 .5	8 .611
Hudson's Bay	5 45 23.7	15 51 .90	15 31 .2	8 .511
California	5 37 32.4	7 42 .43	7 29 .9	8 .464
California	5 37 32.4			
Wardhus	5 53 14	15 48 .93	15 51 .6	8 .724
Kola	5 53 19	16 4 .41	15 56 .6	8 .629
Hudson's Bay	5 45 23.7	8 9 .47	8 1 .3	8 .555
Hudson's Bay	5 45 23.7			
Wardhus	5 53 14	7 39 .46	7 50 .3	8 .905
Kola	5 53 19	7 49 .19	7 55 .3	8 .813
Mean of all				8 .650

The 2d column of the 2d table contains the observed duration, or interval of time, between the 2 internal contacts; the 3d contains the difference of each duration, deduced by computation on a supposition that the sun's parallax was $= 8''.7$ on the day of the transit; the 4th, the difference of that duration, as determined by actual observation: In the last column is given the horizontal parallax on the day of the transit, resulting from a comparison of the 3d and 4th columns. In the above comparison he has used Captain Cook's observation at the ingress, and a mean of his and Mr. Green's observations at the egress; because, on a comparison of the observed times at the ingress and egress, made at the several places, when reduced to the centre of the earth, on a supposition that the sun's parallax on the day of the transit was $= 8''.65$, the difference of meridians, as deduced from Captain Cook's observation at the ingress, agrees much better with the same differences deduced from a mean of the 2 observations at the egress, than those derived either from the observation of Mr. Green, Dr. Solander, or even from a mean of all the 3 observations, as appears from the comparison of them.

The near agreement of the difference of meridians between King George's Island and the 4 other places, as deduced from Captain Cook's observation at the ingress, and from a mean of his and Mr. Green's observations at the egress, sufficiently shows that the observed duration at King George's Island is at least $5^h\ 29^m\ 52^s.5$: and, from a comparison made in the same manner with the observations at Hudson's Bay, it might be shown that the time of the egress is uncertain to a few seconds, owing perhaps to the haziness of the air peculiar to that climate, even at the altitude of 10 or 12 degrees.

By the end of the sun's eclipse on the morning after the transit, the longitude of Wardhus from Paris, according to Father Hell, is $1^h\ 55^m\ 6^s$ E. of Paris, or $2^h\ 4^m\ 22^s$ E. of Greenwich; and, according to the observation of Mr. Rumousky, Kola is $2^h\ 2^m\ 55^s$ E. of Paris, or $2^h\ 12^m\ 11^s$ E. of Greenwich. The point therefore at King George's Island, where the transit was observed, is $9^h\ 57^m\ 53^s.6 = 149^\circ\ 28'\ 24''$ w. of Greenwich; Vill St. Joseph in California is $7^h\ 18^m\ 42\frac{1}{2}^s = 109^\circ\ 40'\ 37''$ w. of Greenwich; and Prince of Wales's Fort in Hudson's Bay $6^h\ 16\ 49\frac{1}{2}^s = 94^\circ\ 12'\ 22''$ w. of Greenwich.

From the near agreement of the several results before found, which are independent of the knowledge of the longitude of each place, and affected only by the necessary error in observing, the accuracy of the observation made at the Cape of Good Hope in 1761, by Messrs. Mason and Dixon, is abundantly confirmed; by comparing which with the best observations made in the places whose longitudes were very nearly ascertained, the sun's parallax on the 5th of June was found $= 8'.692$*. And Mr. Pingré, notwithstanding the several arguments

very speciously produced in favour of his own observation at the island of Rodrigues, as represented in his learned memoir on the sun's parallax, will probably be of opinion, that an error of one minute was committed in writing down the time of hi observation, as was conjectured by many persons, as well as myself; a mistake to which the most experienced observer is sometimes liable, when at the time of observation the minute is nearly completed.

The parallax on the 3d of June being 8″.65, the mean parallax will be found to be = 8″.78; and if the semidiameter of the earth be supposed = 3985 English miles, the mean distance of the earth from the sun will be 93,726,900 English miles. And, as the relative distances of the planets are well known, their absolute distances, and consequently the dimensions of the solar system, will be as follows:

	Relative distance.	Absolute distance.
Mercury	387,10	36,281,700
Venus	723,33	67,795,500
Earth	1000,00	93,726,900
Mars	1523,69	142,818,000
Jupiter	5200,98	487,472,000
Saturn	9540,07	894,162,000

LIV. A short Account of some Basalt Hills, in Hessia. By Mr. R. E. Raspe, F. R. S. p. 580.

Mr. R. discovered in the neighbourhood of Cassel, several hills, composed of basalt rocks, formed in polyedrous and mostly pentagonal columns. As this sort of stone has hitherto met with few observers, and affords many curious singularities, Mr. R. gives the following account of his researches. He observes that the German basalt rocks differ from those of the giant's causeway in Ireland, by their want of articulation; and from those anciently found at Syena in Egypt, and described with tolerable exactness by Strabo, lib. 17, by their being less thick, and not exceeding 8 or 10 inches in breadth, on unequal lengths from 5 to 30 feet. The colour, hardness, weight, and substance of these stones sufficiently show them not to belong to the genus of the marbles, among which Mr. Da Costa ranked them, in imitation of the ancients.

Their substance is vitreous, analogous to that of the horny stones; they resist aqua fortis, and the chizzel: and only yield to a violent fire and the engravers wheel. Being worked in this manner they acquire the polish of the ancient basaltes, named by the Italians Marmo paragone. Mr. R. had not completed a chemical analysis of these stones, which he says they richly deserve, chiefly as they contain small nests of crystals of tin ore, yellow, green, and black. These

* See Phil. Trans., vol. 53, for the year 1763. p. 491.

probably greatly contribute towards giving those stones their singular and constant form. They seem to have acquired that form, in a different manner from that which influenced the strata and veins of other mountains. Lastly, no marks or impressions of any organical bodies are found either in the out or inside of these stones.

From all these considerations he was induced to attribute their origin, to a watery crystallization, which might have taken place, either at the first settling of the chaos, or at the time of a dissolution of a great part of our globe. He had said the same thing in regard to the giant's causeway, in his account of the formation of new islands. But he afterwards entertained some doubts about that opinion, for these two reasons. 1. In the explanation of the plates of the French Encyclopedie, he found that an observation made by Mr. Desmarest, has induced him to attribute the origin of these stony columns to the matter of volcanos refrigerated from fusion, having found the Auvergne basaltes placed on beds of lavas and scoriæ, just close to the opening of an extinguished volcano. 2. He discovered the same appearance at Habichswald about Weissenstein near Cassel. The top of the mountain, on which the famous cascades of the Landgrave Charles are built, and which the English troops made the place of their encampment after the battle of Willemstahl, is hardly composed of any thing but enormous pieces of lavas and scoria. Somewhat lower, and near the middle of the mountain, are found the basaltes. Many of these are formed in polyedrous pillars; but some, which are the nearest to the aforesaid lava, only consist of shapeless roundish masses. On the other side of the mountain, and at a small distance from the lavas and scoriæ, is found one of the richest coal mines he ever saw, in a bed of the thickness of 18 feet.

The Duke of Rochefoucalt at Paris, an eminent lover and encourager of natural history, likewise assured him, that at Bolsena in Italy, the basaltes are found near the lavas of an ancient volcano, and that the whole island of Sicily, chiefly on the side of mount Etna, abounds with the same. Hence it may be allowable to attribute with Mr. Desmarest the origin of the basaltes to volcanos. This opinion is further supported by many circumstances; viz. the vitreous, and hitherto problematical substance of these stones; the want of marine bodies, and lastly, the well-known experiment of some melted metals, which, when hardened, appear in crystallizations not unlike those of watery congelations. Mr. R. adds that the other basalt-mountains, which he had seen in Hassia, about Felsberg, Aldenberg, and Gudensberg, exhibits basaltes without any addition; these mountains standing by themselves, and showing no traces of either lavas or scoriæ.

LV. An Attempt to Explain some of the Principal Phenomena of Electricity, by Means of an Elastic Fluid. By the Hon. Henry Cavendish, F.R.S. p. 584.

Since first writing the following paper, Mr. C. found that this way of ac-

counting for the phenomena of electricity, is not new. Æpinus, in his Tentamen Theoriæ Electricitatis et Magnetismi, has made use of the same, or nearly the same hypothesis; and the conclusions he draws from it, agree nearly with Mr. C.'s, as far as he goes. However, as Mr. C. has carried the theory much farther than he has done, and has considered the subject in a different, and in a more accurate manner, he hopes the Society will not think this paper unworthy their acceptance.

The method he proposes to follow is, first, to lay down the hypothesis; next, to examine by strict mathematical reasoning, or at least, as strict reasoning as the nature of the subject will admit of, what consequences will flow from thence; and lastly, to examine how far these consequences agree with such experiments as have yet been made on this subject. In a future paper, he intends to give the result of some experiments he was making, with intent to examine still further the truth of this hypothesis, and to find out the law of the electric attraction and repulsion.

Hypothesis.—There is a substance, which Mr. C. calls the electric fluid, the particles of which repel each other, and attract the particles of all other matter, with a force inversely as some less power of the distance than the cube: the particles of all other matter also repel each other, and attract those of the electric fluid, with a force varying according to the same power of the distances. Or, to express it more concisely, if we consider the electric fluid as matter of a contrary kind to other matter, the particles of all matter, both those of the electric fluid and of other matter, repel particles of the same kind, and attract those of a contrary kind, with a force inversely as some less power of the distance than the cube. For the future, he would be understood never to comprehend the electric fluid under the word matter, but only some other sort of matter.

It is indifferent whether we suppose all sorts of matter to be indued in an equal degree with the foregoing attraction and repulsion, or whether we suppose some sorts to be indued with it in a greater degree than others; but it is likely that the electric fluid is indued with this property in a much greater degree than other matter; for in all probability the weight of the electric fluid in any body bears but a very small proportion to the weight of the matter; but yet the force with which the electric fluid therein attracts any particle of matter, must be equal to the force with which the matter therein repels that particle; otherwise the body would appear electrical, as will be shown hereafter. To explain this hypothesis more fully, suppose that 1 grain of electric fluid attracts a particle of matter, at a given distance, with as much force as n grains of any matter, lead for instance, repel it: then will 1 grain of electric fluid repel a particle of electric fluid with as much force as n grains of lead attract it; and 1 grain of electric fluid will

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repel 1 grain of electric fluid with as much force as n grains of lead repel n grains of lead.

All bodies in their natural state, with regard to electricity, contain such a quantity of electric fluid interspersed between their particles, that the attraction of the electric fluid in any small part of the body, on a given particle of matter, shall be equal to the repulsion of the matter in the same small part on the same particle. A body in this state Mr. C. calls saturated with electric fluid; if the body contains more than this quantity of electric fluid, he calls it overcharged: if less, he calls it undercharged. This is the hypothesis; he now proceeds to examine the consequences which will flow from it.

Lemma 1.—Let EAe (pl. 6, fig. 1) represent a cone continued infinitely; let A be the vertex, and Bb and Dd planes parallel to the base; and let the cone be filled with uniform matter, whose particles repel each other with a force inversely as the n power of the distance. If n is greater than 3, the force with which a particle at A is repelled by EBbe, or all that part of the cone beyond Bb, is as $\frac{1}{AB^{n-3}}$. For supposing AB to flow, the fluxion of EBbe is proportional to $-\dot{AB} \times AB^2$, and the fluxion of its repulsion on A is proportional to $\frac{-\dot{AB}}{AB^{n-2}}$; the fluent of which is $\frac{1}{n-3 \times AB^{n-3}}$; which when AB is infinite is equal to nothing; consequently the repulsion of EBbe is proportional to $\frac{1}{n-3 \times AB^{n-3}}$ or to $\frac{1}{AB^{n-3}}$.

Corol. If AB is infinitely small, $\frac{1}{AB^{n-3}}$ is infinitely great; therefore the repulsion of that part of the cone between A and Bb, on A, is infinitely greater than the repulsion of all that beyond it.

Lemma 2.—By the same method of reasoning it appears, that if n is equal to 3, the repulsion of the matter between Bb and Dd on a particle at A, is proportional to the logarithm of $\frac{AD}{AB}$; consequently, the repulsion of that part is infinitely small in respect of that between A and Bb, and also infinitely small in respect of that beyond Dd.

Lemma 3.—In like manner, if n is less than 3, the repulsion of the part between A and Bb on A is proportional to AB^{3-n}: consequently the repulsion of the matter between A and Bb on A, is infinitely small in respect of that beyond it.

Corol. It is easy to see, from these 3 lemmata, that if the electric attraction and repulsion had been supposed to be inversely as some higher power of the distance than the cube; a particle could not have been sensibly affected by the repulsion of any fluid, except what was placed close to it. If the repulsion was inversely as the cube of the distance, a particle could not be sensibly affected by the repulsion of any finite quantity of fluid, except what was close to it. But

as the repulsion is supposed to be inversely as some power of the distance less than the cube, a particle may be sensibly affected by the repulsion of a finite quantity of fluid, placed at any finite distance from it.

Definition. If the electric fluid in any body, is by any means confined in such manner that it cannot move from one part of the body to the other, Mr. C. calls it immoveable: if it is able to move readily from one part to another, he calls it moveable.

Prop. 1.—A body overcharged with electric fluid attracts or repels a particle of matter or fluid, and is attracted or repelled by it, with exactly the same force as it would, if the matter in it, together with so much of the fluid as is sufficient to saturate it, was taken away, or as if the body consisted only of the redundant fluid in it. In like manner an undercharged body attracts or repels with the same force, as if it consisted only of the redundant matter; the electric fluid, together with so much of the matter as is sufficient to saturate it, being taken away.—This is evident from the definition of saturation.

Prop. 2.—Two over or undercharged bodies attract or repel each other with just the same force that they would, if each body consisted only of the redundant fluid in it, if overcharged, or of the redundant matter in it, if undercharged.—For, let the two bodies be called A and B: by the last proposition, the redundant substance in B impels each particle of fluid and matter in A, and consequently impels the whole body A, with the same force that the whole body B impels it: for the same reason the redundant substance in A impels the redundant substance in B, with the same force that the whole body A impels it. It is shown therefore, that the whole body B impels the whole body A, with the same force that the redundant substance in B impels the whole body A, or with which the whole body A impels the redundant substance in B; and that the whole body A impels the redundant substance in B, with the same force that the redundant substance in A impels the redundant substance in B. Therefore the whole body B impels the whole body A, with the same force with which the redundant substance in A impels the redundant substance in B, or with which the redundant substance in B impels the redundant substance in A.

Corol. Let the matter in all the rest of space, except in 2 given bodies, be saturated with immoveable fluid; and let the fluid in those 2 bodies be also immoveable. Then, if one of the bodies is saturated, and the other either over or undercharged, they will not at all attract or repel each other. If the bodies are both overcharged, they will repel each other. If they are both undercharged, they will also repel each other. If one is overcharged and the other undercharged, they will attract each other.

N. B. In this corollary, when Mr. C. calls a body overcharged, he would be understood to mean, that it is overcharged in all parts, or at least no where un-

dercharged; in like manner, when he calls it undercharged, he means that it is undercharged in all parts, or at least no where overcharged.

Prop. 3.—If all the bodies in the universe are saturated with electric fluid, it is plain that no part of the fluid can have any tendency to move.

Prop. 4.—If the quantity of electric fluid in the universe is exactly sufficient to saturate the matter therein, but unequally dispersed, so that some bodies are overcharged and others undercharged; then, if the electric fluid is not confined, it will immediately move till all the bodies in the universe are saturated.—For, supposing that any body is overcharged, and the bodies near it are not, a particle at the surface of that body will be repelled from it by the redundant fluid within; consequently some fluid will run out of that body; but if the body is undercharged, a particle at its surface will be attracted towards the body by the redundant matter within, so that some fluid will run into the body.

N. B. In prob. 4, case 3, there will be shown an exception to this proposition: there may perhaps be some other exceptions to it: but he thinks there can be no doubt that this proposition must hold good in general.

Lemma 4.—Let BDE, *bde*, and $\beta\delta\epsilon$ (fig. 2) be concentric spherical surfaces, whose centre is C: if the space* B*b* is filled with uniform matter, whose particles repel with a force inversely as the square of the distance; a particle placed any where within the space C*b*, as at P, will be repelled with as much force in one direction as another, or it will not be impelled in any direction. This is demonstrated in Newt. Princip. lib. 1 prop. 70. It follows also from his demonstration, that if the repulsion is inversely as some higher power of the distance than the square, the particle P will be impelled towards the centre; and if the repulsion is inversely as some lower power than the square, it will be impelled from the centre.

Lemma 5.—If the repulsion is inversely as the square of the distance, a particle placed any where without the sphere BDE, is repelled by that sphere, and also by the space B*b*, with the same force that it would if all the matter therein was collected in the centre of the sphere; provided the density of the matter in it is every where the same at the same distance from the centre. This is easily deduced from prop. 71 of the same book, and has been demonstrated by other authors.

Prop. 5, *prob.* 1.—Let the sphere BDE be filled with uniform solid matter, overcharged with electric fluid; let the fluid in it be moveable, but unable to escape from it: let the fluid in the rest of infinite space be moveable, and sufficient to saturate the matter in it; and let the matter in the whole of infinite

* By the space B*b* or Bβ, Mr. C. means the space comprehended between the spherical surfaces BDE and *bde*, or between BDE and $\beta\delta\epsilon$: by the space C*b* or Cβ, he means the spheres *bde* or $\beta\delta\epsilon$.—Orig.

space, or at least in the space Bβ, whose dimensions will be given below, be uniform and solid; and let the law of the electric attraction and repulsion be inversely as the square of the distance; it is required to determine in what manner the fluid will be disposed both within and without the globe.

Take the space B*b* such, that the interstices between the particles of matter in it shall be just sufficient to hold a quantity of electric fluid, whose particles are pressed close together, so as to touch each other, equal to the whole redundant fluid in the globe, besides the quantity requisite to saturate the matter in B*b*; and take the space Bβ such, that the matter in it shall be just able to saturate the redundant fluid in the globe: then, in all parts of the space B*b*, the fluid will be pressed close together, so that its particles shall touch each other; the space Bβ will be entirely deprived of fluid; and in the space C*b*, and all the rest of infinite space, the matter will be exactly saturated.

For, if the fluid is disposed in the abovementioned manner, a particle of fluid placed any where within the space C*b* will not be impelled in any direction by the fluid in B*b*, or the matter in Bβ, and will therefore have no tendency to move: a particle placed any where without the sphere βδε will be attracted with just as much force by the matter in Bβ, as it is repelled by the redundant fluid in B*b*, and will therefore have no tendency to move: a particle placed any where within the space B*b*, will indeed be repelled towards the surface, by all the redundant fluid in that space which is placed nearer the centre than itself; but as the fluid in that space is already pressed as close together as possible, it will not have any tendency to move; and in the space Bβ there is no fluid to move, so that no part of the fluid can have any tendency to move.

Moveover, it seems impossible for the fluid to be at rest, if it is disposed in any other form; for if the density of the fluid is not every where the same at the same distance from the centre, but is greater near *b* than near *d*, a particle placed any where between those two points will move from *b* towards *d*; but if the density is every where the same at the same distance from the centre, and the fluid in B*b* is not pressed close together, the space C*b* will be overcharged, and consequently a particle at *b* will be repelled from the centre, and cannot be at rest: in like manner, if there is any fluid in Bβ, it cannot be at rest: and, by the same kind of reasoning, it might be shown, that, if the fluid is not spread uniformly within the space C*b*, and without the sphere βδε, it cannot be at rest.

Corol. 1. If the globe BDE is undercharged, every thing else being the same as before, there will be a space B*b*, in which the matter will be entirely deprived of fluid, and a space Bβ, in which the fluid will be pressed close together; the matter in B*b* being equal to the whole redundant matter in the globe, and the redundant fluid in Bβ, being just sufficient to saturate the matter in B*b*: and in

all the rest of space the matter will be exactly saturated. The demonstration is exactly similar to the foregoing.

Corol. 2. The fluid in the globe BDE will be disposed in exactly the same manner, whether the fluid without is immoveable, and disposed in such manner that the matter shall be every where saturated, or whether it is disposed as above described; and the fluid without the globe will be disposed in just the same manner, whether the fluid within is disposed uniformly, or whether it is disposed as above described.

Prop. 6, *prob.* 2.—To determine in what manner the fluid will be disposed in the globe BDE, supposing every thing as in the last problem, except that the fluid on the outside of the globe is immoveable, and disposed in such manner as every where to saturate the matter, and that the electric attraction and repulsion is inversely as some other power of the distance than the square.

I am not able, says Mr. C., to answer this problem accurately; but I think we may be certain of the following circumstances.

Case 1. Let the repulsion be inversely as some power of the distance between the square and the cube, and let the globe be overcharged. It is certain that the density of the fluid will be every where the same at the same distance from the centre. Therefore, first, there can be no space, as c*b*, within which the matter will be every where saturated; for a particle at *b* is impelled towards the centre, by the redundant fluid in B*b*, and will therefore move towards the centre, unless c*b* is sufficiently overcharged to prevent it. Secondly, the fluid close to the surface of the sphere will be pressed close together; for otherwise a particle so near to it, that the quantity of fluid between it and the surface should be very small, would move towards it; as the repulsion of the small quantity of fluid between it and the surface, would be unable to balance the repulsion of the fluid on the other side. Whence, he thinks, we may conclude, that the density of the fluid will increase gradually from the centre to the surface, where the particles will be pressed close together. Whether the matter exactly at the centre will be overcharged, or only saturated, he cannot tell.

Corol. For the same reason, if the globe be undercharged, he thinks we may conclude, that the density of the fluid will diminish gradually from the centre to the surface, where the matter will be entirely deprived of fluid.

Case 2. Let the repulsion be inversely as some power of the distance less than the square; and let the globe be overcharged. There will be a space B*b*, in which the particles of the fluid will be every where pressed close together; and the quantity of redundant fluid in that space will be greater than the quantity of redundant fluid in the whole globe BDE; so that the space c*b*, taken all together, will be undercharged. But he cannot tell in what manner the fluid will be disposed in that space. For it is certain that the density of the fluid will be every

where the same at the same distance from the centre. Therefore, let b be any point where the fluid is not pressed close together, then will a particle at b be impelled towards the surface, by the redundant fluid in the space Bb; therefore, unless the space cb is undercharged, the particle will move towards the surface.

Corol. For the same reason, if the globe is undercharged, there will be a space Bb, in which the matter will be entirely deprived of fluid, the quantity of matter in it being more than the whole redundant matter in the globe; and consequently the space cb, taken all together, will be overcharged.

Lemma 6.—Let the whole space comprehended between two parallel planes, infinitely extended each way, be filled with uniform matter, the repulsion of whose particles is inversely as the square of the distance; the plate of matter formed thereby will repel a particle of matter with exactly the same force, at whatever distance from it, it be placed.

For, suppose that there are two such plates, of equal thickness, placed parallel to each other, let A (fig. 3) be any point not placed in or between the two plates: let BCD represent any part of the nearest plate: draw the lines AB, AC, and AD, cutting the farthest plate in b, c, and d; for it is plain, that if they cut one plate, they must, if produced, cut the other; the triangle BCD is to the triangle bcd, as AB^2 to Ab^2; therefore a particle of matter at A will be repelled with the same force by the matter in the triangle BCD, as by that in bcd. Whence it appears, that a particle at A will be repelled with as much force by the nearest plate, as by the more distant; and consequently will be impelled with the same force by either plate, at whatever distance from it it be placed.

Corol. If the repulsion of the particles is inversely as some higher power of the distance than the square, the plate will repel a particle with more force, if its distance be small than if it be great; and if the repulsion is inversely as some lower power than the square, it will repel a particle with less force, if its distance be small, than if it be great.

Prop. 7, *prob.* 3.—In fig. 4 let the parallel lines Aa, Bb, &c. represent parallel planes infinitely extended each way: let the spaces* AD and EH be filled with uniform solid matter: let the electric fluid in each of those spaces be moveable and unable to escape; and let all the rest of the matter in the universe be saturated with immoveable fluid; and let the electric attraction and repulsion be inversely as the square of the distance. It is required to determine in what manner the fluid will be disposed in the spaces AD and EH, according as one or both of them are over or undercharged.

Let AD be that space which contains the greatest quantity of redundant fluid,

* By the space AD or AB, &c. is meant the space comprehended between the planes Aa and Dd, or between Aa and Bb—Orig.

if both spaces are overcharged, or which contains the least redundant matter, if both are undercharged; or, if one is overcharged, and the other undercharged, let AD be the overcharged one. Then, first, there will be two spaces, AB and GH, which will either be entirely deprived of fluid, or in which the particles will be pressed close together; namely, if the whole quantity of fluid in AD and EH together, is less than sufficient to saturate the matter therein, they will be entirely deprived of fluid; the quantity of redundant matter in each being half the whole redundant matter in AD and EH together: but if the fluid in AD and EH together is more than sufficient to saturate the matter, the fluid in AB and GH will be pressed close together; the quantity of redundant fluid in each being half the whole redundant fluid in both spaces. 2d. In the space CD the fluid will be pressed close together; the quantity of fluid in it being such, as to leave just enough fluid in BC to saturate the matter in it. 3d. The space EF will be entirely deprived of fluid; the quantity of matter in it being such, that the fluid in FG shall be just sufficient to saturate the matter in it: consequently the redundant fluid in CD will be just sufficient to saturate the redundant matter in EF; for as AB and GH together contain the whole redundant fluid or matter in both spaces, the spaces BD and EG together contain their natural quantity of fluid; and therefore, as BC and FG each contain their natural quantity of fluid, the spaces CD and EF together contain their natural quantity of fluid. And, 4th, the spaces BC and FG will be saturated in all parts.

For, 1st. if the fluid is disposed in this manner, no particle of it can have any tendency to move: for a particle placed anywhere in the spaces BC and FG, is attracted with just as much force by EF, as it is repelled by CD; and it is repelled or attracted with just as much force by AB, as it is in a contrary direction by GH, and consequently has no tendency to move. A particle placed any where in the space CD, or in the spaces AB and GH, if they are overcharged, is indeed repelled with more force towards the planes D*d*, A*a*, and H*h*, than it is in the contrary direction; but as the fluid in those spaces is already as much compressed as possible, the particle will have no tendency to move.

2d. It seems impossible that the fluid should be at rest, if it is disposed in any other manner: but as this part of the demonstration is exactly similar to the latter part of that of problem the first, it is omitted.

Corol. 1. If the two spaces AD and EH are both overcharged, the redundant fluid in CD is half the difference of the redundant fluid in those spaces: for half the difference of the redundant fluid in those spaces, added to the quantity in AB, which is half the sum, is equal to the whole quantity in AD. For a like reason, if AD and EH are both undercharged, the redundant matter in EF is half the difference of the redundant matter in those spaces; and if AD is overcharged, and EH undercharged, the redundant fluid in CD exceeds half the redundant

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fluid in AD, by a quantity sufficient to saturate half the redundant matter in EH.

Corol 2. It was before said, that the fluid in the spaces AB and GH (when there is any fluid in them) is repelled against the planes A*a* and H*h*; and consequently would run out through those planes, if there was any opening for it to do so. The force with which the fluid presses against the planes A*a* and H*h*, is that with which the redundant fluid in AB is repelled by that in GH; that is, with which half the redundant fluid in both spaces is repelled by an equal quantity of fluid. Therefore the pressure against A*a* and H*h* depends only on the quantity of redundant fluid in both spaces together, and not at all on the thickness or distance of those spaces, or on the proportion in which the fluid is divided between the two spaces. If there is no fluid in AB and GH, a particle placed on the outside of the spaces AD and EH, contiguous to the planes A*a* or H*h*, is attracted towards those planes by all the matter in AB and GH, *id est*, by all the redundant matter in both spaces; and consequently endeavours to insinuate itself in the space AD or EH; and the force with which it does so, depends only on the quantity of redundant matter in both spaces together. The fluid in CD also presses against the plane D*d*, and the force with which it does so, is that with which the redundant fluid in CD is attracted by the matter in EF.

Corol. 3. If AD is overcharged, and EH undercharged, and the redundant fluid in AD is exactly sufficient to saturate the redundant matter in EH, all the redundant fluid in AD will be collected in the space CD, where it will be pressed close together: the space EF will be entirely deprived of fluid, the quantity of matter in it being just sufficient to saturate the redundant fluid in CD, and the spaces AC and FH will be every where saturated. Moreover, if an opening is made in the planes A*a* or H*h*, the fluid within the spaces AD or EH will have no tendency to run out at it, nor will the fluid on the outside have any tendency to run in at: a particle of fluid too placed any where on the outside of both spaces, as at P, will not be at all attracted or repelled by those spaces, any more than if they were both saturated; but a particle placed any where between those spaces, as at S, will be repelled from *d* towards *e*; and if a communication was made between the two spaces, by the canal *de*, the fluid would run out of AD into EH, till they were both saturated.

Prop. 8. *Prob.* 4.—To determine in what manner the fluid will be disposed in the space AD, supposing that all the rest of the universe is saturated with immoveable fluid, and that the electric attraction and repulsion is inversely as some other power of the distance than the square. I am not able, says Mr. C., to answer this problem accurately, except when the repulsion is inversely as the simple or some lower power of the distance; but I think we may be certain of the following circumstances.

Case 1.—Let the repulsion be inversely as some power of the distance between the square and the cube, and let AD be overcharged. 1st. It is certain that the density of the fluid must be every where the same, at the same distance from the planes A*a* and D*d*. 2d. There can be no space, as BC, of any sensible breadth, in which the matter will not be overcharged. And, 3d. The fluid close to the planes A*a* and D*d* will be pressed close together. Whence he thinks, we may conclude, that the density of the fluid will increase gradually from the middle of the space to the outside, where it will be pressed close together. Whether the matter exactly in the middle will be overcharged, or only saturated, he cannot tell.

Case 2. Let the repulsion be inversely as some power of the distance between the square and the simple power, and let AD be overcharged. There will be two spaces, AB and DC, in which the fluid will be pressed close together, and the quantity of redundant fluid in each of those spaces will be more than half the redundant fluid in AD; so that the space BC, taken all together, will be undercharged; but he cannot tell in what manner the fluid will be disposed in that space. The demonstrations of these two cases are exactly similar to those of the two cases of prob. 2.

Case 3. If the repulsion is inversely as the simple, or some low power, of the distance, and AD is overcharged, all the fluid will be collected in the spaces AB and CD, and BC will be entirely deprived of fluid. If AD contains just fluid enough to saturate it, and the repulsion is inversely as the distance, the fluid will remain in equilibrio, in whatever manner it is disposed; provided its density is every where the same, at the same distance from the planes A*a* and D*d*, but if the repulsion is inversely as some less power than the simple one, the fluid will be in equilibrio, whether it is either spread uniformly, or whether it is all collected in that plane which is in the middle between A*a* and D*d*, or whether it is all collected in the spaces AB and CD; but not, he believes, if it is disposed in any other manner. The demonstration depends on this circumstance; namely, that if the repulsion is inversely as the distance, two spaces, AB and CD, repel a particle, placed either between them, or on the outside of them, with the same force as if all the matter of those spaces was collected in the middle plane between them. It is needless mentioning the 3 cases in which AD is undercharged, as the reader will easily supply the place.

Though the 4 foregoing problems do not immediately tend to explain the phenomena of electricity, Mr. C. chose to insert them; partly because they seem worth engaging our attention in themselves; and partly because they serve, in some measure, to confirm the truth of some of the following propositions, in which he was obliged to make use of a less accurate kind of reasoning.

In the following propositions, Mr. C. always supposes the bodies he speaks of to consist of solid matter, confined to the same spot, so as not to be able to alter

its shape or situation by the attraction or repulsion of other bodies on it: he also supposes the electric fluid in these bodies to be moveable, but unable to escape, unless when otherwise expressed. As for the matter in all the rest of the universe, he supposes it to be saturated with immoveable fluid. He also supposes the electric attraction and repulsion to be inversely as any power of the distance less than the cube, except when otherwise expressed.

By a canal, he means a slender thread of matter, of such kind that the electric fluid shall be able to move readily along it, but shall not be able to escape from it, except at the ends, where it communicates with other bodies. Thus, when he says that two bodies communicate with each other by a canal, he means that the fluid shall be able to pass readily from one body to the other by that canal.

Prop. 9. If any body, at a distance from any over or undercharged body, be overcharged, the fluid within it will be lodged in greater quantity near the surface of the body than near the centre. For, if you suppose it to be spread uniformly all over the body, a particle of fluid in it, near the surface, will be repelled towards the surface by a greater quantity of fluid than that by which it is repelled from it; consequently the fluid will flow towards the surface, and make it denser there: moreover, the particles of fluid close to the surface will be pressed close together; for otherwise, a particle placed so near it, that the quantity of redundant fluid between it and the surface should be very small, would move towards it; as the small quantity of redundant fluid between it and the surface would be unable to balance the repulsion of that on the other side.

From the 4 foregoing problems it seems likely, that if the electric attraction or repulsion is inversely as the square of the distance, almost all the redundant fluid in the body will be lodged close to the surface, and there pressed close together, and the rest of the body will be saturated. If the repulsion is inversely as some power of the distance between the square and the cube, it is likely that all parts of the body will be overcharged: and if it is inversely as some less power than the square, it is likely that all parts of the body, except those near the surface will be undercharged.

Corol. For the same reason, if the body is undercharged, the deficiency of fluid will be greater near the surface than near the centre, and the matter near the surface will be entirely deprived of fluid. It is likely too, if the repulsion is inversely as some higher power of the distance than the square, that all parts of the body will be undercharged: if it is inversely as the square, that all parts, except near the surface will be saturated, and if it is inversely as some less power than the square, that all parts, except near the surface, will be overcharged.

Prop. 10. Let the bodies A and D (fig. 5), communicate with each other, by the canal EF; and let one of them, as D, be overcharged; the other body A will

be so also. For as the fluid in the canal is repelled by the redundant fluid in D, it is plain, that unless A was overcharged, so as to balance that repulsion, the fluid would run out of D into A. In like manner, if one is undercharged, the other must be so too.

Prop. 11. Let the body A (fig. 6) be either saturated or over or undercharged; and let the fluid within it be in equilibrio. Let now the body B, placed near it, be rendered overcharged, the fluid within it being supposed immoveable, and disposed in such manner, that no part of it shall be undercharged; the fluid in A will no longer be in equilibrio, but will be repelled from B; therefore the fluid will flow from those parts of A which are nearest to B, to those which are more distant from it; and consequently the part adjacent to MN (that part of the surface of A which is turned towards B) will be made to contain less electric fluid than it did before, and that adjacent to the opposite surface RS will contain more than before.

It must be observed, that when a sufficient quantity of fluid has flowed from MN towards RS, the repulsion which the fluid in the part adjacent to MN exerts on the rest of the fluid in A, will be so much weakened, and the repulsion of that in the part near RS will be so much increased, as to compensate the repulsion of B, which will prevent any more fluid flowing from MN to RS. The reason why he supposes the fluid in B to be immoveable, is, that otherwise a question might arise, whether the attraction or repulsion of the body A, might not cause such an alteration in the disposition of the fluid in B, as to cause some parts of it to be undercharged; which might make it doubtful, whether B did on the whole repel the fluid in A. It is evident however, that this proposition would hold good, though some parts of B were undercharged, provided it did on the whole repel the fluid in A.

Corol. If B had been made undercharged, instead of overcharged, it is plain that some fluid would have flowed from the farther part RS to the nearer part MN, instead of from MN to RS.

Prop. 12. Let us now suppose that the body A communicates by the canal EF, with another body D, placed on the contrary side of it from B, as in fig. 5; and let these two bodies be either saturated, or over or undercharged; and let the fluid within them be in equilibrio. Let now the body B be overcharged: it is plain that some fluid will be driven from the nearer part MN to the farther part RS, as in the former proposition; and also some fluid will be driven from RS, through the canal, to the body D; so that the quantity of fluid in D will thus be increased, and the quantity in A, taking the whole body together, will be diminished, the quantity in the part near MN will also be diminished: but whether the quantity in the part near RS will be diminished or not, does not appear for certain; but Mr. C. imagines it would be not much altered.

Corol. In like manner, if B is made undercharged, some fluid will flow from D to A, and also from that part of A near RS, to the part near MN.

Prop. 13. Suppose now that the bodies A and D communicate by the bent canal MPN*npm* (fig. 7), instead of the straight one EF: let the bodies be either saturated or over or undercharged, as before; and let the fluid be at rest; then if the body B is made overcharged, some fluid will still run out of A into D; provided the repulsion of B on the fluid in the canal is not too great.

The repulsion of B on the fluid in the canal, will at first drive some fluid out of the leg MP*pm* into A, and out of NP*pn* into D, till the quantity of fluid in that part of the canal which is nearest to B is so much diminished, and its repulsion on the rest of the fluid in the canal is so much diminished also, as to compensate the repulsion of B: but as the leg NP*pn* is longer than the other, the repulsion of B on the fluid in it will be greater; consequently some fluid will run out of A into D, on the same principle that water is drawn out of a vessel through a syphon: but if the repulsion of B on the fluid in the canal is so great, as to drive all the fluid out of the space GPH*pg*, so that the fluid in the leg MG*pm* does not join to that in NH*pn*; then it is plain that no fluid can run out of A into D; any more than water will run out of a vessel through a syphon, if the height of the bend of the syphon above the water in the vessel, is greater than that to which water will rise in vacuo.

Corol. If B is made undercharged, some fluid will run out of D into A; and that though the attraction of B on the fluid in the canal is ever so great.

Prop. 14. Let ABC, fig. 8, be a body overcharged with immoveable fluid, uniformly spread; let the bodies near ABC on the outside be saturated with immoveable fluid; and let D be a body inclosed within ABC, and communicating by the canal DG with other distant bodies saturated with fluid; and let the fluid in D and the canal and those bodies be moveable; then will the body D be rendered undercharged.

For let us first suppose that D and the canal are saturated, and that D is nearer to B than to the opposite part of the body C; then will all the fluid in the canal be repelled from C by the redundant fluid in ABC; but if D is nearer to C than to B, take the point F, such that a particle placed there would be repelled from C with as much force, as one at D is repelled towards C; the fluid in DF, taking the whole together, will be repelled with as much force one way as the other, and the fluid in FG is all of it repelled from C: therefore in both cases the fluid in the canal, taking the whole together, is repelled from C; consequently some fluid will run out of D and the canal, till the attraction of the unsaturated matter there is sufficient to balance the repulsion of the redundant fluid in ABC.

Prop. 15. If we now suppose that the fluid on the outside of ABC is moveable; the matter adjacent to ABC on the outside will become undercharged.

Mr. C. sees no reason however to think that that will prevent the body D from being undercharged; but he cannot say exactly what effect it will have, except when ABC is spherical, and the repulsion is inversely as the square of the distance; in this case it appears, by prob. 1, that the fluid in the part DB of the canal will be repelled from C, with just as much force as in the last proposition; but the fluid in the part BG will not be repelled at all: consequently D will be undercharged, but not so much as in the last proposition.

Corol. If ABC is now supposed to be undercharged, it is certain that D will be overcharged, provided the matter near ABC on the outside is saturated with immoveable fluid; and there is great reason to think that it will be so, though the fluid in that matter is moveable.

Prop. 16. Let AEFB, fig. 9, be a long cylindric body, and D an undercharged body; and let the quantity of fluid in AEFB be such, that the part near EF shall be saturated. It appears, from what has been said before, that the part near AB will be overcharged; and moreover there will be a certain space, as A*ab*B, adjoining to the plane AB, in which the fluid will be pressed close together; and the fluid in that space will press against the plane AB, and will endeavour to escape from it; and, by prop. 2, the two bodies will attract each other: then the force with which the fluid presses against the plane AB, is very nearly the same with which the two bodies attract each other in the direction EA; provided that no part of AEFB is undercharged.

Suppose so much of the fluid in each part of the cylinder, as is sufficient to saturate the matter in that part, to become solid; the remainder, or the redundant fluid remaining fluid as before. In this case the pressure against the plane AB must be exactly equal to that with which the two bodies attract each other, in the direction EA: for the force with which D attracts that part of the fluid which we supposed to become solid, is exactly equal to that with which it repels the matter in the cylinder; and the redundant fluid in E*ab*F is at liberty to move, if it had any tendency to do so, without moving the cylinder; so that the only thing which has any tendency to impel the cylinder in the direction EA, is the pressure of the redundant fluid in A*ab*B against AB; and as the part near EF is saturated, there is no redundant fluid to press against the plane EF, and thus to counteract the pressure against AB. Suppose now all the electric fluid in the cylinder to become fluid; the force with which the two bodies attract each other, will remain exactly the same; and the only alteration in the pressure against AB, will be, that that part of the fluid in A*ab*B, which we at first supposed solid and unable to press against the plane, will now be at liberty to press against it; but as the density of the fluid, when its particles are pressed close together, may be supposed many times greater than when it is no denser than sufficient to saturate

the matter in the cylinder, and consequently the quantity of redundant fluid in AabB many times greater than that which is required to saturate the matter in it, it follows that the pressure against AB will be very little more than on the first supposition.

N. B. If any part of the cylinder is undercharged, the pressure against AB is greater than the force with which the bodies attract. If the electric repulsion is inversely as the square or some higher power of the distance, it seems very unlikely that any part of the cylinder should be undercharged; but if the repulsion is inversely as some lower power than the square, it is not improbable but some part of the cylinder may be undercharged.

Lemma 7. Let AB, fig. 10, represent an infinitely thin flat circular plate, seen edgeways, so as to appear to the eye as a straight line; let C be the centre of the circle; and let DC, passing through C, be perpendicular to the plane of the plate; and let the plate be of uniform thickness, and consist of uniform matter, whose particles repel with a force inversely as the n power of the distance; n being greater than 1, and less than 3: the repulsion of the plate on a particle at D, is proportional to $\frac{DC}{DC^{n-1}} - \frac{DC}{DA^{n-1}}$; provided the thickness of the plate and size of the particle D is given.

For if CA is supposed to flow, the corresponding fluxion of the quantity of matter in the plate, is proportional to CA $\times$ C·A; and the corresponding fluxion of the repulsion of the plate on the particle D, in the direction DC, is proportional to $\frac{CA \times C\dot{}A}{DA^n} \times \frac{DC}{DA}, = \frac{D\dot{}A \times DC}{DA^n}$; for D·A is to C·A :: CA : DA; the variable part of the fluent of which is $\frac{-DC}{\overline{n-1} \times DA^{n-1}}$: whence the repulsion of the plate on the particle D, is proportional to

$$\frac{DC}{\overline{n-1} \times DC^{n-1}} - \frac{DC}{\overline{n-1} \times DA^{n-1}}, \text{ or to } \frac{DC}{DC^{n-1}} - \frac{DC}{DA^{n-1}}.$$

Corol. If DC^{n-1} is very small in respect of CA^{n-1}, the particle D is repelled with very nearly the same force as if the diameter of the plate was infinite.

Lemma. Let L and l represent the two legs of a right angled triangle, and h the hypothenuse; if the shorter leg l is so much less than the other, that l^{n-1} is very small in respect of L^{n-1}, then $h^{3-n} - L^{3-n}$ will be very small in respect of l^{3-n}. For $h^{3-n} = (L^2 + l^2)^{\frac{3-n}{2}} = L^{3-n} \times (1 + \frac{l^2}{L^2})^{\frac{3-n}{2}} = L^{3-n} \times 1 + \frac{\overline{3-n} \times l^2}{2L^2} - \frac{\overline{3-n} \times \overline{n-1} \times l^4}{8L^4}$, &c. therefore $h^{3-n} - L^{3-n} = \frac{\overline{3-n} \times l^2}{2L^{n-1}} - \frac{\overline{3-n} \times \overline{n-1} \times l^4}{8L^{n+1}}$ &c. $= \frac{l^{3-n} \times \overline{3-n} \times l^{n-1}}{2L^{n-1}} - \frac{l^{3-n} \times \overline{3-n} \times \overline{n-1} \times l^{n+1}}{8L^{n+1}}$ &c. which is very small in respect of l^{3-n}; as l^{n-1} is by the supposition very small in respect of L^{n-1}.

Lemma 9.—Let DC now represent the axis of a cylindric or prismatic column

of uniform matter; and let the diameter of the column be so small, that the repulsion of the plate AB on it shall not be sensibly different from what it would be, if all the matter in it was collected in the axis: the force with which the plate repels the column, is proportional to $DC^{3-n} + AC^{3-n} - DA^{3-n}$; supposing the thickness of the plate and base of the column to be given. For, if DC is supposed to flow, the corresponding fluxion of the repulsion is proportional to $\frac{D{\cdot}C}{DC^{n-2}} - \frac{DC \times D{\cdot}C}{DA^{n-1}} = \frac{D{\cdot}C}{DC^{n-1}} - \frac{D{\cdot}A}{DA^{n-2}}$; the fluent of which, $\frac{AC^{3-n} + DC^{3-n} - DA^{3-n}}{3-n}$, vanishes when DC vanishes.

Corol. 1. If the length of the column is so great, that AC^{n-1} is very small in respect of DC^{n-1}, the repulsion of the plate on it is very nearly the same as if the column was infinitely continued. For, by lemma 8, $AC^{3-n} + DC^{3-n} - DA^{3-n}$ differs very little in this case from AC^{3-n}; and if DC is infinite, it is exactly equal to it.

Corol. 2. If AC^{n-1} is very small in respect of DC^{n-1}, and the point E be taken in DC, such that EC^{n-1} shall be very small in respect of AC^{n-1}, the repulsion of the plate on the small part of the column EC, is to its repulsion on the whole column DC, very nearly as EC^{3-n} to AC^{3-n}.

Lemma 10. If we now suppose all the matter of the plate to be collected in the circumference of the circle, so as to form an infinitely slender uniform ring, its repulsion on the column DC will be less than when the matter is spread uniformly all over the plate in the ratio of $\frac{3-n \times AC^2}{2} \times (\frac{1}{AC^{n-1}} - \frac{1}{DA^{n-1}})$ to $DC^{3-n} + AC^{3-n} - DA^{3-n}$.

For it was before said, that if the matter of the plate be spread uniformly, its repulsion on the column will be proportional to $DC^{3-n} + AC^{3-n} - DA^{3-n}$, or may be expressed by it; let now AC, the semidiameter of the plate, be increased by the infinitely small quantity $A{\cdot}C$; the quantity of matter in the plate will be increased by a quantity, which is to the whole, as $2A{\cdot}C$ to AC; and the repulsion of the plate on the column will be increased by $3 - n \times A{\cdot}C \times AC^{2-n} - A{\cdot}C \times \frac{AC}{DA} \times 3 - n \times DA^{2-n} = 3 - n \times A{\cdot}C \times AC \times (\frac{1}{AC^{n-1}} - \frac{1}{DA^{n-1}})$: therefore if a quantity of matter, which is to the whole quantity in the plate, as $2A{\cdot}C$ to AC, be collected in the circumference, its repulsion on the column DC, will be to that of the whole plate, as $3 - n \times A{\cdot}C \times AC \times (\frac{1}{AC^{n-1}} - \frac{1}{DA^{n-1}})$ to $DC^{3-n} + AC^{3-n} - DA^{3-n}$; and consequently the repulsion of the plate, when all the matter is collected in its circumference, is to its repulsion when the matter is spread uniformly, as $\frac{3-n \times AC^2}{2} \times (\frac{1}{AC^{n-1}} - \frac{1}{DA^{n-1}})$ to $DC^{3-n} + AC^{3-n} - DA^{3-n}$.

Corol. 1. If the length of the column is so great, that AC^{n-1} is very small

in respect of DC^{n-1}, the repulsion of the plate when all the matter is collected in the circumference, is to its repulsion when the matter is spread uniformly, very nearly as $\frac{3-n \times AC^{3-n}}{2}$ to AC^{3-n}, or as $3 - n$ to 2.

Corol. 2. If EC^{n-1} is very small in respect of AC^{n-1}, the repulsion of the plate on the short column EC, when all the matter in the plate is collected in its circumference, is to its repulsion when the matter is spread uniformly, very nearly as $\frac{3-n \times n-1 \times EC^2}{4AC^{n-1}}$ to EC^{3-n}, or as $3 - n \times n - 1 \times EC^{n-1}$ to $4AC^{n-1}$; and is therefore very small in comparison of what it is when the matter is spread uniformly.

For, by the same kind of process as was used in lemma 8, it appears, that if EC^2 is very small in respect of AC^2, then $AC^2 \times (\frac{1}{AC^{n-1}} - \frac{1}{EA^{n-1}})$ differs very little from $\frac{n-1 \times EC^2}{2EA^{n-1}}$, or from $\frac{n-1 \times EC^2}{2AC^{n-1}}$; and if EC^{n-1} is very small in respect of AC^{n-1}, then EC^2 is a fortiori very small in respect of AC^2.

Corol. 3. Suppose now that the matter of the plate is denser near the circumference than near the middle, and that the density at and near the middle is to the mean density, or the density which it would every where be of if the matter was spread uniformly, as δ to 1, then the repulsion of the plate on EC will be less than if the matter was spread uniformly, in a ratio approaching much nearer to that of δ to 1, than to that of equality.

Corol. 4. Let every thing be as in the last corollary, and let π be taken to 1, as the force with which the plate actually repels the column DC (DC^{n-1} being very great in respect of AC^{n-1}) is to the force with which it would repel it, if the matter was spread uniformly; the repulsion of the plate on EC will be to its repulsion on DC, in a ratio between that of $EC^{3-n} \times \delta$ to $AC^{3-n} \times \pi$, and that of EC^{3-n} to $AC^{3-n} \times \pi$, but will approach much nearer to the former ratio than to the latter.

Lemma 11. In the line DC produced, take CF equal to CA: if all the matter of the plate AB is collected in the circumference, its repulsion on the column CD, infinitely continued, is equal to the repulsion of the same quantity of matter collected in the point F, on the same column. For the repulsion of the plate on the column in the direction CD, is the same whether the matter of it be collected in the whole circumference, or in the point A. Suppose it therefore to be collected in A; and let an equal quantity of matter be collected in F; take FG constantly equal to AD; and let AD and FG flow; the fluxion of CD is to the fluxion of FG, as AD to CD; and the repulsion of A on the point D, in the direction CD, is to the repulsion of F on G, as CD to AD; therefore the fluxion of the repulsion of

A on the column CD, in the direction CD, is equal to the fluxion of the repulsion of F on CG; and when AD equals AC, the repulsion of both A and F, on their respective columns, vanishes; and therefore the repulsion of A on the whole column CD, equals that of F on CG; and when CD and CG are both infinitely extended, they may be considered as the same column.

Prop. 17. Let two similar bodies, of different sizes, and consisting of different sorts of matter, be both overcharged, or both undercharged, but in different degrees; and let the redundance or deficience of fluid in each be very small, in respect of the whole quantity of fluid in them: it is impossible for the fluid to be disposed accurately in a similar manner in both of them;* as it has been shown that there will be a space, close to the surface, which will either be as full of fluid as it can hold, or will be entirely deprived of fluid; but it will be disposed as nearly in a similar manner in both, as is possible. To explain this, let BDE and *bde*, fig. 12, be the two similar bodies; and let the space comprehended between the surfaces BDE and FGH (or the space BF as he calls it for shortness) be that part of BDE, which is either as full of fluid as it can hold, or entirely deprived of it: draw the surface *fgh*, such that the space *bf*, shall be to the space BF, as the quantity of redundant or deficient fluid in *bde*, to that in BDE, and that the thickness of the space *bf* shall every where bear the same proportion to the corresponding thickness of BF: then will the space *bf* be either as full of fluid as it can hold, or entirely deprived of it; and the fluid within the space *fgh* will be disposed very nearly similarly to that in the space FGH.

For it is plain, that if the fluid could be disposed accurately in a similar manner in both bodies, the fluid would be in equilibrio in one body, if it was in the other; therefore draw the surface $\beta\delta\epsilon$ such that the thickness of the space βf, shall be every where to the corresponding thickness of BF, as the diameter of *bde* to the diameter of BDE; and let the redundant fluid or matter in *bf* be spread uniformly over the space βf; then if the fluid in the space *fgh* is disposed exactly similarly to that in FGH, it will be in equilibrio; as the fluid will then be disposed exactly similarly in the spaces $\beta\delta\epsilon$ and BDE: but as, by the supposition, the thickness of the space βf is very small in respect of the diameter of *bde*, the fluid or matter in the space *bf* will exert very nearly the same force on the rest of the fluid, whether it is spread over the space βf, or whether it is collected in *bf*.

Prop. 18.—Let two bodies, B and *b*, be connected to each other by a canal of

* By the fluid being disposed in a similar manner in both bodies, Mr. C. means that the quantity of redundant or deficient fluid, in any small part of one body, is to that in the corresponding small part of the other, as the whole quantity of redundant or deficient fluid in one body, to that in the other. By the quantity of deficient fluid in a body, he means the quantity of fluid wanting to saturate it. Notwithstanding the impropriety of this expression, he begs leave to make use of it, as it will consequently save a great deal of circumlocution.

any kind, and be either over or undercharged: it is plain that the quantity of redundant or deficient fluid in B, would bear exactly the same proportion to that in *b*, whatever sort of matter B consisted of, if it was possible for the redundant or deficient fluid in any body, to be disposed accurately in the same manner, whatever sort of matter it consisted of. For suppose B to consist of any sort of matter; and let the fluid in the canal and two bodies be in equilibrio: let now B be made to consist of some other sort of matter, which requires a different quantity of fluid to saturate it; but let the quantity and disposition of the redundant or deficient fluid in it remain the same as before: it is plain that the fluid will still be in equilibrio; as the attraction or repulsion of any body depends only on the quantity and disposition of the redundant and deficient fluid in it. Therefore by the preceding proposition, the quantity of redundant or deficient fluid in B, will actually bear very nearly the same proportion to that in *b*, whatever sort of matter B consists of; provided the quantity of redundant or deficient fluid in it is very small in respect of the whole.

Prop. 19. Let two bodies B and *b*, fig. 11, be connected together by a very slender canal AD*da*, either straight or crooked: let the canal be every where of the same breadth and thickness; so that all sections of this canal made by planes perpendicular to the direction of the canal in that part, shall be equal and similar: let the canal be composed of uniform matter; and let the electric fluid in it be supposed incompressible, and of such density as exactly to saturate the matter in it; and let it nevertheless be able to move readily along the canal; and let each particle of fluid in the canal be attracted and repelled by the matter and fluid in the canal and in the bodies B and *b*, just in the same manner that it would be if it was not incompressible;* and let the bodies B and *b* be either over or undercharged. Then the force with which the whole quantity of fluid in the canal is impelled from A towards D, in the direction of the axis of the canal, by the united attractions and repulsions of the two bodies, must be nothing; as otherwise the fluid in the canal could not be at rest: observing that by the force with which the whole quantity of fluid is impelled in the direction of the axis of the canal, he means the sum of the forces, with which the fluid in each part of the canal is impelled in the direction of the axis of the canal in that place, from A towards D; and observing also, that an impulse in the contrary direction, from D towards A, must be considered as negative.

For as the canal is exactly saturated with fluid, the fluid in it is attracted or repelled only by the redundant matter or fluid in the two bodies. Suppose now that the fluid in any section of the canal, as E*e*, is impelled with any given force

* This supposition of the fluid in the canal being incompressible, is not mentioned as a thing which can ever take place in nature, but is merely imaginary; the reason for making of which will be given hereafter.

in the direction of the canal at that place, the section Dd would, in consequence of it, be impelled with exactly the same force in the direction of the canal at D, if the fluid between Ee and Dd was not at all attracted or repelled by the two bodies; and consequently the section Dd is impelled in the direction of the canal, with the sum of the forces, with which the fluid in each part of the canal is impelled, by the attraction or repulsion of the two bodies in the direction of the axis in that part; and consequently, unless this sum was nothing, the fluid in Dd could not be at rest.

Corol. Therefore the force with which the fluid in the canal is impelled one way in the direction of the axis, by the body B, must be equal to that with which it is impelled by b in the contrary direction.

Prop. 20. Let two similar bodies B and b, fig. 13, be connected by the very slender cylindric or prismatic canal Aa, filled with incompressible fluid, in the same manner as described in the preceding proposition: let the bodies be overcharged; but let the quantity of redundant fluid in each bear so small a proportion to the whole, that the fluid may be considered as disposed in a similar manner in both; let the bodies also be similarly situated in respect of the canal Aa; and let them be placed at an infinite distance from each other, or at so great a one, that the repulsion of either body on the fluid in the canal, shall not be sensibly less than if they were at an infinite distance: then, if the electric attraction and repulsion is inversely as the n power of the distance, n being greater than 1, and less than 3, the quantity of redundant fluid in the two bodies will be to each other, as the $n - 1$ power of their corresponding diameters AF and af.

For if the quantity of redundant fluid in the two bodies is in this proportion, the repulsion of one body on the fluid in the canal, will be equal to that of the other body on it, in the contrary direction; and consequently the fluid will have no tendency to flow from one body to the other, as may thus be proved. Take the points D and E very near to each other; and take da to DA, and ea to EA, as af to AF; the repulsion of the body B on a particle at D, will be to the repulsion of b on a particle at d, as $\frac{1}{AF}$ to $\frac{1}{af}$; for, as the fluid is disposed similarly in both bodies, the quantity of fluid in any small part of B, is to the quantity in the corresponding part of b, as AF^{n-1} to af_{n-1}; and consequently the repulsion of that small part of B, on D, is to the repulsion of the corresponding part of b on d, as $\frac{AF^{n-1}}{AF^n}$, or $\frac{1}{AF}$ to $\frac{1}{af}$. But the quantity of fluid in the small part DE of the canal, is to that in de, as DE to de, or as AF to af; therefore the repulsion of B on the fluid in DE, is equal to that of b on the fluid in de: therefore, taking ag to Aa, as af to AF, the repulsion of b on the fluid in ag, is equal to that of B on the fluid in Aa; but the repulsion of b on ag may be considered as the same as

its repulsion on Aa; for, by the supposition, the repulsion of B on Aa may be considered as the same as if it was continued infinitely; and therefore the repulsion of b on ag may be considered as the same as if it was continued infinitely.

N. B. If n was not greater than 1, it would be impossible for the length of Aa to be so great, that the repulsion of B on it might be considered as the same as if it was continued infinitely; which was the reason for requiring n to be greater than 1.

Corol. By just the same method of reasoning it appears, that if the bodies are undercharged, the quantity of deficient fluid in b will be to that in B, as af^{n-1} to AF^{n-1}.

Prop. 21. Let a thin flat plate be connected to any other body, as in the preceding proposition, by a canal of incompressible fluid, perpendicular to the plane of the plate; and let that body be overcharged; then the quantity of redundant fluid in the plate will bear very nearly the same proportion to that in the other body, whatever the thickness of the plate may be, provided its thickness is very small in proportion to its breadth, or smallest diameter. For there can be no doubt, but under that restriction, the fluid will be disposed very nearly in the same manner in the plate, whatever its thickness may be; and therefore its repulsion on the fluid in the canal will be very nearly the same, whatever its thickness may be.

Prop. 22. Let AB and DF, fig. 14, represent two equal and parallel circular plates, whose centres are C and E; let the plates be placed so, that a right line joining their centres shall be perpendicular to the plates; let the thickness of the plates be very small in respect of their distance CE; let the plate AB communicate with the body H, and the plate DF with the body L, by the canals CG and EM of incompressible fluid, such as are described in prop. 19; let these canals meet their respective plates in their centres C and E, and be perpendicular to the plane of the plates; and let their length be so great, that the repulsion of the plates on the fluid in them may be considered as the same as if they were continued infinitely; let the body H be overcharged, and let L be saturated. It is plain, from prop. 12, that DF will be undercharged, and AB will be more overcharged than it would otherwise be. Suppose now, that the redundant fluid in AB is disposed in the same manner as the deficient fluid is in DF; let P be to 1, as the force with which the plate AB would repel the fluid in CE, if the canal ME was continued to C, is to the force with which it would repel the fluid in CM; and let the force with which AB repels the fluid in CG, be to the force with which it would repel it, if the redundant fluid in it was spread uniformly, as π to 1; and let the force with which the body H repels the fluid in CG, be the same with which a quantity of redundant fluid, which we will call B, spread uniformly

over AB, would repel it in the contrary direction. Then will the redundant fluid in AB be equal to $\frac{B}{2P\pi - P^2\pi}$; and therefore, if P is very small, will be very nearly equal to $\frac{B}{2P\pi}$; and the deficient fluid in DF will be to the redundant fluid in AB, as $1 - P$ to 1; and therefore, if P is very small, will be very nearly equal to the redundant fluid in AB.

For it is plain, that the force with which AB repels the fluid in EM, must be equal to that with which DF attracts it; for otherwise some fluid would run out of DF into L, or out of L into DF: for the same reason, the excess of the repulsion of AB on the fluid in CG, above the attraction of FD on it, must be equal to the force with which a quantity of redundant fluid equal to B, spread uniformly over AB, would repel it, or it must be equal to that with which a quantity equal to $\frac{B}{\pi}$, spread in the manner in which the redundant fluid is actually spread in AB, would repel it. By the supposition, the force with which AB repels the fluid in EM, is to the force with which it would repel the fluid in CM, supposing EM to be continued to C, as $1 - P$ to 1; but the force with which any quantity of fluid in AB would repel the fluid in CM, is the same with which an equal quantity similarly disposed in DF, would repel the fluid in EM; therefore the force with which the redundant fluid in AB repels the fluid in EM, is to that with which an equal quantity similarly disposed in DF, would repel it, as $1 - P$ to 1: therefore, if the redundant fluid in AB be called A, the deficient fluid in DF must be $A \times \overline{1 - P}$: for the same reason, the force with which DF attracts the fluid in CG, is to that with which AB repels it, as $A \times \overline{1 - P} \times \overline{1 - P}$, or $A \times (1 - P)^2$, to A; therefore, the excess of the force with which AB repels CG, above that with which DF attracts it, is equal to that with which a quantity of redundant fluid equal to $A - A \times (1 - P)^2$, or $A \times (2P - P^2)$, spread over AB, in the manner in which the redundant fluid in it is actually spread, would repel it: therefore, $A \times (2P - P^2)$ must be equal to $\frac{B}{\pi}$, or A must be equal to $\frac{B}{2P\pi - P^2\pi}$.

Corol. 1. If the density of the redundant fluid near the middle of the plate AB, is less than the mean density, or the density which it would every where be of, if it was spread uniformly, in the ratio of δ to 1; and if the distance of the two plates is so small, that EC^{n-1} is very small in respect of AC^{n-1}, and that EC^{3-n} is very small in respect of AC^{3-n}, the quantity of redundant fluid in AB will be greater than $\frac{B}{2} \times (\frac{AC}{EC})^{3-n}$, and less than $\frac{B}{2\delta} \times (\frac{AC}{EC})^{3-n}$, but will approach much nearer to the latter value than the former. For, in this case, $P\pi$ is, by lemma 10, corol. 4, less than $(\frac{EC}{AC})^{3-n}$, and greater than $(\frac{EC}{AC})^{3-n} \times \delta$, but approaches much nearer to the latter value than the former; and if EC^{3-n} is very small in respect of AC^{3-n}, P is very small.

Remarks. If DF was not undercharged, it is certain that AB would be considerably more overcharged near the circumference of the circle than near the centre; for if the fluid was spread uniformly, a particle placed any where at a distance from the centre, as at N, would be repelled with considerably more force towards the circumference, than it would towards the centre. If the plates are very near together, and consequently DF nearly as much undercharged as AB is overcharged, AB will still be more overcharged near the circumference than near the centre, but the difference will not be near so great as in the former case: for, let NR be many times greater than CE, and NS less than CE; and take E*r* and E*s* equal to CR and CS; there can be no doubt, he thinks, but that the deficient fluid in DF will be lodged nearly in the same manner as the redundant fluid in AB; and therefore the repulsion of the redundant fluid at R, on a particle at N, will be very nearly balanced by the attraction of the redundant matter at *r*, for R is not much nearer to N than *r* is; but the repulsion of S will not be near balanced by that of *s*; for the distance of S from N is much less than that of *s*. Let now a small circle, whose diameter is ST, be drawn round the centre N, on the plane of the plate; as the density of the fluid is greater at T than at S, the repulsion of the redundant fluid within the small circle tends to impel the point N towards C; but as there is a much greater quantity of fluid between N and B, than between N and A, the repulsion of the fluid without the small circle tends to balance that; but the effect of the fluid within the small circle is not much less than it would be, if DF was not undercharged; whereas much the greater part of the effect of that part of the plate on the outside of the circle, is taken off by the effect of the corresponding part of DF: consequently the difference of density between T and S will not be near so great, as if DF was not undercharged. Hence he imagines, that if the two plates are very nearly together, the density of the redundant fluid near the centre will not be much less than the mean density, or δ will not be much less than 1; moreover, the less the distance of the plates, the nearer will δ approach to 1.

Corol. 2. Let now the body H consist of a circular plate, of the same size as AB, placed so, that the canal CG shall pass through its centre, and be perpendicular to its plane; by the supposition, the force with which H repels the fluid in the canal CG, is the same with which a quantity of fluid, equal to B, spread uniformly over AB, would repel it in the contrary direction: therefore, if the fluid in the plate H was spread uniformly, the quantity of redundant fluid in it would be B; and if it was all collected in the circumference, would be $\frac{2B}{3-n}$; and therefore the real quantity will be greater than B, and less than $\frac{2B}{3-n}$.

Corol. 3. Therefore, if we suppose δ to be equal to 1, the quantity of redundant fluid in AB will exceed that in the plate H, in a greater ratio than that of

$(\frac{AC}{CE})^{3-n} \times \frac{3-n}{4}$ to 1, and less than that of $(\frac{AC}{CE})^{3-n} \times \frac{1}{2}$ to 1: and from the preceding remarks it appears, that the real quantity of redundant fluid in AB can hardly be much greater than it would be if δ was equal to 1.

Corol. 4. Hence, if the electric attraction and repulsion is inversely as the square of the distance, the redundant fluid in AB, supposing δ to be equal to 1, will exceed that in the plate H, in a greater ratio than that of AC to 4CE, and less than that of AC to 2CE.

Corol. 5. Let now the body H consist of a globe, whose diameter equals AB; the globe being situated in such a manner, that the canal CG, if continued, would pass through its centre; and let the electric attraction and repulsion be inversely as the square of the distance, the quantity of redundant fluid in the globe will be 2B; for the fluid will be spread uniformly over the surface of the globe, and its repulsion on the canal will be the same as if it was all collected in the centre of the sphere, and will therefore be the same with which an equal quantity, disposed in the circumference of AB, would repel it in the contrary direction, or with which half that quantity, or B, would repel it, if spread uniformly over the plate.

Corol. 6. Therefore, if δ was equal to 1, the redundant fluid in AB would exceed that in the globe, in the ratio of AC to 4CE; and therefore it will in reality exceed that in the globe, in a rather greater ratio than that of AC to 4CE; but if the plates are very near together, it will approach very near to that ratio, and the nearer the plates are, the nearer it will approach to it.

Corol. 7. Whether the electric repulsion is inversely as the square of the distance or not, if the body H is as much undercharged, as it was before overcharged, AB will be as much undercharged as it was before overcharged, and DF as much overcharged as it was before undercharged.

Corol. 8. If the size and distance of the plates be altered, the quantity of redundant or deficient fluid in the body H remaining the same, it appears, by comparing this proposition with the 20th and 21st propositions, that the quantity of redundant and deficient fluid in AB, will be as $AC^{n-1} \times (\frac{AC}{EC})^{3-n}$, or as $\frac{AC^2}{EC^{3-n}}$, supposing the value of δ to remain the same.

Prop. 23. Let AE, fig. 15, be a cylindric canal, infinitely continued beyond E; and let AF be a bent canal, meeting the other at A, and infinitely continued beyond F: let the section of this canal, in all parts of it, be equal to that of the cylindric canal, and let both canals be filled with uniform fluid of the same density: then the force with which a particle of fluid P, placed any where at pleasure, repels the whole quantity of fluid in AF, in the direction of the canal, is the same with which it repels the fluid in the canal AE, in the direction AE.— On the centre P, draw 2 circular arches BD and *bd*, infinitely near to each other,

cutting AE in B and β, and AF in D and δ; and draw the radii Pb and Pd. As PB = PD, the force with which P repels a particle at B, in the direction Bβ, is to that with which it repels an equal particle at D, in the direction Dδ, as $\frac{Bb}{B\beta}$ to $\frac{Dd}{D\delta}$, or as $\frac{1}{B\beta}$ to $\frac{1}{D\delta}$; and therefore the force with which it repels the whole fluid in Bβ, in the direction Bβ, is the same with which it repels the whole fluid in Dδ, in the direction Dδ, that is in the direction of the canal; and therefore the force with which it repels the whole fluid in AE, in the direction AE, is the same with which it repels the whole fluid in AF, in the direction of the canal.

Corol. If the bent canal ADF, instead of being infinitely continued, meets the cylindric canal in E, as in fig. 16, the repulsion of P on the fluid in the bent canal ADE, in the direction of the canal, will still be equal to its repulsion on that in the cylindric canal AE, in the direction AE.

Prop. 24. If two bodies, for instance the plate AB, and the body H, of prop. 22, communicate with each other, by a canal filled with incompressible fluid, and are either over or undercharged; the quantity of redundant fluid in them will bear the same proportion to each other, whether the canal by which they communicate is straight or crooked, or into whatever part of the bodies the canal is inserted, or in whatever manner the two bodies are situated in respect of each other; provided that their distance is infinite, or so great that the repulsion of each body on the fluid in the canal shall not be sensibly less than if it was infinite.

Let the parallelograms AB and DF, fig. 17, represent the two plates, and H and L the bodies communicating with them: let now H be removed to h; and let it communicate with AB, by the bent canal gc; the quantity of fluid in the plates and bodies remaining the same as before; and let us, for the sake of ease in the demonstration, suppose the canal gc to be every where of the same thickness as the canal GC; though the proposition will evidently hold equally good, whether it is or not: then the fluid will still be in equilibrio. For let us first suppose the canal gc to be continued through the substance of the plate AB, to C, along the line crC; the part crC being of the same thickness as the rest of the canal, and the fluid in it of the same density: by the preceding proposition, the repulsion or attraction of each particle of fluid or matter in the plates AB and DF, on the fluid in the whole canal Crcg, in the direction of that canal, is equal to its repulsion or attraction on the fluid in the canal CG, in the direction CG; and therefore the whole repulsion or attraction of the two plates on the canal Crcg, is equal to their repulsion or attraction on CG: but as the fluid in the plate AB is in equilibrio, each particle of fluid in the part Crc of the canal, is impelled by the plates, with as much force in one direction as the other; and consequently the plates impel the fluid in the canal cg, with as much force as they do that in the whole canal Crcg, that is, with the same force that they impel the fluid in

CG. In like manner the body *h* impels the fluid in *cg*, with the same force that H does the fluid in CG; and consequently *h* impels the fluid in *cg*, one way in the direction of the canal, with the same force that the two plates impel it the contrary way; and therefore the fluid in *cg* has no tendency to flow from one body to the other.

Corol. By the same method of reasoning, with the help of the corollary to the 23d proposition, it appears that if AB and H each communicate with a third body, by canals of incompressible fluid, and a communication is made between AB and H by another canal of incompressible fluid, the fluid will have no tendency to flow from one to the other through this canal; supposing that the fluid was in equilibrio before this communication was made. In like manner, if AB and H communicate with each other, or each communicate with a third body, by canals of real fluid, instead of the imaginary canals of incompressible fluid used in these propositions, and a communication is also made between them by a canal of incompressible fluid, the fluid can have no tendency to flow from one to the other. The truth of the latter part of this corollary will appear by supposing an imaginary canal of incompressible fluid to be continued through the whole length of the real one.

Prop. 25. Let now a communication be made between the two plates AB and DF, by the canal NRS of incompressible fluid, of any length; and let the body H and the plate AB be overcharged. It is plain that the fluid will flow through that canal from AB to DF. Now the whole force with which the fluid in the canal is impelled along it, by the joint action of the two plates, is the same with which the whole quantity of fluid in the canal CG or *cg* is impelled by them; supposing the canal NRS to be every where of the same breadth and thickness as CG or *cg*. For suppose that the canal NRS, instead of communicating with the plate DF, is bent back just before it touches it, and continued infinitely along the line S*s*; the force with which the two plates impel the fluid in S*s*, is the same with which they impel that in EL, supposing S*s* to be of the same breadth and thickness as EL; and is therefore nothing; therefore the force with which they impel the fluid in NRS, is the same with which they impel that in NRS*s*; which is the same with which they impel that in CG.

Prop. 26. Let now *xyz* be a body of an infinite size, containing just fluid enough to saturate it; and let a communication be made between *h* and *xyz*, by the canal *hy* of incompressible fluid, of the same breadth and thickness as *gc* or GC; the fluid will flow through it from *h* to *xyz*; and the force with which the fluid in that canal is impelled along it, is equal to that with which the fluid in NRS is impelled by the two plates.

If the canal *hy* is of so great a length, that the repulsion of *h* on it is the same as if it was continued infinitely, then the thing is evident; but if it is not,

let the canal *hy*, instead of communicating with *xyz*, so that the fluid can flow out of the canal into *xyz*, be continued infinitely through its substance, along the line *yv*: now it must be observed that a small part of the body *xyz*, namely, that which is turned towards *h*, will by the action of *h* on it, be rendered undercharged; but all the rest of the body will be saturated; for the fluid driven out of the undercharged part will not make the remainder, which is supposed to be of an infinite size, sensibly overcharged: now the force with which the fluid in the infinite canal *hyv*, is impelled by the body *h* and the undercharged part of *xyz*, is the same with which the fluid in *gc* is impelled by them; but as the fluid in all parts of *xyz* is in equilibrio, a particle in any part of *yv* cannot be impelled in any direction; and therefore the fluid in *hy* is impelled with as much force as that in *hyv*; and therefore the fluid in *hy* is impelled with as much force as that in *gc*; and is therefore impelled with as much force as the fluid in NRS is impelled by the two plates.

It perhaps may be asked, whether this method of demonstration would not equally tend to prove that the fluid in *hy* was impelled with the same force as that in NRS, though *xyz* did not contain just fluid enough to saturate it. He answers not; for this demonstration depends on the canal *yv* being continued, within the body *xyz*, to an infinite distance beyond any over or undercharged part; which could not be if *xyz* contained either more or less fluid than that.

Prop. 27. Let two bodies B and *b*, fig. 13, be joined by a cylindric or prismatic canal A*a*, filled with real fluid; and not by an imaginary canal of incompressible fluid as in the 20th proposition; and let the fluid in it be in equilibrio: the force with which the whole or any given part of the fluid in the canal, is impelled in the direction of its axis, by the united repulsions and attractions of the redundant fluid or matter in the two bodies and the canal, must be nothing; or the force with which it is impelled one way in the direction of the axis of the canal, must be equal to that with which it is impelled the other way. For as the canal is supposed cylindric or prismatic, no particle of fluid in it can be prevented from moving in the direction of its axis, by the sides of the canal; and therefore the force with which each particle is impelled either way in the direction of the axis, by the united attractions and repulsions of the two bodies and the canal, must be nothing, otherwise it could not be at rest; and therefore the force with which the whole, or any given part of the fluid in the canal, is impelled in the direction of the axis, must be nothing.

Corol. 1. If the fluid in the canal is disposed in such manner, that the repulsion or attraction of the redundant fluid or matter in it, on the whole or any given part of the fluid in the canal, has no tendency to impel it either way in the direction of the axis; then the force with which that whole or given part is impelled by the two bodies, must be nothing; or the force with which it is im-

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pelled one way in the direction of the axis, by the body B, must be equal to that with which it is impelled in the contrary direction by the other body; but not if the fluid in the canal is disposed in a different manner.

Corol. 2. If the bodies, and consequently the canal, is overcharged; then, in whatever manner the fluid in the canal is disposed, the force with which the whole quantity of redundant fluid in the canal is repelled by the body B, in the direction A*a*, must be equal to that with which it is repelled by *b*, in the contrary direction. For the force with which the redundant fluid is impelled in the direction A*a*, by its own repulsion, is nothing; for the repulsion of the particles of any body on each other, have no tendency to make the whole body move in any direction.

Remarks. When Mr. C. first thought of the 20th and 22d propositions, he imagined that when two bodies were connected by a cylindric canal of real fluid, the repulsion of one body on the whole quantity of fluid in the canal, in one direction, would be equal to that of the other body on it, in the contrary direction, in whatever manner the fluid was disposed in the canal; and that therefore those propositions would have held good very nearly, though the bodies were joined by cylindric canals of real fluid; provided the bodies were so little over or undercharged, that the quantity of redundant or deficient fluid in the canal, should be very small in respect of the quantity required to saturate it; and consequently that the fluid in it should be very nearly of the same density in all parts. But from the foregoing proposition it appears that he was mistaken, and that the repulsion of one body on the fluid in the canal, is not equal to that of the other body on it, unless the fluid in the canal is disposed in a particular manner: besides that, when two bodies are both joined by a real canal, the attraction or repulsion of the redundant matter or fluid in the canal, has some tendency to alter the disposition of the fluid in the 2 bodies; and in the 22d proposition, the canal CG exerts also some attraction or repulsion on the canal EM, on all which accounts the demonstration of those propositions is defective, when the bodies are joined by real canals. He has good reason however to think, that those propositions actually hold good, very nearly, when the bodies are joined by real canals; and that, whether the canals are straight or crooked, or in whatever direction the bodies are situated in respect of each other: though he is by no means able to prove that they do: he therefore chose still to retain those propositions, but to demonstrate them on this ideal supposition, in which they are certainly true, in hopes that some more skilful mathematician may be able to show whether they really hold good or not.

What principally makes him think that this is the case, is, that as far as he can judge from some experiments he has made, the quantity of fluid in different bodies agrees very well with those propositions, on a supposition that the electric

repulsion is inversely as the square of the distance. It should also seem from those experiments, that the quantity of redundant or deficient fluid in two bodies, bore very nearly the same proportion to each other, whatever is the shape of the canal by which they are joined, or in whatever direction they are situated in respect of each other.

Though the above propositions should be found not to hold good, when the bodies are joined by real canals, still it is evident, that in the 22d proposition, if the plates AB and DF are very near together, the quantity of redundant fluid in the plate AB will be many times greater than that in the body H, supposing H to consist of a circular plate of the same size, as AB, and DF will be nearly as much undercharged as AB is overcharged.

Sir Isaac Newton supposes that air consists of particles which repel each other with a force inversely as the distance: but it appears plainly from the foregoing pages, that if the repulsion of the particles was in this ratio and extended indefinitely to all distances, they would compose a fluid extremely different from common air. If the repulsion of the particles was inversely as the distance, but extended only to a given very small distance from their centres, they would compose a fluid of the same kind as air, in respect of elasticity, except that its density would not be in proportion to its compression: if the distance to which the repulsion extends, though very small, is yet many times greater than the distance of the particles from each other, it might be shown, that the density of the fluid would be nearly as the square root of the compression. If the repulsion of the parts extend indefinitely, and was inversely as some higher power of the distance than the cube, the density of the fluid would be as some power of the compression less than $\frac{1}{3}$. The only law of repulsion, Mr. C. can think of, which will agree with experiment, is one which seems not very likely; namely, that the particles repel each other with a force inversely as the distance; but that, whether the density of the fluid is great or small, the repulsion extends only to the nearest particles: or, what comes to the same thing, that the distance to which the repulsion extends, is very small, and also is not fixed, but varies in proportion to the distance of the particles.

PART II.—*Containing a Comparison of the Foregoing Theory with Experiment.*

§ 1. It appears from experiment, that some bodies suffer the electric fluid to pass with great readiness between their pores; while others will not suffer it to do so without great difficulty; and some hardly suffer it to do so at all. The first sort of bodies are called conductors, the others non-conductors. What this difference in bodies is owing to, Mr. C. does not pretend to explain. It is evident that the electric fluid in non-conductors may be considered as moveable, or answering to the definition given of that term immediately before prop. 1.

As to the fluid contained in non-conducting substances, though it does not absolutely answer to the definition of immoveable, as it is not absolutely confined from moving, but only does so with great difficulty; yet it may in most cases be considered as such without sensible error. Air does in some measure permit the electric fluid to pass through it; though, if it is dry, it lets it pass but very slowly, and not without difficulty; it is therefore to be called a non-conductor.

It appears that conductors would readily suffer the fluid to run in and out of them, were it not for the air which surrounds them: for if the end of a conductor is inserted into a vacuum, the fluid runs in and out of it with perfect readiness; but when it is surrounded on all sides by the air, as no fluid can run out of it without running into the air, the fluid will not do so without difficulty. If any body is surrounded on all sides by the air, or other non-conducting substances, it is said to be insulated: if on the other hand it any where communicates with any conducting body, it is said to be not insulated. When he says that a body communicates with the ground, or any other body, he would be understood to mean that it does so by some conducting substance.

Though the terms positively and negatively electrified are much used, yet the precise sense in which they are to be understood, seems not well ascertained; namely, whether they are to be understood in the same sense in which he has used the words over or undercharged, or whether, when any number of bodies, insulated and communicating with each other by conducting substances, are electrified by means of excited glass, they are all to be called positively electrified (supposing, according to the usual opinion, that excited glass contains more than its natural quantity of electricity); even though some of them, by the approach of a stronger electrified body, are made undercharged. He uses the words in the latter sense; but as it will be proper to ascertain the sense in which he uses them more accurately, he gives the following definition. In order to judge whether any body, as A, is positively or negatively electrified: suppose another body B, of a given shape and size, to be placed at an infinite distance from it, and from any other over or undercharged body; and let B contain the same quantity of electric fluid, as if it communicated with A by a canal of incompressible fluid: then if B is overcharged, he calls A positively electrified; and if it is undercharged, he calls A negatively electrified; and the greater the degree in which B is over or undercharged, the greater is the degree in which A is positively or negatively electrified.

It appears from the corol. to the 24th proposition, that if several bodies are insulated, and connected together by conducting substances, and one of these bodies is positively or negatively electrified, all the other bodies must be electrified in the same degree: for suppose a given body B to be placed at an infinite distance from any over or undercharged body, and to contain the same quantity

of fluid as if it communicated with one of those bodies by a canal of incompressible fluid; all the rest of these bodies must, by that corol., contain the same quantity of fluid as if they communicated with B by canals of incompressible fluid: but yet it is possible that some of those bodies may be overcharged, and others undercharged: for suppose the bodies to be positively electrified, and let an overcharged body D be brought near one of them, that body will become undercharged, provided D is sufficiently overcharged; and yet by the definition it will still be positively electrified in the same degree as before.

Moreover, if several bodies are insulated and connected together by conducting substances, and one of these bodies is electrified by excited glass, there can be no doubt but they will all be positively electrified; for if there is no other over or undercharged body placed near any of these bodies, the thing is evident; and though some of these bodies may, by the approach of a sufficiently overcharged body, be rendered undercharged; yet he does not see how it is possible to prevent a body placed at an infinite distance, and communicating with them by a canal of incompressible fluid, from being overcharged. In like manner if one of these bodies is electrified by excited sealing wax, they will all be negatively electrified.

It is impossible for any body communicating with the ground to be either positively or negatively electrified; for the earth, taking the whole together, contains just fluid enough to saturate it, and consists in general of conducting substances; and consequently though it is possible for small parts of the surface of the earth to be rendered over or undercharged, by the approach of electrified clouds, or other causes; yet the bulk of the earth, and especially the interior parts, must be saturated with electricity. Therefore assume any part of the earth which is itself saturated, and is at a great distance from any over or undercharged part; any body communicating with the ground, contains as much electricity as if it communicated with this part by a canal of incompressible fluid, and therefore is not at all electrified.

If any body A, insulated and saturated with electricity, is placed at a great distance from any over or undercharged body, it is plain that it cannot be electrified; but if an overcharged body is brought near it, it will be positively electrified; for supposing A to communicate with any body B, at an infinite distance, by a canal of incompressible fluid, it is plain that unless B is overcharged, the fluid in the canal could not be in equilibrio, but would run from A to B. For the same reason a body insulated and saturated with fluid, will be negatively electrified if placed near an undercharged body.

§ 2. The phenomena of the attraction and repulsion of electrified bodies seem to agree exactly with the theory; as will appear by considering the following cases. *Case* 1. Let two bodies, A and B, both conductors of electricity, and

both placed at a great distance from any other electrified bodies, be brought near each other. Let A be insulated, and contain just fluid enough to saturate it; and let B be positively electrified. They will attract each other; for as B is positively electrified, and at a great distance from any overcharged body, it will be overcharged; therefore, on approaching A and B to each other, some fluid will be driven from that part of A which is nearest to B to the farther part: but when the fluid in A was spread uniformly, the repulsion of B on the fluid in A was equal to its attraction on the matter in it; therefore when some fluid is removed from those parts where the repulsion of B is strongest, to those where it is weaker, B will repel the fluid in A with less force than it attracts the matter; and consequently the bodies will attract each other.

Case 2. If we now suppose that the fluid is at liberty to escape from out of A, if it has any disposition to do so, the quantity of fluid in it before the approach of B being still sufficient to saturate it; that is, if A is not insulated and not electrified, B being still positively electrified, they will attract with more force than before: for in this case, not only some fluid will be driven from that part of A which is nearest to B to the opposite part, but also some fluid will be driven out of A. It must be observed, that if the repulsion of B on a particle at E, fig. 19, the farthest part of A, is very small in respect of its repulsion on an equal particle placed at D, the nearest part of A, the two bodies will attract with very nearly the same force, whether A is insulated or not; but if the repulsion of B, on a particle at E, is very near as great as on one at D, they will attract with very little force if A is insulated. For instance, let a small overcharged ball be brought near one end of a long conductor not electrified; they will attract with very near the same force, whether the conductor be insulated or not; but if the conductor be overcharged, and brought near a small unelectrified ball, they will not attract with near so much force, if the ball is insulated, as if it is not.

Case 3. If we now suppose that A is negatively electrified, and not insulated, it is plain that they will attract with more force than in the last case; as A will be still more undercharged in this case than in the last.

N. B. In these 3 cases, we have not as yet taken notice of the effect which the body A will have in altering the quantity and disposition of the fluid in B; but in reality this will make the bodies attract each other with more force than they would otherwise do; for in each of these cases the body A attracts the fluid in B; which will cause some fluid to flow from the farther parts of B to the nearer, and will also cause some fluid to flow into it, if it is not insulated, and will consequently cause B to act upon A with more force than it would otherwise do.

Case 4. Let us now suppose that B is negatively electrified; and let A be insulated, and contain just fluid enough to saturate it; they will attract each other; for B will be undercharged; it will therefore attract the fluid in A, and will cause

some fluid to flow from the farthest part of A, where it is attracted with less force, to the nearest part, where it is attracted with more force; so that B will attract the fluid in A with more force than it repels the matter.

Case 5 *and* 6. If A is now supposed to be not insulated and not electrified, B being still negatively electrified; it is plain that they will attract with more force than in the last case: and if A is positively electrified, they will attract with still more force.

In these last 3 cases also, the effect which A has in altering the quantity and disposition of the fluid in B, tends to increase the force with which the two bodies attract.

Case 7. It is plain that a non-conducting body saturated with fluid, is not at all attracted or repelled by an over or undercharged body, until, by the action of the electrified body on it, it has either acquired some additional fluid from the air, or had some driven out of it, or till some fluid is driven from one part of the body to the other.

Case 8. Let us now suppose that the two bodies A and B are both positively electrified in the same degree. It is plain, that were it not for the action of one body on the other, they would both be overcharged, and would repel each other. But it may perhaps be said, that one of them as A may, by the action of the other on it, be either rendered undercharged on the whole, or at least may be rendered undercharged in that part nearest to B; and that the attraction of this undercharged part on a particle of the fluid in B, may be greater than the repulsion of the more distant overcharged part: so that on the whole the body A may attract a particle of fluid in B. If so, it must be affirmed that the body B repels the fluid in A; for otherwise, that part of A which is nearest to B could not be rendered undercharged. Therefore, to obviate this objection, let the bodies be joined by the straight canal DC of incompressible fluid (fig. 19). The body B will repel the fluid in all parts of this canal; for as A is supposed to attract the fluid in B, B will not only be more overcharged than it would otherwise be, but it will also be more overcharged in that part nearest to A, than in the opposite part. Moreover, as the near undercharged part of A is supposed to attract a particle of fluid in B, with more force than the more distant overcharged part repels it; it must, a fortiori, attract a particle in the canal with more force than the other repels it; therefore the body A must attract the fluid in the canal; and consequently some fluid must flow from B to A, which is impossible; for as A and B are both electrified in the same degree, they contain the same quantity of fluid as if they both communicated with a third body at an infinite distance, by canals of incompressible fluid; and therefore by the corol. to prop. 24, if a communication is made between them by a canal of incompressible fluid, the fluid would have no disposition to flow from one to the other.

Case 9. But if one of the bodies, as A, is positively electrified, in a less degree than B, then it is possible for the bodies to attract each other; for in this case the force with which B repels the fluid in A may be so great, as to make the body A either entirely undercharged, or at least to make the nearest part of it so much undercharged, that A shall on the whole attract a particle of fluid in B. It may be worth remarking, with regard to this case, that when two bodies, both electrified positively but unequally, attract each other, you may by removing them to a greater distance from each other, cause them to repel; for as the stronger electrified body repels the fluid in the weaker with less force when removed to a greater distance, it will not be able to drive so much fluid out of it, or from the nearer to the farther part, as when placed at a less distance.

Case 10 *and* 11. By the same reasoning it appears, that if the two bodies are both negatively electrified in the same degree, they must repel each other: but if they are both negatively electrified in different degrees, it is possible for them to attract each other.

All these cases are exactly conformable to experiment.

Case 12. Let two cork balls be suspended by conducting threads, from the same positively electrified body, in such manner, that if they did not repel, they would hang close together: they will both be equally electrified, and will repel each other: let now an overcharged body, more strongly electrified than them, be brought under them; they will become less overcharged, and will separate less than before: on bringing the body still nearer, they will become not at all overcharged, and will not separate at all: and on bringing the body still nearer, they will become undercharged, and will separate again.

Case 13. Let all the air of a room be overcharged; and let two cork balls be suspended close to each other by conducting threads communicating with the wall. By prop. 15, it is highly probable that the balls will be undercharged; and therefore they should repel each other.

These last two cases are experiments of Mr. Canton's, and are described in Phil. Trans., 1753, p. 350, where are other experiments of the same kind, all readily explicable by the foregoing theory.

I have now, says Mr. C., considered all the principal or fundamental cases of electric attractions and repulsions which I can think of; all of which appear to agree perfectly with the theory.

§ 3. On the cases in which bodies receive electricity from or part with it to the air.

Lemma 1. Let the body A, fig. 6, either stand near some over or undercharged body, or at a distance from any. It seems highly probable, that if any part of its surface, as MN, is overcharged, the fluid will endeavour to run out through that part, provided the air adjacent to it is not overcharged.

For let G be any point in that surface, and P a point within the body, extremely near to it; it is plain that a particle of fluid at P, must be repelled with as much force in one direction as another (otherwise it could not be at rest) unless all the fluid between P and G is pressed close together, in which case it may be repelled with more force towards G, than it is in the contrary direction: now a particle at G is repelled in the direction PG, i. e. from P to G, by all the redundant fluid between P and G; and a particle at P is repelled by the same fluid in the contrary direction; so that as the particle at P is repelled with not less force in the direction PG than in the contrary, Mr. C. does not see how a particle at G can help being repelled with more force in that direction than the contrary, unless the air on the outside of the surface MN was more overcharged than the space between P and G.

In like manner, if any part of the surface is undercharged, the fluid will have a tendency to run in at that part from the air. The truth of this is somewhat confirmed by the 3d problem; as in all the cases of that problem, the fluid was shown to have a tendency to run out of the spaces AD and EH, at any surface which was overcharged, and to run in at any which was undercharged.

Corol. 1. If any body at a distance from other over or undercharged bodies, be positively electrified, the fluid will gradually run out of it from all parts of its surface into the adjoining air; as it is plain that all parts of the surface of that body will be overcharged: and if the body is negatively electrified, the fluid will gradually run into it at all parts of its surface from the adjoining air.

Corol. 2. Let the body A, fig. 6, insulated, and containing just fluid enough to saturate it, be brought near the overcharged body B; that part of the surface of A which is turned towards B will, by prop. 2, be rendered undercharged, and will therefore imbibe electricity from the air; and at the opposite surface RS, the fluid will run out of the body into the air.

Corol. 3. If we now suppose that A is not insulated, but communicates with the ground, and consequently that it contained just fluid enough to saturate it before the approach of B, it is plain that the surface MN will be more undercharged than before; and therefore the fluid will run in there with more force than before; but it can hardly have any disposition to run out at the opposite surface RS; for if the canal by which A communicates with the ground is placed opposite to B, as in fig. 5, then the fluid will run out through that canal, till it has no longer any tendency to run out at RS; and by the remarks at the end of prop. 27, it seems probable that the fluid in A will be nearly in the same quantity, and disposed nearly in the same manner, into whatever part of A the canal is inserted, by which it communicates with the ground.

Corol. 4. If B is undercharged, the case will be reversed; that is, it will run out where it before ran in, and will ran in where it before ran out.

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As far as I can judge, these corollaries seem conformable to experiment: thus far is certain, that bodies at a distance from other electrified bodies receive electricity from the air, if negatively electrified, and part with some to it if positively electrified: and a body not electrified, and not insulated, receives electricity from the air if brought near an overcharged body, and loses some when brought near an undercharged body: and a body insulated and containing its natural quantity of fluid, in some cases receives, and in others loses electricity, when brought near an over or undercharged body.

§ 4. The well-known effects of points in causing a quick discharge of electricity seem to agree very well with this theory.

It appears from the 20th proposition, that if two similar bodies of different sizes are placed at a very great distance from each other, and connected by a slender canal, and overcharged, the force with which a particle of fluid, placed close to corresponding parts of their surface, is repelled from them, is inversely as the corresponding diameters of the bodies. If the distance of the two bodies is small, there is not so much difference in the force with which the particle is repelled by the two bodies; but still, if the diameters of the two bodies are very different, the particle will be repelled with much more force from the smaller body than from the larger. It is true indeed, that a particle placed at a certain distance from the smaller body, will be repelled with less force than if it be placed at the same distance from the greater body; but this distance is, he believes, in most cases pretty considerable; if the bodies are spherical, and the repulsion inversely as the square of the distance, a particle placed at any distance from the surface of the smaller body, less than a mean proportional between the radii of the two bodies, will be repelled from it with more force, than if it be placed at the same distance from the larger body.

Mr. C. thinks therefore, that we may be well assured, that if two similar bodies are connected together by a slender canal, and are overcharged, the fluid must escape faster from a smaller body than from an equal surface of the larger; but as the surface of the larger body is greatest, he does not know which body ought to lose most electricity in the same time; and indeed it seems impossible to determine positively from this theory which should, as it depends in great measure on the manner in which the air opposes the entrance of the electric fluid into it. Perhaps in some degrees of electrification the smaller body may lose most, and in others the larger.

Let now ACB, fig. 18, be a conical point, standing on any body DAB, C being the vertex of the cone; and let DAB be overcharged: Mr. C. imagines that a particle of fluid placed close to the surface of the cone, any where between *b* and C, must be repelled with at least as much, if not more force, than it would, if the part A*ab*B of the cone was taken away, and the part *acb* connected to DAB by

a slender canal; and consequently, from what has been said before, it seems reasonable to suppose that the waste of electricity from the end of the cone must be very great in proportion to its surface; though it does not appear from this reasoning, whether the waste of electricity from the whole cone, should be greater or less than from a cylinder of the same base and altitude. All that has been here said relating to the flowing out of electricity from overcharged bodies, holds equally true with regard to the flowing in of electricity into undercharged bodies.

But a circumstance which, he believes, contributes as much as any thing to the quick discharge of electricity from points, is the swift current of air caused by them, and taken notice of by Mr. Wilson and Dr. Priestley (vide Priestley, p. 117 and 591); and which is produced in this manner. If a globular body ABD is overcharged, the air close to it, all round its surface, is rendered overcharged, by the electric fluid, which flows into it from the body; it will therefore be repelled by the body; but as the air all round the body is repelled with the same force, it is in equilibrio, and has no tendency to fly off from it. If now the conical point ACB be made to stand out from the globe, as the fluid will escape much faster in proportion to the surface from the end of the point, than from the rest of the body, the air close to it will be much more overcharged than that close to the rest of the body; it will therefore be repelled with much more force; and consequently a current of air will flow along the sides of the cone, from B towards C; by which means there is a continual supply of fresh air, not much overcharged, brought in contact with the point; whereas otherwise the air adjoining to it would be so much overcharged, that the electricity would have but little disposition to flow from the point into it.

The same current of air is produced in a less degree, without the help of the point, if the body, instead of being globular, is oblong or flat, or has knobs on it, or is otherwise formed in such manner as to make the electricity escape faster from some parts of it than the rest.

In like manner, if the body ABD be undercharged, the air adjoining to it will also be undercharged, and will therefore be repelled by it; but as the air close to the end of the point will be more undercharged than that close to the rest of the body, it will be repelled with much more force; which will cause exactly the same current of air, flowing the same way, as if the body was overcharged; and consequently the velocity with which the electric fluid flows into the body, will be very much increased. Mr. C. believes indeed that it may be laid down as a constant rule, that the faster the electric fluid escapes from any body when overcharged, the faster will it run into that body when undercharged.

Points are not the only bodies which cause a quick discharge of electricity; in particular, it escapes very fast from the ends of long slender cylinders; and a

swift current of air is caused to flow from the middle of the cylinder towards the end: this will easily appear by considering, that the redundant fluid is collected in much greater quantity near the ends of the cylinders, than near the middle. The same thing may be said, but he believes in a less degree, of the edges of thin plates.

What has been just said concerning the current of air, serves to explain the reason of the revolving motion of Dr. Hamilton's and Mr. Kinnersley's bent pointed wires, vide Phil. Trans. vol. 51, p. 905, and vol. 53, p. 86; also Priestley, p. 429: for the same repulsion which impels the air from the thick part of the wire towards the point, tends to impel the wire in the contrary direction.

It is well known, that if a body B is positively electrified, and another body A, communicating with the ground, be then brought near it, the electric fluid will escape faster from B, at that part of it which is turned towards A, than before. This is plainly conformable to theory; for as A is thus rendered undercharged, B will in its turn be made more overcharged, in that part of it which is turned towards A, than it was before. But it is also well known that the fluid will escape faster from B, if A be pointed, than if it be blunt; though B will be less overcharged in this case than in the other; for the broader the surface of A, which is turned towards B, the more effect will it have in increasing the overcharge of B. The cause of this phenomenon is as follows:

If A is pointed, and the pointed end turned towards B, the air close to the point will be very much undercharged, and therefore will be strongly repelled by A, and attracted by B, which will cause a swift current of air to flow from it towards B, by which means a constant supply of undercharged air will be brought in contact with B, which will accelerate the discharge of electricity from it in a very great degree: and moreover, the more pointed A is, the swifter will be this current. If, on the other hand, that end of A which is turned towards B, is so blunt, that the electricity is not disposed to run into A faster than it is to run out of B, the air adjoining to B may be as much overcharged as that adjoining to A is undercharged; and therefore may, by the joint repulsion of B and attraction of A, be impelled from B to A, with as much or more force than the air adjoining to A is impelled in the contrary direction; so that what little current of air there is may flow in the contrary direction.

It is easy applying what has been here said to the case in which B is negatively electrified.

§ 5. In the paper of Mr. Canton's, quoted in the 2d section, and in a paper of Dr. Franklin's (Phil. Trans., 1755, p. 300, and Franklin's letters, p. 155) are some remarkable experiments, showing that when an overcharged body is brought near another body, some fluid is driven to the farther end of this body, and also some driven out of it, if it is not insulated. The experiments are all

strictly conformable to the 11th, 12th, and 13th propositions: but it is needless to point out the agreement, as the explanation given by the authors does it sufficiently.

§ 6. *On the Leyden Vial.*—The shock produced by the Leyden vial, seems owing only to the great quantity of redundant fluid collected on its positive side, and the great deficiency on its negative side; so that if a conductor was prepared of so great a size, as to be able to receive as much additional fluid by the same degree of electrification, as the positive side of a Leyden vial, and was positively electrified in the same degree as the vial, he does not doubt but what as great a shock would be produced by making a communication between this conductor and the ground, as between the two surfaces of the Leyden vial, supposing both communications to be made by canals of the same length and same kind.

It appears plainly from the experiments which have been made on this subject, that the electric fluid is not able to pass through the glass; but yet it seems as if it was able to penetrate without much difficulty to a certain small depth, perhaps he might say an imperceptible depth, within the glass; as Dr. Franklin's analysis of the Leyden vial shows that its electricity is contained chiefly in the glass itself, and that the coating is not greatly over or undercharged.

It is well known that glass is not the only substance which can be charged in the manner of the Leyden vial; but that the same effect may be produced by any other body, which will not suffer the electricity to pass through it.

* Hence the phenomena of the vial seem easily explicable by means of the 22d proposition. For let ACGM, fig. 20, represent a flat plate of glass, or any other substance which will not suffer the electric fluid to pass through it, seen edgewise; and let B*bd*D, and E*ef*F, or B*d* and E*f*, as he calls them for shortness, be two plates of conducting matter of the same size, placed in contact with the glass opposite to each other; and let B*d* be positively electrified; and let E*f* communicate with the ground; and let the fluid be supposed either able to enter a little way into the glass, but not to pass through it, or unable to enter it at all; and if it is able to enter a little way into it, let $b\beta\delta d$, or $b\delta$, as he calls it, represent that part of the glass into which the fluid can enter from the plate B*d*, and $e\varphi$ that which the fluid from E*f* can enter. By the abovementioned proposition, if *be*, the thickness of the glass, is very small in respect of *bd*, the diameter of the plates, the quantity of redundant fluid forced into the space B*d*, or Bδ, that is, into the plate B*d*, if the fluid is unable to penetrate at all into the glass, or into the plate B*d*, and the space $b\delta$ together, if the fluid is able to penetrate into

* The following explication is strictly applicable only to that sort of Leyden vial, which consists of a flat plate of glass or other matter. It is evident however, that the result must be nearly of the same kind, though the glass is made into the shape of a bottle as usual, or into any other form; but he proposes to consider those sort of Leyden vials more particularly in a future paper.—Orig.

the glass, will be many times greater than what would be forced into it by the same degree of electrification if it had been placed by itself; and the quantity of fluid driven out of Eφ, will be nearly equal to the redundant fluid in Bδ.

If a communication be now made between Bδ and Eφ, by the canal NRS, the redundant fluid will run from Bδ to Eφ; and if in its way it passes through the body of any animal, it will, by the rapidity of its motion, produce in it that sensation called a shock.

It appears from the 26th proposition, that if a body of any size was electrified in the same degree as the plate Bd, and a communication was made between that body and the ground, by a canal of the same length, breadth, and thickness, as NRS; that then the fluid in that canal would be impelled with the same force as that in NRS, supposing the fluid in both canals to be incompressible; and consequently, as the quantity of fluid to be moved, and the resistance to its motion, are the same in both canals, the fluid should move with the same rapidity in both: and he sees no reason to think that the case will be different, if the communication is made by canals of real fluid.

Therefore what was said in the beginning of this section, namely, that as great a shock would be produced by making a communication between the conductor and the ground, as between the two sides of the Leyden vial, by canals of the same length and same kind, seems a necessary consequence of this theory; as the quantity of fluid which passes through the canal is, by the supposition, the same in both; and there is the greatest reason to think, that the rapidity with which it passes will be nearly, if not quite the same, in both. Mr. C. hopes soon to be able to say whether this agrees with experiment as well as theory.

It may be worth observing, that the longer the canal NRS is, by which the communication is made, the less will be the rapidity with which the fluid moves along it; for the longer the canal is, the greater is the resistance to the motion of the fluid in it; whereas the force with which the whole quantity of fluid in it is impelled, is the same, whatever be the length of the canal. Accordingly, it is found in melting small wires, by directing a shock through them, that the longer the wire the greater charge it requires to melt it.

As the fluid in Bδ is attracted with great force by the redundant matter in Eφ, it is plain that if the fluid is able to penetrate at all into the glass, great part of the redundant fluid will be lodged in $b\delta$; and in like manner there will be a great deficience of fluid in $e\varphi$. But in order to form some estimate of the proportion of the redundant fluid, which will be lodged in $b\delta$, let the communication between Ef and the ground be taken away, as well as that by which Bd is electrified; and let so much fluid be taken from Bδ, as to make the redundant fluid in it equal to the deficient fluid in Eφ. If we suppose that all the redundant fluid is

collected in $b\delta$, and all the deficient in $e\varphi$, so as to leave Bd and Ef saturated; then, if the electric repulsion is inversely as the square of the distance, a particle of fluid placed any where in the plane bd, except near the extremities b and d, will be attracted with very near as much force by the redundant matter in $e\varphi$, as it is repelled by the redundant fluid in $b\delta$; but if the repulsion is inversely as some higher power than the square, it will be repelled with much more force by $b\delta$, than it is attracted by $e\varphi$, provided the depth $b\beta$ is very small in respect of the thickness of the glass; and if the repulsion is inversely as some lower power than the square, it will be attracted with much more force by $e\varphi$, than it is repelled by $b\delta$. Hence it follows, that if the depth to which the fluid can penetrate, is very small in respect of the thickness of the glass, but yet is such that the quantity of fluid naturally contained in $b\delta$, or $e\varphi$, is considerably more than the redundant fluid in Bδ; then, if the repulsion is inversely as the square of the distance, almost all the redundant fluid will be collected in $b\delta$, leaving the plate Bd not very much overcharged; and in like manner Ef will be not very much undercharged: if the repulsion is inversely as some higher power than the square, Bd will be very much overcharged, and Ef very much undercharged; and if the repulsion is inversely as some lower power than the square, Bd will be very much undercharged, and Ef very much overcharged.

Suppose now the plate Bd to be separated from the plate of glass, still keeping it parallel to it, and opposite to the same part of it that it before was applied to; and let the repulsion of the particles be inversely as some higher power of the distance than the square. When the plate is in contact with the glass, the repulsion of the redundant fluid in that plate, on a particle in the plane bd, *id est*, the inner surface of the plate, must be equal to the excess of the repulsion of the redundant fluid in $b\delta$ on it, above the attraction of Eφ on it; therefore, when the plate Bd is removed ever so small a distance from the glass, the repulsion of the redundant fluid in the plate, on a particle in the inner surface of that plate, will be greater than the excess of the repulsion of $b\delta$ on it, above the attraction of Eφ; for the repulsion of $b\delta$ will be much more diminished by the removal, than the attraction of Eφ: consequently some fluid will fly from the plate to the glass, in the form of sparks: so that the plate will not be so much overcharged when removed from the glass, as it was when in contact with it. Mr. C. imagines however, that it would still be considerably overcharged.

If one part of the plate is separated from the glass before the rest, as must necessarily be the case if it consists of bending materials, he guesses it would be at least as much, if not more, overcharged, when separated, as if it is separated all at once. In like manner, it should seem that the plate Ef will be considerably undercharged, when separated from the glass, but not so much so as when in contact with it. From the same kind of reasoning he concludes, that if the

repulsion is inversely as some lower power of the distance than the square, the plate B*d* will be considerably undercharged, and E*f* considerably overcharged, when separated from the glass, but not in so great a degree as when they are in contact with it.

§ 7. There is an experiment of Mr. Wilke and Æpinus, related by Dr. Priestley, p. 258, called by them, electrifying a plate of air: it consisted in placing two large boards of wood, covered with tin plates, parallel to each other, and at some inches asunder. If a communication was made between one of these and the ground, and the other was positively electrified, the former was undercharged; the boards strongly attracted each other; and, on making a communication between them, a shock was felt like that of the Leyden vial.

Mr. C. is uncertain whether, in this experiment, the air contained between the two boards is very much overcharged on one side, and very much undercharged on the other, as is the case with the plate of glass in the Leyden vial; or whether the case is, that the redundant or deficient fluid is lodged only in the two boards, and that the air between them serves only to prevent the electricity from running from one board to the other; but whichever of these is the case, the experiment is equally conformable to the theory.

It must be observed, that a particle of fluid, placed between the two plates, is drawn towards the undercharged plate, with a force exceeding that with which it would be repelled from the overcharged plate, if it was electrified with the same force, the other plate being taken away, nearly in the ratio of twice the quantity of redundant fluid actually contained in the plate, to that which it would contain if electrified with the same force by itself; so that, unless the plate is very weakly electrified, or their distance is very considerable, the fluid will be apt to fly from one to the other, in the form of sparks.

§ 8. Whenever any conducting body, as A, communicating with the ground, is brought sufficiently near an overcharged body B, the electric fluid is apt to fly through the air from B to A, in the form of a spark: the way by which this is brought about seems to be this. The fluid placed any where between the two bodies, is repelled from B towards A, and will consequently move slowly through the air from one to the other: now it seems as if this motion increased the elasticity of the air, and made it rarer: this will enable the fluid to flow in a swifter current, which will still further increase the elasticity of the air, till at last it is so much rarefied, as to form very little opposition to the motion of the electric fluid, on which it flies in an uninterrupted mass from one body to the other.

In the same manner may the electric fluid pass from one body to another, in the form of a spark, if the first body communicates with the ground, and the other body is negatively electrified, or in any other case in which one body is

strongly disposed to part with its electricity to the air, and the other is strongly disposed to receive it.

In like manner, when the electric fluid is made to pass through water, in the form of a spark, as in Signor Beccaria's* and Mr. Lane's† experiments, Mr. C. imagines that the water, by the rapid motion of the electric fluid through it, is turned into an elastic fluid, and so much rarefied as to make very little opposition to its motion; and when stones are burst or thrown out from buildings struck by lightning, in all probability that effect is caused by the moisture in the stone, or some of the stone itself, being turned into an elastic fluid.

It appears plainly, from the sudden rising of the water in Mr. Kinnersley's electrical air thermometer,‡ that when the electric fluid passes through the air, in the form of a spark, the air in its passage is either very much rarefied, or entirely displaced: and the bursting of the glass vessels, in Beccaria's and Lane's experiments, shows that the same thing happens with regard to the water, when the electric fluid passes through it in the form of a spark. Now, Mr. C. saw no means by which the displacing of the air or water can be brought about, but by supposing its elasticity to be increased, by the motion of the electric fluid through it, unless you suppose it to be actually pushed aside, by the force with which the electric fluid endeavours to issue from the overcharged body: but he can by no means think that the force with which the fluid endeavours to issue, in the ordinary cases in which electric sparks are produced, is sufficient to overcome the pressure of the atmosphere, much less that it is sufficient to burst the glass vessels in Beccaria's and Lane's experiments.

The truth of this is confirmed by prop. 16. For, let an undercharged body be brought near to, and opposite to the end of a long cylindrical body, communicating with the ground; by that proposition the pressure of the electric fluid against the base of the cylinder, is scarcely greater than the force with which the two bodies attract each other, provided that no part of the cylinder is undercharged; which is very unlikely to be the case, if the electric repulsion is inversely as the square of the distance, as he has great reason to believe it is; and consequently, if the spark was produced by the air being pushed aside, by the force with which the fluid endeavours to issue from the cylinder, no sparks should be produced, unless the electricity was so strong, that the force with which the bodies attracted each other was as great as the pressure of the atmosphere against the base of the cylinder; whereas it is well known, that a spark may be produced, when the force, with which the bodies attract, is very trifling in respect of that.

* Elettricismo artificiale e naturale, p. 110. Priestley, p. 209.

† Phil. Trans. 1767, p. 451.

‡ Phil. Trans. 1763, p. 84. Priestley, p. 216.

We may frequently observe, in discharging a Leyden vial, that if the two knobs are approached together very slowly, a hissing noise will be perceived before the spark; which shows, that the fluid begins to flow from one knob to the other, before it passes in the form of a spark; and therefore serves to confirm the truth of the opinion, that the spark is brought about in the gradual manner here described.

END OF THE SIXTY-FIRST VOLUME OF THE ORIGINAL.

I. Technical Description of an Uncommon Bird from Malacca. By James Badenach, M.D. Vol. LXII, Anno 1772. p. 1.

This uncommon species of bird,* Dr. B. met with at Malacca in August 1770. The male, female, and two young ones, were purchased at that place from the natives, but died soon afterwards on board, in the passage from that port to China. The character and history of this bird, as they then occurred to him, are as follow.

Male. (Pl. 7, fig. 1), size of a common partridge, body greenish above, blackish beneath; larger wing-feathers grey, tail short and rounded, with black tip; front bare, with a red crest rising from the hind head, and consisting of about 15 downy feathers of about 1½ inch in length, suberect and divaricated; bill convex, short; upper mandible black, arching over the lower, with a red wax-like margin; nostrils oblong; orbits red, eyes purple; at the base of the bill are some whitish vibrifæ or whiskers; thighs half naked, legs long, slender, and red; feet 4 toed, divided, flesh-coloured, and somewhat knotty; hind toe thicker and shorter than the rest, and truncated. *Female* rather less than the male, without crest, and with the larger wing-feathers and wing-coverts red-ferruginous. *Young* downy, black, delighting in water. *Voice* of both male and female a strong and frequent sibilus. *Nest* among grass and reeds. *Food* rice, or bread sopped in water.

II. Investigation of the Specific Characters which Distinguish the Rabbit from the Hare. By the Hon. Daines Barrington, V. P. R. S. p. 4.

Ray makes the distinction between the hare and the rabbit to consist in the smaller size of the latter, its property of burrowing, and the greater whiteness of the flesh when dressed: he chiefly relies however on the one being larger than the other; as this is the most material circumstance in which they are

* This bird is the *Columba cristata* or *Lesser Crowned Pigeon* of Latham. It seems however to belong to the Partridge tribe, and is described in the Naturalist's Miscellany under the name of *Tetrao Porphyrio* or *Violaceous Partridge.*

supposed by him to vary, whether exterior or interior. A hare however does not exceed a rabbit so much in bulk as a Patagonian does a Laplander, or a mastiff does a lap dog, which yet are not to be considered as differing in species. Besides this, age, climate, and food, as well as other circumstances, often occasion great distinction between animals of the same species, in point of bulk. The hare, for example, which is found in most parts of North America, is a third less than the European hare, and consequently is scarcely larger than our rabbit.

The next criterion which Ray fixes on to disinguish the rabbit from the hare, is that the latter burrows in the ground: this however only holds with regard to the warren rabbit; for those called hedge rabbits seldom burrow, and many of them sit in forms as hares do. The 3d and last is, that the flesh of the rabbit is more white when dressed; which distinction is always to be found between the European hare and rabbit, but it does not often happen that one can dress the flesh of an animal which comes from another part of the globe; it is therefore a criterion we can seldom have recourse to.

Linnæus, thus describes the rabbit in his Fauna Suecica. (Art. Lepus).

Lepus Cuniculus, cauda abbreviata, auriculis nudatis.

Lepus cauda brevissima, pupillis rubris.

With regard to the first circumstance of the cauda abbreviata, he equally applies it to the hare in his Systema Naturæ, published in 1766, and drops the cauda brevissima of the Fauna Suecica; where in propriety the rabbit should not have found a place, as it is not indigenous in Sweden, the climate being too cold for it. Linnæus therefore could only have described from a tame rabbit, which probably had balder ears by some accident than common, as his next criterion is auriculis nudatis. On examining a great number of rabbits, Mr. B. does not find that their ears are balder than those of a hare; this 2d circumstance therefore establishes no specific difference. From the 3d and last particular which this great naturalist relies on, Mr. B. also is convinced that the specimen before him was not only a tame rabbit, but that its fur was either white or carroty, because rabbits of these colours only have red pupils.*

Accordingly, Linnæus has omitted the pupillis rubris, as applied to the rabbit, in the 12th edition of his Systema Naturæ; but adds another distinction, which will be found equally to fail. He there says, that the ears of a rabbit are shorter than the head; whereas those of a hare are longer: which is a just observation, when the warren rabbit is examined; but the tame rabbit, and particularly those which are white or carroty, have ears that are considerably longer than their head,

* I have examined a great number of rabbits thus coloured, which commonly have red pupils, though I have seen some with black: the grey rabbit however never has eyes of a red colour. When the white rabbits are very young, their eyes are often like a ferret's; but when they are grown to their full size, the pupils are generally quite red.—Orig.

This circumstance therefore establishes no more a specific difference between the rabbit and the hare, than the greater length of the ears of a dog would, which in some varieties of that animal are known to be excessively long.

Mons. de Buffon, in his description of the hare and rabbit, agrees with Ray, that there is nothing either exterior or interior which seems to constitute a specific difference, though he endeavours to establish an incontestible proof that they are really distinct. He informs us, that he had tried to procure a breed between rabbits and hares, but never could succeed in the experiment. This most ingenious and able writer does not state, however, at what ages the hares or rabbits were thus confined, which is known be a most material circumstance, by those who have raised male canary birds.*

In the 5th vol. of his Natural History, p. 210, Mons. de Buffon gives an account of his making the same sort of experiment between the wolf and a dog, in the following words: "J'ai fait élever une louve prise dans les bois, de deux ou trois mois." In this passage, the word is applied to a wolf, of 3 months old, and to show that Mons. de Buffon did not think the age at which the animal is confined to be material in such an experiment, he immediately afterwards states, that he caught some foxes in snares (which were probably therefore full grown), and kept them a considerable time with dogs of different sexes. After this, he says, it is evident from these experiments, that wolves, foxes, and dogs are specifically different, without distinguishing between the foxes being full grown when caught, and the wolf which was only 3 months old. But the decisive argument against Mons. de Buffon's experiment not being satisfactory, is to be found in Mr. Pennant's Synopsis of Quadrupeds, p. 144: where he informs us, that a breed was actually procured between a dog and a wolf at Mr. Brooks's, animal merchant, in Holborn.

M. de Buffon also supposes that the rabbit is much more sagacious than the hare, because, both having equal powers of burrowing, the one thus secures himself from most enemies, while the other, by not taking the same precaution, continues liable to their attacks. There are, however, several causes for the rabbit's burrowing, and the hare's neglecting to do so. In the first place, the fore-legs of a rabbit are shorter in proportion to its hind legs, and at the same time much stronger; the claws are also longer and sharper, resembling much those of a mole. It was before observed that the rabbits, which the sportsmen call hedge rabbits, seldom burrow; and they neglect taking this trouble, for the same reason that induces the hare to trust to her form, because they have an

* Birds which differ specifically scarcely ever breed except both are taken early from the nest, and particularly the hen; I have procured a breed from two robins in a cage the present year by attending to this circumstance, and I believe I could equally succeed with almost any other kind of birds, as, when they are thus reared, they have not the least awe of man.—Orig.

opportunity of selecting a proper place for their concealment. The ground, however, in a warren, is eaten so very bare by rabbits, that it is impossible for them to hide themselves if they make a form in any part of it, and they therefore very judiciously choose to burrow under ground.

Another reason, perhaps, for the rabbit's burrowing arises from the animal's being not only born, but continuing the first six weeks of its life, under ground; they therefore only practise what they have seen and learned in their earliest infancy, as birds from the same circumstance always build their nest in the same form, and with the same materials. Mr. B. therefore cannot allow entirely of the distinction arising from the superior sagacity of the rabbit, because it burrows; and Mons. de Buffon himself informs us, that tame rabbits turned into a warren do not burrow for many generations.

Having thus endeavoured to show that no proper criteria have hitherto been fixed on to distinguish the rabbit from the hare, Mr. B. suggests the two following, which he flatters himself, will be found less liable to the same exceptions. If the hind legs of a European hare are measured from the uppermost joint to the toe, the number of inches will turn out to be just half of the length of the back, from the rump to the mouth; the tail not being included. The hind legs of the rabbit being measured in the same manner, and compared with the back, are not much more than one-third; from which it seems not unfair to consider any animal of the hare genus, whose legs thus measured are less than the half of the distance from the rump to the mouth, as a rabbit; and on the contrary when they are either one half, or more, as a hare. If the fore and hind legs of a rabbit and hare be also respectively compared, it will be found that the fore legs of the former are proportionally shorter than those of a hare.

By both these criteria the quadruped from Hudson's Bay, which gave occasion to this paper, must rather be considered as a hare, than a rabbit, as it is called in that part of the world, according to the admeasurements subjoined, which include the respective proportions also of the Alpine hare.*

	Fore Leg. Inches.	Hind Leg. Inches.	Back and Head. Inches.
Rabbit	$4\frac{1}{2}$	$6\frac{3}{4}$	$16\frac{1}{2}$
Hare	$7\frac{3}{4}$	11	22
Hudson's Bay Quadruped	$6\frac{3}{8}$	$10\frac{6}{8}$	18
Alpine Hare	$6\frac{4}{8}$ From the uppermost joint to the toe.	$10\frac{6}{8}$ From the uppermost joint to the toe.	22

From the proportion of these parts, in the Hudson's Bay quadruped, according

* This species of hare is found in the highlands of Scotland, whence Mr. B. received a specimen, which he presented to the Museum of the Royal Society.—Orig.

to this table, Mr. B. thinks that it may with greater propriety be classed as belonging to the hare species, than by any other marks of a specific difference which has been hitherto relied on.

III. On the Sulphureous Mineral Waters of Castle-Loed and Fairburn, in Rosshire; and of the Salt Purging Water of Pitkeathly, in Perthshire, Scotland. By Donald Monro, M. D., F. R. S. p. 15.

The following account of the Castle-Loed mineral water is contained in a letter from Dr. Mackenzy: "The Castle-Loed is a strong sulphureous mineral water; when taken up from the spring, it is as pure and transparent as the clearest rock water; but if kept in an open vessel, or an ill-corked bottle, it soon becomes of a milky sort of foulness, and it loses its strong sulphureous smell in 24 hours.

"The bottom of the well, and of the channel which conveys its water from thence, is black, as if dyed with ink; and the leaves of the alder bushes that fall into the well, or into its channel, soon contract a blackish colour in the water; but when taken out, and dried in the sun or shade, appear covered with a whitish dust, which is undoubtedly sulphur; for, by burning one or more on an ignited shovel, or clear live coal, they produce a blue flame, and emit a very suffocating sulphureous smell.

"All that I can learn of the operation of this water, from some sensible people of credit and observation, who have drank it, this as well as former seasons, is, that it very sensibly increases the urine, and sometimes remarkably opens the pores; but I do not find, from the report of any, that it purges, though drunk to the quantity of 3, sometimes of 4, English quarts in the day. Almost every person remarks, that it whets the appetite, and sits light on the stomach. I have been told by several, that they have had head-achs immediately after drinking their morning bottle, but of no long duration, nor to any great degree.

"It is impossible to say with certainty the number of cures these waters have made, or what particular cases have received most benefit from using them; for every person in the county prescribes water for himself, and runs to the well, or sends for the water, for every complaint, acute and chronic. I have indeed myself directed several people with various complaints to drink them. Some very foul faces have been quite cleared; and, at this time, a gentleman's son, 9 years of age, with a herpes round the neck, which had proved extremely obstinate to other means, has got a perfect cure by drinking and washing with them; and his sister, a young lady of 18, who, from an untoward recovery from the meazles and small-pox, fell into a sort of habitual erysipelas on the face, head, breast, and arms, is now using them, and I think with evident advantage. Some foul ulcers on the legs, and one with every appearance of a carious thigh bone, have been perfectly cured. And a servant-maid in my own

family, who had been for several years, periodically in the winter, afflicted with severe rheumatic pains in her arms and shoulders, received remarkable benefit from this water, one summer; in so much, that the winter succeeding she had little or none of her rheumatic pains, and her appetite and digestion were much improved.

So far Dr. Mackenzy. From others Dr. D. M. had been informed, that this water had been used with success in many of those cutaneous disorders commonly called scorbutic, and in curing the itch. Then follow Dr. D. M.'s experiments on this water, from which he infers that the mineral water, of Castle-Loed, is one of the strongest sulphureous waters hitherto found in Great-Britain, though he makes no doubt but that there are many such which have not hitherto been examined: that, in its natural state, it is highly impregnated with a volatile sulphureous vapour, which evaporates soon when exposed to the open air, and flies off immediately when exposed to heat; and that the water then loses its strong sulphureous smell and taste, though there is the strongest reason to suspect that it still contains a sulphureous matter dissolved in it, by some means hitherto unknown; for it neither contains an alkaline salt nor quick-lime, the two only subtances hitherto known to be capable of dissolving sulphur, and keeping it suspended in water: that it lets drop to the bottom of the well, and of its channels, a fine powder of sulphur, which adheres to the leaves and branches of trees found there. As this water contains but very little purging salt, and does not operate by stool, sea water, or some purging salt, may be added to the first glasses drank in a morning, when purging is required. Equal parts of the Castle-Loed and sea water mixed together, make a water in most respects similar to the Harrowgate; and probably will be found to answer in most cases where the Harrowgate has been found useful; and it may often be used with more advantage than the purging sulphureous waters, as they sometimes purge people of weak constitutions too freely, and weaken them too much.

With regard to the second of these mineral waters, viz. that of Fairburn, (which Dr. Mackenzy states to be a weaker water of the same nature as that of Castle-Loed) Dr. D. M. infers from his experiments, that though it does not appear to be such a strong sulphureous water as the Castle-Loed, yet it may have its uses, and be serviceable to those who have not an opportunity of using the other; and it may perhaps be useful in some cases, where the other may not agree.

On the subject of the other mineral water here mentioned, viz. the salt purging water of Pitkeathly, in the county of Perth, Dr. D. M. remarks that there are but few salt purging waters, which have hitherto been discovered in Scotland; the Pitkeathly, situated about 6 miles from the town of Perth, is the one in most esteem, and the most frequented.

As no particular treatise had been published on these waters, and Dr. D. M. wished to know their particular nature and contents, he wrote to his Grace the Duke of Athol, whose seat of Dunkeld is within 14 or 15 miles of the wells, begging the favour of him, to ask some of the medical people in the neighbourhood to examine them, and to send him an account of the result. And in consequence his Grace was so obliging as to send him a letter from Dr. Wood, of Perth, giving the following description of the Pitkeathly springs; and afterwards 6 bottles of the water.

"The spring rises in a very low marshy ground, undistinguishable from any other; but, by the taste of its water, it is generally believed to contain no mineral principle, but a small proportion of marine salts. It acquires somewhat of a putrid taste by keeping, but retains its purging quality; and it keeps much better in open, than in corked bottles. It purges gently, and without griping. An adult person drinks commonly a bottle and a half or 2 bottles, in a morning. In scrophulous and scorbutic habits, it is certainly a most useful water. A new spring has been lately discovered about 200 or 300 yards from the old one, but its waters seem to be much of the same strength and quality as the former."

Dr. D. M. afterwards wrote to Dr. Wood, and begged to know of him what proportion of sea salts these waters contained, and whether they had any mixture of a bittern in their composition; and he had the following answer, dated Oct. 17, 1770. "Since I received your letter, I evaporated a Scotch pint (4 lb.) of these waters in a white stone basin, and I obtained 2 drs. of a salt, which always ran per deliquium, and would not crystallize. I shall try it again in the summer, as at this season the air, being much charged with watery particles, may have prevented the crystallization. By dropping a solution of potash into 3 Scotch pints (12 lb.) of the waters, I got 85 grs. of a very fine magnesia."

The 6 bottles of this water which were sent to Dr. D. M., arriving at a time when he was much engaged, they remained for several months in the hamper in which they were originally packed; and he did not try any experiment with the water till the 2d of Oct., 1771. It was then clear and transparent as the purest rock water, only it seemed to have some few particles of light earth swimming through it. It had then a fetid sulphureous smell, resembling somewhat that of a foul gun or of rotten eggs, and it tinged silver in the same way as sulphureous waters; and it had a sulphureous and slight saltish taste. This fetid sulphureous smell, taste, and property of tinging silver, which this as well as most other salt waters acquire by keeping, he suspected to be owing to a fermentation taking place in the water, and slightly uniting some of the fine oily matter with some of the acid of the salts which these waters contain, and thus forming a sulphureous vapour, which is volatile while they remain slightly united, but which by a more intimate union would form a real fixed sulphur.

From Dr. Wood's account of this water, it is evident that this fetid vapour, or at least the principles which form it, are volatile; for he says the water keeps much better in open than in corked bottles.

Each drop of a solution of the fossil, as well as of the vegetable alkali, occasioned a thick white cloud, that fell to the bottom of the glass. And each drop of a solution of silver in the nitrous acid gave a milky cloud. Syrup of violets became green, and an infusion of galls occasioned no particular change of colour. 102 oz. 3 drs. and 1 scr. were put into a large stone basin, and set on a sand heat to evaporate with a slow fire. As soon as the water was warm, it let drop a light dark coloured earth, which gathered in small heaps at the bottom of the basin; and during this time, the water threw up some air bubbles to its surface; when it was evaporated to about a pint (1lb.) it was taken off the fire, and filtrated through paper: the coffin through which it passed, after being dried, was found to have acquired 21 grs. of additional weight; though he could not collect more than 3 grs. of a stone grey coloured earth, which proved to be of the absorbent or calcarious kind, for it effervesced with and dissolved in the vitriolic acid; the remaining additional weight of the coffin, he believes, depended on some of the salts of the water being taken up by the spungy filtrating paper.

After this, the water was again set on the sand heat, and evaporated till a pellicle appeared on the surface; and during the evaporation it threw up a great number of air bubbles: after this, it was set in a cool place for 3 days, at the end of which time there appeared a quantity of thin lamellæ, mixed with a small granulated salt, covered with a light coloured yellowish liquor; these he separated, and threw the liquor into filtrating paper; and by these operations he got $53\frac{1}{2}$ grs. of a salt which tasted sharp and salt, besides what had been taken up by the coffin, which had increased 9 grs. in weight more than he had got of salt. This salt being put in a tea cup appeared next day white, and had contracted a little moisture, but did not run per deliquium.

The remaining water, which was now a yellowish ley, was again evaporated to a pellicle, and he separated a quantity of white salt in lamellæ, which remained moist, till it was set in a tea cup on the sand heat, and evaporated to dryness, when it weighed 1 dr. and 14 grs.; this salt attracted more moisture than the former, and seemed at first as if it would run soon per deliquium; but the next day it remained in the same state.

As he imagined that both this, and the salt before separated, was mostly sea salt mixed with a bittern and oily matter, which prevented the crystallization; he dissolved the whole of both in distilled water, and evaporated with a very slow fire till a crystallization began to appear, and then set it in a cool place, and got some large perfect crystals of sea salt; and by repeating this several times, he obtained a full drachm of perfect crystals, which diminished in their size as the

process advanced, and afterwards 1 scr. more of thin lamellæ, which, on examining with a magnifying glass, appeared to be made up of small square crystals; there remained a small quantity of a salt ley, which probably would have yielded a few more such lamellæ.

The liquor which remained after the first 2 parcels of salt were separated, was next evaporated; but no pellicle appearing, the operation was continued till it was quite dry, when it formed one transparent yellow or amber-coloured salt cake, which weighed 1 dr. and 34 grs. This salt, on being put into a tea cup, presently began to run per deliquium, and dissolved entirely by standing in a cupboard in a room where there was a fire; but the fire having been let out in the evening, and the night proving cold, he found next morning that a crystallization had taken place, for there was a crystallized cake at the bottom of the cup, which was covered with an amber coloured ley; it at first seemed to be all one piece, with a number of small points standing on its surfaces; but on reclining the tea cup to a side, it then appeared to be made entirely up of a number of oblong crystals about the length of a barleycorn, but not so thick, and that the points before-mentioned were the ends of these crystals.. Not having time to examine them particularly in the morning, and to know their exact figure and number of sides, he set them by, till he should come home again about 1 o'clock; but the day proving warm, they were mostly dissolved before that time.

Oil of vitriol, dropped into a tea cup in which there was some of this ley, immediately occasioned a white firm coagulum like chalk, which was insoluble in water, and, when well washed and freed of its acid, felt gritty, and was quite insipid in the mouth; this is certainly a selenites formed by the earth of this ley and the vitriolic acid.

From this account of the Pitkeathly waters, it appears, says Dr. D. M., that 6 lb. 6 oz. 3 dr. 1 scr. besides a few grains of an absorbent or calcarious earth, contain 3 drs. 41$\frac{1}{2}$ grs. (besides what was lost in filtrating and other operations) of a saline matter, of which near $\frac{2}{3}$ were sea salt, the rest a bittern or salt with an earthy basis, which concreted by the force of fire into a yellowish saline mass, that runs soon per deliquium, and crystallizes though with difficulty. The small quantity he had of residuum prevented him from determining with precision, the exact proportion of sea salt and of this bittern; neither was he for the same reason, able to determine whether this bittern or ley was all made up of a calcarious marine, with on oily matter common to all waters, or whether it contained likewise a sal catharticum amarum with a vitriolic acid. From the acid of vitriol forming an insoluble selenites with the earthy basis of this bittern, it is evident, that at least all the earthy basis is not a magnesia, such as makes

the basis of the sal catharticum amarum of the shops, or what goes by the name of Epsom salts, otherwise it would have formed a salt easily soluble in water.

IV. Account of a Solar Eclipse observed at George's Island, by Captain Wallis, and several Astronomical Observations made at Portsmouth. By Mr. George Witchell, F. R. S., and Master of the Royal Academy at Portsmouth. p. 33.

Extract of a Letter from Captain Wallis, June 20, 1771.

"Saturday, July 25th, 1767, being at anchor in his Majesty's ship Dolphin in harbour, went on shore on a low point of land, not above 4 feet higher than the sea, and observed an eclipse of the sun as below. Latitude, by the mean of many observations, 17° 30′ south, longitude, by various observations of the distance of the sun from the moon, between 149° 30′ and 149° 50′ west from London. The eclipse began at $6^h\ 51^m\ 50^s$ ap. t., and ended at $8^h\ 1^m\ 0^s$; duration $1^h\ 9^m\ 10^s$. They were not certain of the instant of the beginning of the eclipse, from a little negligence; but very certain of the end."

Remarks by Mr. W.—As the sun's altitudes are given, without any correction, Mr. W. supposes they were taken by bringing down the image of the sun, till it appeared bisected by the visible horizon: he therefore recomputed the time, by allowing for the dip and refraction, which together amount to 8^m. This correction makes the apparent time of the beginning $6^h\ 51^m\ 12^s$, and the end $8^h\ 0^m\ 37^s$; hence the duration of the eclipse was $1^h\ 9^m\ 25^s$; but, by a careful computation from Mayer's new tables, the duration should have been $1^h\ 13^m\ 20\frac{1}{2}^s$, which is almost 4^m longer than the observation affords; but as it is remarked that the beginning was not exactly taken, and the moon entering very obliquely on the sun, the defect in 4^m would be but little. It seems most reasonable to attribute the whole of the error to the beginning of the eclipse. Mr. W. therefore deduced the longitude from the end, and made it to be $9^h\ 55^m\ 55^s$ west from Greenwich, or 148° 58′$\frac{3}{4}$, which is 41′$\frac{1}{4}$ less than the mean result of the lunar observations, which, considering all circumstances, is not a very great difference for the first observations that were ever made on this island.

Astronomical Observations made at the Royal Academy, Portsmouth.

1769, May 9, at $8^h\ 13^m\ 9^s$, apparent time, Mr. Bradley observed the immersion of ζ Π^{orum} by the moon; uncertain to a few minutes, on account of the strong twilight. The emersion was not taken. The transit of Venus, and solar eclipse, next morning, were both observed here; but, having then no better instrument for determining the going of the clock, than an indifferent Hadley's sextant, I do not think the observations worthy of being laid before the society; and, for the same reason, omit the observations of the comet.

1770, April 7, at $11^h\ 23^m\ 33^s$, ap. time, by Mr. Bradley's observation, the

moon occulted ε ♌'s. My time was within 2^s or 3^s the same; but we did not observe the emersion. This occultation was observed both at Greenwich, by Mr. Maskelyne, and at Oxford, by Professor Hornsby; by comparing which, it appears that this place is west of Greenwich $4^m\ 24^s\frac{1}{2}$ of time, and that Oxford is west of Greenwich $4^m\ 58^s\frac{1}{2}$.

1770, April 28, at $9^h\ 48^m\ 13^s$, apparent time, Mr. Bradley and I, both at the same instant, observed the immersion of ζ ♉¹ by the moon. The emersion was not taken. By comparing this with Mr. Maskelyne's observation, our longitude comes out $4^m\ 23^s\frac{3}{4}$ west from Greenwich.

These observations were made before our observatory was finished; but that being completed in the month of September, and furnished with an excellent, though small, mural quadrant and transit instrument, both made by that eminent artist Mr. John Bird, we began to observe meridian transits, from which I shall select those that were made for determining the solstices, and the oppositions of the 3 superior planets, which I shall transcribe, just as they were taken, excepting only making the necessary allowance for the error of the line of collimation.

Observations for determining the Solstices.

By comparing these obs. together, I make the true zenith distance of the sun's centre, at the winter solstice, to be	74°	16′	13″.4
And at the summer solstice	27	19	51 .6
Therefore, the distance of the tropics	46	56	21 .8
Its half is	23	28	10 .9
By Mr. Mayer's tables, the decrement of the obliquity, in 3 months is			0.1
Hence the mean obliquity, December 21, 1770, is......	23	28	11 .0
And June 21, 1771	23	28	10 .8
Therefore the lat. of the observatory, by these observations is	50	48	2 .4 Nor.

Next followed some observations, by Mr. W. on the oppositions of the superior planets to the sun.

V. Abstract of Mr. T. Barker's Meteorological Register at Lyndon, Rutland. p. 42.

This abstract contains the quantity of rain which fell last year, (1771), and an abstract of his observations of the barometer and thermometer, with a general account of the weather. The whole depth of rain was 17.588 inches.

VI. Directions for Using the Common Micrometer, taken from a Paper in the late Dr. Bradley's Hand Writing; communicated by Nevil Maskelyne, Astron. Royal, and F. R. S. p. 46.

Micrometers, as first contrived, being only adapted to the measuring small angles, as the diameters of the sun and moon, or other planets, and taking the

distance of such objects as appeared within the aperture of the telescope at the same time, they were not of so general use as those which are contrived not only to answer the ends that the first inventors aimed at, but also to take the difference of right ascension and declination of such objects as are farther asunder than the telescope will take in at once, but which pass through its aperture at different times. Mr. Cassini first made use of threads, intersecting one another at half right angles, for determining the difference of right ascensions and declinations of objects near the same parallel; and this apparatus, being simple and easily procured, is of very great use to such as are not provided with a micrometer made according to the late improvements. But, where such a one is at hand, that method, however curious, need not be made use of, the micrometer serving for the same purpose with greater exactness. It was for this reason indeed that the late alteration in the form of the micrometer was made, they being before not so convenient for making such sort of observations, both hairs being usually moveable, and no provision being made for setting the hairs parallel to the diurnal motion of the objects to be observed; both which inconveniencies are avoided in the present micrometers.

The micrometer, as now contrived, is not only of use in measuring small angles or distances, between such objects as appear within the aperture of the telescope at the same time, but also in taking the difference of right ascension and declination between stars and planets, &c. which in their apparent diurnal motion follow one another through the telescope, if kept in the same situation. In making the first kind of observations, turn the short tube which carries the eye glass and micrometer, &c. till the cross thread (or that which cuts the parallel threads at right angles) lies parallel to a line passing through the objects whose distance is to be measured; and then, by raising or depressing the telescope by help of the stand, bring the objects to appear on or near the cross thread, and one of them just to touch the fixed parallel thread: then turn the index of the micrometer till the moveable thread touches the other object, and the number of revolutions, and parts of a revolution, shown by the index, turned into minutes and seconds by the table made as hereafter directed, will be the apparent angular distance of those objects. It is here supposed, that the threads exactly close, so as to touch each other when the index stands at the beginning of the divisions: for, if they do not, there must be an allowance made in every observation; to avoid which, it is always best to adjust the threads to the beginning of the divisions when they are first put on; for which purpose, the holes in the little plate which carries the moveable thread are made oblong, to give room to move it as occasion requires, before it is pinched hard by the small screws which fasten it to the moveable arm, through which the long screw passes. The other parallel thread, which he calls the fixed one, must be first

adjusted by setting its edge exactly over the two marks made on each side the short diameter of the aperture in the broad plates, and the cross thread must be likewise set to agree with the strokes made on each side the longest diameter, and then the intersection of the cross thread and the fixed parallel one, will be the centre of the motion given to the outer plate of the micrometer (to which the great screw index and threads are fastened) by the worm, by turning of which the fixed parallel thread may easily be made to lie parallel to the apparent motion of any object, in order to take the difference of declination and right ascension from any other, that follows through the aperture of the telescope.

This contrivance is of very great use to make a star, &c. move true along the fixed parallel thread, which is absolutely necessary in order to take the true difference of right ascension and declination between it and any other that follows. Without this contrivance it is very difficult to make a star move exactly on the thread, and it can only be done by repeated trials, which may sometimes take up a great deal of time.

If therefore a star be made to move on the parallel thread just at the cross, and (the telescope continuing fixed in the same position) it be afterwards, near its going out of the aperture, found not to be on the thread, that must then be brought to the star by the help of the worm, and then the thread will lie parallel to the diurnal motion of the star in that part of the heavens, and consequently the cross thread will represent a meridian, and the others parallels of declination, and the difference of time between the passage of the star at the cross wire (which was made to move along the thread), and the transit of any other star, &c. over the cross thread which represents a meridian, turned into degrees and minutes, will give the difference of right ascension. And if the moveable parallel thread be brought, by turning the index, to touch the other star about the time of its passage over the cross thread, then the number of revolutions and parts shown by the index, turned into minutes and seconds of a degree by the table, will be the difference of declination between the two stars. If the star be made to pass along the fixed thread so as to seem perfectly bisected, there must be an allowance made for the semidiameter of the thread or wire, because he supposes the index to be adjusted as before to the inner edges of the wires; but it may, if found convenient, be adjusted to the middle of the threads, or else correction may be made in the observed distance.

In taking any angle, it is convenient that each of the parallel threads be about the same distance from the middle of the aperture of the eye-glass; and for this reason the whole micrometer is contrived to slide to and fro, as the case requires. The same motion is also of use in taking the difference of right ascension and declination, by sliding the fixed parallel thread (on which the preceding star is brought to move) towards one side of the eye-glass; for by that means a greater

angle may be taken in between the parallel threads, if need be. And it must always be remembered that the moveable parallel thread should be set either north or south of the other, according as the following star is expected to be really south or north of the preceding.

In making an observation, either the inner or the outer edges, or the middle of the wires, may be brought to touch the objects; but then it must be remembered to allow something for the thickness of the wire, in case the observation be not made from that part to which the index is adjusted. In observing the diameters of the sun, moon, or planets, it may perhaps be most convenient to make use of the outer edges of the threads, because they will appear most distinct when quite within the limb of the planet, &c.; but if there should be any sensible inflection of the rays of light in passing by the wires, this would be best avoided by using the inner edge of one wire and the outer edge of the other. And in taking the distance or difference of declination between two stars, &c. the middle of the threads may perhaps be most convenient: but, however the observation is made, due correction must be allowed for the thickness of the wire, if requisite.

The difference of declination of two stars, &c. may be observed with great exactness, because the motion of the stars is parallel to the threads; but in taking any other distance, the motion of the stars being oblique to them, is a great impediment, because if one star be brought to one thread, before the eye can be directed so as to judge how the other thread agrees to the other star, the former must be somewhat removed from its thread, so that in this sort of observations the best way of judging when the threads are at the proper distance, is by frequently moving the eye backwards and forwards from one to the other: this method must chiefly be made use of when the distance of the objects is pretty large, and the motion or rolling of the eye great.

The micrometer is so contrived that it may be applied to telescopes of different lengths; but then there must be a table for each telescope, by which the revolutions of the screw may be turned into minutes and seconds of a degree. In order to this, it is necessary that the threads of the micrometer should be placed exactly in the common focus of the object-glass and eye-glass, that is, where images of objects seen through the telescope are distinctly formed. The readiest way of doing this, is, first to slide the micrometer into the grooves fixed to the short brass tube, which carries the whole apparatus of eye-glass, &c. and then to draw the eye-glass out by means of its sliding work, till the threads of the micrometer be in its focus, which is known by their appearing most distinct, &c. Then thrust the short tube before mentioned into its proper place, as far as the shoulders of the brass work will admit, and place the object glass in its cell, and looking through the telescope at some very distant object, slide the wooden tube

in or out till you make the object appear most distinct, or till it has the least motion on the threads when the eye is moved to and fro; for then the threads of the micrometer will be in the common focus of both glasses, and that will be the proper distance that the object-glass ought always to be at from the threads; and there should be made some mark or ketch in the wooden tube, in order to set it always at the same distance.

The proper distance of the threads from the object-glass being thus settled, the table for turning the revolutions, &c. of the screw into angles, or minutes and seconds of a degree, may be made several ways; but as good and easy a method as any is, carefully to measure how many inches and parts of an inch the object glass is distant from the threads, and with the same scale to find also how many inches and parts of an inch 100, &c. revolutions or threads of the screw of the micrometer are equal to: then, making the first distance radius, the last will be the sine or tangent of an angle answering to 100 revolutions. And having the angle answering to 100 revolutions, the angle for any other number will be easily known and set down in the table, as also the parts of a revolution: for in small angles, such as can be observed with the micrometer, their sines, tangents, or chords, are nearly in the same proportion with the angles themselves. The distance before mentioned, to be used as radius, ought strictly to be taken from the threads, to a point within the object-glass, about one third of its thickness, from that surface which is towards the wires, if the glass be, as usual, equally convex on both sides; but if the focus of the object glass is pretty long, and its thickness not great, the error that can arise by measuring from any part of the object-glass will become insensible as to the alteration in the angle.

The table for the micrometer may also be made by setting up two marks at a distance on the ground, and observing with the micrometer the revolutions, &c. which they subtend when seen through the telescope, and then computing the angles those objects subtend at the object-glass, by measuring their distance from each other and from the object-glass. The like may also be done by opening the threads to any number of revolutions, and then making a star move exactly on the perpendicular thread, and noting the time it is passing from one parallel thread to the other; for that time turned into minutes and seconds of a degree, by allowing for the star's declination and going of the clock, &c. will be the angle answering to the number of revolutions; from which the whole table may be made. This method perhaps might be most advantageously practised in stars near the pole, where the apparent motion being slow, a second in time will answer to a much smaller angle than towards the equator. But he believes, on trial, the first method will be found most easy and practicable, especially if the scale made use of be well divided.

VII. Of the Roots used by the Indians, in the Neighbourhood of Hudson's Bay, to dye Porcupine Quills. By Mr. John Reinhold Forster, F.R.S. p. 54.

Among the curiosities presented by the Hudson's Bay Company to the R. S., is a small parcel of porcupine quills, dyed by the wild natives, some red and some yellow, with the roots of some plants they use for that purpose. Mr. F. examined them carefully, and found that they are probably of the same kind with those mentioned by Prof. Kalm, vol. 3, p. 14, and 160 of the English translation. The one root, dying yellow, is called by the French in Canada, Tisavoyanne jaune; the other, dying red, has the name of Tisavoyanne rouge. Prof. Kalm declares the latter to be a new plant, belonging to the genus of galium, and received by Dr. Linnæus in his Species Plantarum, p. 153, by the specific name of tinctorium, on account of its dying quality. It grows in woody, moist places, in a fine soil. Kalm observes, 'that the roots of this plant are employed by the Indians in dying the quills of the American porcupine red, which they put into several places of their work: air, sun, and water, seldom change this colour. The French women in Canada sometimes dye their cloth red with these roots, which are but small, like those of the galium luteum or yellow bedstraw.' Dr. Linnæus describes this plant, as having 6 narrow linear leaves at each knot of the stem, and 4 at the branches; commonly 2 flowers are on each stalk, and its seeds are smooth. The roots, when dry, are of the thickness of a crow quill, brown on the outside, and of a bright purple red, when broken, on the inside.

The 2d plant, or the Tisavoyanne jaune, is, according to Prof. Kalm, vol. 3, p. 160, 'the three-leaved hellebore (helleborus trifolius Linn.); grows plentifully in woods, in mossy, not too wet, places. Its leaves and stalks are employed by the Indians to dye yellow several kinds of their work, made of prepared skins. The French learned from them to dye wool and other things yellow with this plant.' Among the roots sent as a specimen from Hudson's Bay, Mr. F. found several leaves, which he separated, and found the plant undoubtedly to be the three-leaved hellebore. In the 4th vol. of Dr. Linnæus's Amœnitates Academicæ, is a figure of this plant, which on comparison Mr. F. found by no means to be accurate: for the leaves in our specimens, and in those collected by a gentleman who favoured him with the sight of the plant, are far more pointed, than in the engraved figure. The stalks have constantly but one flower.

The dyed porcupine quills sent along with the roots from Hudson's Bay, are of the brightest red and yellow: and this circumstance suggested the thought of trying whether these roots might not be usefully employed in dying. For this purpose, he boiled a piece of flannel in a solution of half salt of tartar and half alum: the wet flannel was put into the decoction of the three-leaved hellebore

roots, and boiled in it for the space of about 12 or 15 minutes; the flannel, when taken out, was dyed with a bright and lasting yellow dye. A white porcupine quill, boiled in the same decoction, became nearly of as bright a yellow, as those sent over from Hudson's Bay. This experiment made him believe that he had hit upon the right method of dying with the three-leaved hellebore; and will, he hopes, prompt the directors of the Hudson's Bay company to order larger quantities of this root from their settlements, as it will no doubt become a useful article of commerce.

The flannel, boiled in salt of tartar and alum as above mentioned, was likewise immersed and boiled for nearly the same space of time as in the former experiment, in a decoction of the root of the galium tinctorium, but it would dye only a dull and faint red. A porcupine quill boiled with it became yellow, but by no means red. This operation convinced him, that the Indians must certainly have some method to extract the bright and lasting colour, which he could not do. They use perhaps the root quite fresh, which circumstance probably makes them succeed in their dying process. If it could be brought about, to extract and afterwards to fix on wool the dye of this root, it would, no doubt, on account of its bright colour, be a valuable acquisition for our manufactures, and he does not in the least doubt of the probability of succeeding in the attempt, as the woollen stuffs are animal substances as well as the porcupine quills, and therefore easily susceptible of any dye.

The Spaniards of Mexico have but lately learned of the inhabitants of California, the art of dying the deepest and most lasting black that ever was yet known. They call the plant they employ for that purpose cascalote; it is arboreous, with small leaves and yellow flowers; its growth is still slower than that of an oak; it is the least corrosive of all the known substances employed in dying, and strikes the deepest black: so that, for instance, it penetrates a hat to such a degree, that the very rags of it are thoroughly black. The leaves of the cascalote are similar to those of the husiaoke, another plant likewise used for dying black with, but of an inferior quality. The latitude of California gives us hope that the country near the Mississippi, or one of the Floridas, contains this cascalote, the acquisition of which would be of infinite use in our manufactures.

VIII. Of a Subærated Denarius of the Plætorian Family, adorned with an Etrus- Inscription on the Reverse, never before published or explained. By the Rev. J. Swinton, B.D., F.R.S. p. 60.

This piece exhibits on one side a female head, representing the goddess Libera, or Proserpina, according to M. Havercamp, before which stand the letters P. COSINI, very ill preserved. On the reverse, we discover a bust of the goddess SORS, on a sort of basis, adorned with the inscription F SOR ANT; under which,

in the exergue, appear in Etruscan letters FIR, or rather FVR, ANTIE, i. e. FORS, FORTVNA, or SORS, ANTII, or ANTIAT, equivalent to the Latin inscription above it. The Etruscan elements seem rather better preserved than the Latin. The coin is however in but indifferent conservation, though pretty much of the thin silver plate remains still upon it.

The symbol on the reverse here is the same that occurs on the reverses of 2 or 3 other consular coins of the Plætorian family, with the word SORS attending it. The Latin inscription on this piece is extremely similar to one on a denarius of the Rustian family. The symbol there is a double Fortune, or rather 2 galeated Fortunes, which were considered as deities by the Romans. The Etruscan inscription, on the reverse of this denarius, in the exergue, seems to allude to a passage in Tully, relative to the origin of those deities denominated SORTES by the Romans, and to be illustrated by, as well as to throw some light upon, that famous passage: and as the inscription formed of those characters, mentioned by Tully, cannot well be supposed to have contained any other word than FIR, or rather FVR, applicable to the deity, or deities, so called, and worshipped, both at Antium and Præneste; we may fairly suppose the Etruscan inscription to have glanced at that celebrated passage.

The medals of the Plætorian family similar to this, Havercamp takes to have been struck in the time of the civil war that succeeded Julius Cæsar's death; in which perhaps he may not be very remote from truth. If it should however be allowed probable by the learned, this coin, which must be nearly of the same date with that war, will seem to have preceded about 40 years the birth of Christ.

Who P. Cosinius was, whose name seems to have been handed down to us by the denarius, cannot at present be known. But that the Cosinian family was of some note in Rome, we may infer, not only from this curious denarius, but likewise from 2 or 3 ancient Roman inscriptions, which have preserved the name of that family. As for M. Plætorius, mentioned on this, and other similar coins, he was, according to M. Havercamp, questor to Brutus, one of Cæsar's murderers; and this piece appeared a little after that emperor's death. The Etruscan letters were not then entirely out of use: nay, they were not totally disused in some parts of Italy, and particularly at Falerii, a considerable number of years after that tragical event. This we learn from Strabo, who flourished when Tiberius sat upon the imperial throne.

IX. A Deduction of the Quantity of the Sun's Parallax from the Comparison of the several Observations of the late Transit of Venus, made in Europe, with those made in George Island in the South Seas. Communicated by Mr. Euler, Jun., Sec. of the Imp. Acad. of Petersburg. From the Latin. p. 69.

This is a brief account of a calculation of the sun's parallax, given by Mr.

Lexell, in the Petersburg Commentaries, vol. 16, after the method of Mr. Euler, senior, in the 14th vol. of the same Commentaries. This calculation is deduced from the observations made in several places, as mentioned in the title of the paper. Since the observations of the external contacts made at King George's Island, seem not to agree well with those of the internal, the author deems it proper to consider the two cases separately; the former way according to both the external and internal contacts, and the other the internal contacts only. Now by combining the observations of George Island with those made in Europe, the sun's horizontal parallax found for the former case is $p = 8.68 - 0.0077y$, for the latter $p = 8.58 - 0.0080y$; where y denotes the correction of Venus's geocentric latitude for the assumed time of conjunction. When in a similar manner the observations made at Prince of Wales's Fort in Hudson's Bay, are compared with the European, he obtains for the former case $p = 8.82 - 0.0019y$, and for the latter $p = 8.74 - 0.0022y$. Lastly, the California observations, compared with the European, give $p = 8.61 - 0.0062y$. That, among these conclusions, some certain medium may be taken so as to be near the truth, it must be noted that each of these is to be considered as possessing a greater degree of certainty, as the co-efficients are greater, by which the equations are affected, from which those values of the letter p are derived, as then the less is the chance of error in the observation for changing the true value of the parallax. The probabilities therefore of the conclusions deduced from the several American observations, estimating in this way, are found to be proportional to the numbers 11, 8, and 4. Also to distinguish the best observations from the more uncertain ones, the author adopts 3 hypotheses: viz. 1. How the medium above mentioned may be taken from the conclusions which are found, when all the observations indiscriminately are used, by which is obtained $p = 8.63 - 0.0063y$; 2dly, by taking into the computation only the times of the internal contacts for George Isle, which gives $p = 8.57 - 0.0057y$; and 3dly, by excluding the observations of the external contacts made at Hudson's Bay, which gives $p = 8.62 - 0.0065y$. But as there does not appear sufficient reason why the times of the exterior contacts should be accounted doubtful, the medium so taken, between the medium deduced from the two latter hypotheses, may safely be taken to give the parallax $p = 8.60 - 0.006y$. For a final verification of this conclusion, each of the American observations were compared with those made in Lapland, where both the ingress and egress of Venus could be observed, as, for this kind of observations, the errors arising from the longitude of places, in estimating the parallax, are of small moment. Then the mediums being taken as above said, give, for the three hypotheses, the following values of p: viz. 1st, $p = 8.68 - 0.0076y$; 2d, $p = 8.67 - 0.0074y$; 3d, $p = 8.62 - 0.0077y$; And these conclusions differ not more from those

above found, than might arise from the small errors of observation. Lastly, from the several American observations collated together, are derived the following values of p: viz. from the external contacts at Hudson's Bay and George Island, $p = 9.16 - 0.011y$; from the internal contacts at the same places $p = 8.47 - 0.011y$; from the internal contacts at Hudson's Bay and California $p = 8.46 - 0.0096y$; and lastly from the internal contacts at California and George Island $p = 8.48 - 0.0012y$.

But now to find the true value of the correction y, for determining the absolute value of the parallax p, it is observed that from the times of the internal contacts it seems that this correction must be about 8″, supposing the sun's semidiameter to be 946″.38, which is a mean between the value of the semidiameter assumed by M. Lalande, and that used by the English astronomers. Then this value used for y, gives 8.″55 for the sun's parallax, and the semidiameter of Venus 28 .6, which must be within 2 or 3 tenth's of a second of the truth. But if the correction of the latitude be a little less, and it is probably not below 5″ by the micrometer observations, this will not reduce the value of the parallax more than the 50th part of a second.

Another calculation is made in two different ways, from the observations at George Island; the one by combining the internal contact observed at the ingress by Mr. Green, with the internal contact at the egress by Capt. Cook; and the other, by a mutually change, by the observation of the former internal contact of Capt. Cook combined with that of the latter by Mr. Green. The former hypothesis gives the parallax $p = 8.48 - 0.0080y$, and the latter $p = 8.65 - 0.0080y$, the mean between which, $p = 8.57 - 0.008y$, differs very little from the former determination.

X. On a New Chart of the Red Sea, with two Draughts of the Roads of Mocha and Judda, and several Observations made during a Voyage on that Sea. By Capt. Charles Newland. p. 77.

This chart of the Red Sea, was constructed from materials that Capt. N. became possessed of, during his residence in the East Indies; which chart, on his voyage to Mocha and Judda, he experienced to be the best he ever saw. The only material error he discovered in it, is, that the Abyssinian shore opposite Mocha is placed too far to the westward by 25 or 30 miles, and that there are several small islands on the same shore, not taken notice of in any chart.

Longitude of Judda by 12 distances of the ☾ from ☉.

Worked by the British mariner's guide	39°	53′	45″
And by the Ephemeris for 1769	40	1	7
Difference		7	22

By Jupiter's satellites	39°	$26'$	$45''$	E
By ☾ and ☉	40	1	7	E
Difference		34	22	

XI. Remarks and Observations made on Board the Ship Kelsall, on a Voyage to Judda and Mocha, in 1769. *By the Same.* p. 79.

In the run from Socatra to Cape Aden, Capt. N. made the dist. $8^{\circ} 20'$ W. and from Cochin $29^{\circ} 39'$ W. The latitude of the above cape he made $12^{\circ} 45'$ N. for the south point of it. This cape, or headland, is one of the most remarkable he ever saw, when coming from the eastward; it is so very high and rugged, that it may be seen, 15 leagues at least in fine weather. The tops of those ragged rocks resemble so many chimneys and spires; and, on approaching the cape, you see a zigzag wall, or whitish pathway, cut through the rocks, not at a very great distance from the waterside; a little below this, at the S. E. end, is seen something that looks very like two mosques; but this cannot be seen at a greater distance than 4 or 5 leagues; but when it is, you may be certain it is Cape Aden, and may then steer your course for Babelmandel accordingly.

A little to the westward of this cape, there is another high craggy headland equally high and craggy as that of Aden, between which two there is an opening, much resembling a small narrow strait, but in reality it is only a deep bay, the bottom of which is very low land, so low, that it cannot be seen from the mast-head, except you are close in shore: by this deception, people have mistaken it for the strait of Babelmandel, and have been so far embayed, before they perceived their mistake, that it was with the greatest difficulty they got out again.

On each side of this bay lies a large rock, just at the entrance, and at about a quarter of a mile from the shore: when these are seen, you may be sure it is not the strait of Babelmandel. Was a ship to fall in with this place, and had not had an observation for some days before, it would be very easy to mistake one for the other; there is only this difference, that Cape Aden is high and ragged, and Babelmandel is rather low and smooth, and the island, as the Directory observes, makes like a gunner's coin. The best course to steer from Cape Aden to Saint Anthony is W. by S. by the compass, and that will carry you clear of the shoal lying off that point.

From Mocha towards Judda, the islands of Jebbel-Zeker Aloric are pretty large, and may be seen in clear weather 7 or 8 leagues; they are 6 in number, the southernmost lies in the latitude of $13^{\circ} 45'$ N. and bears from Mocha N.W. by W. nearly, distance about 40 miles. A little to the northward of those islands lies Jebbel-Zeker, a very high large island, that may be seen in fair weather 12 or 13 leagues. Very near this island, on the N. E. side, lie 3 small ones, not

discernible at a distance of 4 leagues. The N. end of the large island Jebbel-Zeker lies in the latitude of 14° 10′ N. The true course from Jebbel-Zeker to the Subugars is N.W. by N.; distance 20 leagues. N.E. of those islands lies a low white island, which he called Sandy Island, environed all round with shoal water; to the southward of which, the shoal seemed, from the mast-head, to extend from the island 3 or 4 miles. He passed it at about 6 miles distance, and never had less than 26 fathom, sandy ground. Two or 3 miles within him appeared like very shoal water. Its latitude is 15° 22′ N.

About 40 miles N.N.E. from the Subugars, lies the island Comoran, a very low blackish island. N. W. by N. by the compass, from the Subugars, lies the island Jebbel-Tar, distance about 25 miles. This island is of a moderate height, and may be seen 9 or 10 leagues from the mast-head in clear weather; its latitude is about 15° 36′ N. and when it bore W. about 10 miles, he had 33 fathom water, a sandy bottom. From Jebbel-Tar to the small islands on the Arabian side, laid down in about 18° N. latitude, he made the course N. 22° 49′, W. distance 159 miles. The southernmost of these islands lies in the latitude of 18° 2′ N.

Should you not be so fortunate as to get a pilot before you come near Judda, it would be most certainly prudent to keep 30 or 40 miles from the shore, at least so far that you can but just discern the high land of Goofs and Gedan, at which distance there is no danger. Though this may appear a great distance for the pilots to come off to the ship, yet they will immediately do it as soon as they hear your gun, and not till then. It is indeed amazing, and almost incredible to be told, how far these pilots will hear the guns on a still morning or evening, which are the proper times for the guns to be fired. Observe to fire the first as soon as you see the sun appear in the horizon, and the second as soon as the lower limb is just out of the water; in the evening, the first as soon as the lower limb touches the water, and the second when the upper limb is below the horizon. Four firings in one day is all that are necessary; but they are to be repeated every day till you get a pilot. They know pretty near the time the India ships will arrive, and go down to the water side every night and morning, and just as the sun is rising or setting, they lay their ear close to the ground for 3 or 4 minutes, and pretend to say, that if a ship is not more than 2 or $2\frac{1}{2}$ degrees distance when the gun is fired, they can either hear the report, or find the ground shake under them; on which they take a boat and come off to pilot you in. This may seem a little extraordinary to a person that never was there; but, however strange it may appear, Capt. N. was assured by a gentleman of undoubted veracity, that he ran by the log 95 miles, from the time of firing his 2 guns in the morning, till he saw the pilot in the evening; and when he came on board, he declared that he heard the two guns that morning at sun-rising, on the strength of which, he took his boat and put off.

To sail into Judda harbour, or rather road, without a pilot, would be impossible for a stranger, there being so many sand banks and shelves of rocks; but when you are in, it is one of the safest places that can possibly be; you may make your ship fast with any old junk, and there is no danger, though you are surrounded with nothing but rocks and sands. The best bearing for anchoring is the great mosque E. by S. and the extremes of the land from S. by E. to N.N.W. distance from the landing place about 2 miles. Latitude of Judda, 21° 28′ N. Longitude ditto, 39° 26′ 45″ E. Variation of the compass, 11° 52′ W.

Latitude observed at Mocha 13° 23′ N. Variation of the compass 12° 33′ W.

XII. An Easy Method to Distil Fresh Water from Salt Water at Sea. By Capt. Newland. p. 90.

The materials necessary for this process are the following; a copper or iron pot of 15 or 20 gallons, an empty cask, some sheet lead, a small jar, a few wood-ashes or soap, and billet-wood for fuel.

First, in order to make the pipe or worm, Capt. N. took as much sheet lead as was sufficient for the purpose, and beat it on a sponge staff to make it round: this done, he was somewhat at a loss for solder; however he supplied that deficiency with good paste and dungerec, or thin canvas, laid well on, and over that a 2d coat of paste and dungerec, and then a covering of small cered line hove close together and very tight round, over which he put a 3d coat of paste and dungerec, which he found was sufficient to keep it from blowing. The next thing was to fix the pipe in the pot or still-head. When he had well secured the pot in the fagong or fire-place, he filled it about two-thirds full of salt water, about 15 gallons, with which he mixed 2 or 3 double handfuls of wood-ashes, and stirred it well together, in order to soften the salt water; he then fixed the lid (which was made of plank 3 inches thick) in which there are 2 holes, one for the end of the pipe, the other to put in water as occasion requires, without taking off the lid. It must be well observed, that the end of the pipe is not put more than 2 or 3 inches within the still head; for, should it be put too far in, when the water boils, the bubbles or saline particles get into the end of the pipe, and make the water brackish in the receiver. To prevent the steam from coming out at the plug-hole or lid, he made a kind of mortar, with wood-ashes, salt water, and rope cut very small, and beat well together, and then applied it, which answered the purpose very well. Now the pipe is fixed in the still-head, he next proceeds to carry it through the worm tub, into the receiver. The worm-tub is nothing more than an empty cask, with one of the heads taken out, and in each side a round hole cut, of about 3 inches diameter, for the pipe to pass through into the receiver, which is fixed at a little distance from the tub. The receiver has also a wooden lid like that of the still-head, with a hole in it

for the end of the worm to go through into the receiver; care must be taken, that no steam comes out there, as well as at the still head. An empty jar will answer the purpose of a receiver very well. Notwithstanding the pipe passes through the tub of cold water, the jar will be very hot; he therefore thought it necessary to keep a person continually wetting it with cold water, which not only kept the jar from breaking, but made the fresh water cold and fit for use immediately after the still was taken off. The foregoing directions strictly observed, a quantity of 8 or 10 gallons will be produced every 12 hours.

Note. Every 5 or 6 hours you must replenish the still with about 5 gallons of water; as he found the first stock consumed about a gallon per hour by boiling.

XIII. Observations on the Milky Appearance of some Spots of Water in the Sea. By the same. p. 93.

It has been remarked by several navigators, on their passage from Mocha to Bombay, Surat, &c. that they had discovered in the night spots of water as white as milk, and could never assign any reason for it; and many have been so much alarmed, that they have immediately hove to and sounded; but Capt. N. never heard of any body ever getting ground. In his passage across those seas in the Kelsall, he discovered all of a sudden, about 8 o'clock in the evening, the water all round as white as milk, intermixed with streaks or serpentine lines of black water. He immediately drew a bucket of it, and carried it to the light, where it appeared just as other water; he drew several more, and found it the same: some he kept till the next morning, when he could perceive no difference from that alongside. They had run by the log 50^{m}, from the time they first observed it till daylight, and during all that time the water continued white as milk, but at full daylight it was of its usual colour. The next evening about 7 o'clock the water appeared again as white as before; he then drew another bucket and carried it to a very dark place, and holding his head close to the bucket, could perceive, with his naked eye, an innumerable quantity of animalcules floating about alive, which enlightened that small body of water to an amazing degree. Hence he concludes that the whole mass of water must be filled with this small fish spawn or animalcules, and that this is doubtless the reason of the water's appearing so white in the night-time. They ran by the log, from the time they first saw it, till the latter part of the 2d night, the time they lost sight of it, about 170 miles. The latitude about 15° 10′ N. and S. W. dist. from Cape Aden 12° 18′ E.

On the 30th of August 1769, at 3 o'clock in the morning, he saw a comet 8° 20′ from Aldebaran S. W. and the tail streaming to the westward. He made the meridian distance from Cape Aden to striking sounding on the Malabar coast, in the lat. of 14° 2′ N., 27° 31′ E.

XIV. A Letter from Mr. Peter Dollond, describing some Additions and Alterations made to Hadley's Quadrant, to render it more Serviceable at Sea. p. 95.

The glasses of the Hadley's quadrant should have their two surfaces perfect planes, and perfectly parallel to each other. From several years practice in grinding these glasses, I have found out methods of making them to great exactness; but the advantage, that should arise from the goodness of the glasses, has often been defeated by the index glass being bent by the brass frame that contains it: to prevent this, I have contrived the frame, so that the glass lies on 3 points, and the part that presses against the front of the glass has also 3 points exactly opposite to the former. These points are made to confine the glass by 3 screws at the back, that act exactly opposite to the points between which the glass is placed. This little contrivance may be of some use; but the principal improvements are in the methods of adjusting the glasses, particularly for the back observation.

The method hitherto practised for adjusting that part of the instrument, by means of the opposite horizons at sea, has been attended with so many difficulties, that it has hardly ever been used; for so little dependance could be made on the observations taken this way, that the best Hadley's sextants made for the purposes of observing the distances of the moon from the sun or fixed stars, have been always made without the horizon glass for the back observation; for want of which, many valuable observations of the sun and moon have been lost, when their distance has exceeded 120 degrees.

To make the adjustment of the back observation easy and exact, I have applied an index to the back horizon glass, by which it may be moved into a parallel position to the index glass, in order to give it the two adjustments, in the same manner as the fore horizon glass is adjusted. Then, by moving the index, to which the back horizon glass is fixed, exactly 90 degrees (which is known by the divisions made for that purpose) the glass will be set at right angles to the index glass, and consequently will be properly adjusted for use, and the observations may be made with the same accuracy by this, as by the fore observation.

To adjust the horizon glasses in the perpendicular position to the plane of the instrument, I have contrived to move each of them by a single screw, that goes through the frame of the quadrant, and is turned by means of a milled head at the back, which may be done by the observer while he is looking at the object.

To these improvements I have added Mr. Maskelyne's method of placing darkening glasses behind the horizon glasses. These glasses, which serve for darkening the object seen by direct vision, in adjusting the instrument by the sun or moon, I have placed in such a manner, as to be turned behind the fore horizon glass, or behind the back horizon glass, that they may be used with either; there are 3 of these glasses of different degrees of darkness; the lightest

or palest I imagine will be of use in taking the sun's altitude when the horizon appears glaring, which I believe often happens by the reflection of the sea.

XV. Remarks on the Hadley's Quadrant, tending principally to Remove the Difficulties which have hitherto attended the Use of the Back-observation, and to Obviate the Errors that might arise from a Want of Parallelism in the two Surfaces of the Index Glass. By N. Maskelyne, F. R. S. p. 99.

The back observation with Hadley's quadrant being founded on the same principles, and in theory, equally perfect with the fore observation, and being at the same time necessary to extend the use of the instrument up to 180 degrees (it being impracticable to measure angles with any convenience beyond 120 degrees with the fore observation) it may seem surprising that it has not been brought equally into general use, more especially as the method of finding the longitude by observations of the moon has been practised at sea for some years past; since this method would receive considerable advantage from the use of the back observation in taking distances of the sun and moon between the first and last quarter, could such observations be as much depended on as the fore observations. The causes of this seem to have been principally these two, the difficulty of adjusting the back horizon glass, and the want of a method of directing the sight parallel to the plane of the quadrant. The back horizon glass, like the fore one, requires 2 adjustments: the first, or common one, disposes it at right angles to the index glass, when the index stands at 0 on the arch; which is usually performed by setting 0 of the index of the arch of the quadrant by double the dip of the horizon of the sea, and then holding the quadrant vertical with the arch downwards, and turning the back horizon glass about, by means of its lever or perpetual screw, till the reflected back horizon appears to coincide with the fore horizon seen directly. But this operation is so difficult in practice with the back horizon glass wholly silvered, except a small transparent slit in the middle, as it has been usually made, that few, if any, persons, have ever received proper satisfaction from it. If the back horizon glass was silvered in every respect like the fore horizon glass, which it ought to be, the upper part being left unsilvered, and a telescope was applied to it, perhaps this adjustment might be rendered somewhat easier and more exact; but it could not even thus be made so exact as the adjustment of the fore horizon glass may, by making use of the sun's limbs.

The 2d adjustment of the back horizon glass, in the common construction of the quadrant, is still more troublesome, since it cannot be executed without setting the index 90 degrees off the arch, in order to place the index glass parallel to the back horizon glass; when this adjustment may be performed in the same manner as the corresponding adjustment of the fore horizon glass. But the

bending of the index, that follows the setting it off the arch, is a very disagreeable circumstance, having a tendency, especially on board of ship, to expose both the index and centre work to damage; and may even, without extraordinary precautions taken by the instrument maker in placing the plane of the index glass exactly according to the length of the index, disturb its perpendicularity to the plane of the quadrant: on these accounts it would be much better if this adjustment of the back horizon glass could be performed, like those of the fore horizon glass, with the index remaining on the arch of the quadrant. Fortunately, this desideratum has been lately effected by an ingenious contrivance invented by Mr. Dollond, which he has given an account of in the preceding paper, by means of an additional index applied to the back horizon glass; by which both the adjustments may be made by the same observations, and with nearly the same exactness, as those of the fore horizon glass.

Besides the difficulty of adjusting the back horizon glass, the want of a method of directing the line of sight parallel to the plane of the quadrant, has proved also a considerable obstacle to the use of the back observation: this will easily appear from the following proposition, that the error of the angle measured, arising from any small deviation of the visual ray from a parallelism to the plane of the quadrant, is to twice an arch equal to the verse sine of the deviation; as the tangent of half the angle measured by the quadrant, is to radius, very nearly. Thus a deviation of 1° in the line of sight, will produce an error of about 1′ in measuring an angle of 90°, whether by the fore or back observation; but the same deviation will produce an error of 4′ in measuring an angle of 150°, of 6′ in taking an angle of 160°, and 12′ in taking an angle of 170°. Hence a pretty exact adjustment of the line of sight, or axis of the telescope, is requisite in measuring large angles, such as those taken by the back observation; and therefore a director of the sight ought by no means to be omitted in the construction of the instrument (as it commonly has been since Mr. Hadley's time, though recommended by him), except a telescope be made use of, which, if rightly placed, answers the same purpose better, especially in observing the distance of the moon from the sun between the first and last quarter. The director of the sight may be placed exact enough by construction, but the telescope cannot; and Mr. Hadley, not having been aware of the importance of an exact position of it, has accordingly given no directions for the placing it. Mr. M. therefore endeavours to supply this defect in the following remarks.

In the first place, he would by all means recommend an adjusting piece to be applied to the telescope, by which its axis may be brought parallel to the plane of the quadrant: in the next place, the back horizon glass ought to be silvered in the same manner as the fore horizon glass: and thirdly, 2 thick silver wires should be placed within the eye tube, in the focus of the eye glass, parallel to

each other, and to the plane of the quadrant. If they were put at such a distance as to divide the diameter of the field of view into 3 equal parts, it might be as convenient as any other interval. In this manner wires were placed in the telescope by Mr. Hadley, as appears by his account of the instrument in Philos. Trans., N° 420. These wires are to be adjusted parallel to the plane of the quadrant, by turning the eye tube round about which contains the wires, till they appear parallel to the plane of the quadrant. The axis of the telescope, by which is meant the line joining the centre of the object glass and the middle point between the two wires, is to be adjusted parallel to the plane of the quadrant, by either of the two following methods.

1st Method. When the distance of the moon from the sun is greater than 90 degrees, by giving a sweep with the quadrant, and moving the index, bring the nearest limbs to touch each other at the wire nearest the plane of the quadrant. Then, the index remaining unmoved, make the like observation at the wire farthest from the plane of the quadrant; and note whether the nearest limbs are in contact as they were at the other wire; if they are, the axis of the telescope is parallel to the plane of the quadrant: but if they are not, it is inclined to the same, and must be corrected as follows. If the nearest limbs of the sun and moon seem to lap over each other at the wire farthest from the plane of the quadrant, the object ends of the telescope is inclined from the plane of the quadrant, and must be altered by the adjustment made for that purpose; but, if the nearest limbs of the sun and moon do not come to touch each other at the wire farthest from the plane of the quadrant, the object end of the telescope is inclined towards the plane of the quadrant, and must be altered by the adjustment accordingly. Let these operations be repeated till the observation is the same at both the parallel wires, and the axis of the telescope will be adjusted parallel to the plane of the quadrant. In like manner, the axis of the telescope may be also adjusted parallel to the plane of the quadrant for the fore observation.

Second method. Set the index to 0, and hold the plane of the quadrant parallel to the horizon of the sea, with the divided arch upwards, the two wires being parallel to, and including both the direct fore horizon, and the reflected back horizon, between them. Raise or lower the plane of the quadrant till the direct and reflected horizons coincide together: if the coincidence happens in the middle between the two wires, or rather, to be more exact, above the middle by such a part of the field of view as answers to the number of minutes in the depression of the horizon (which may be easily estimated if the angular interval of the wires be first found by experiment, in the manner hereafter mentioned) the axis of the telescope is parallel to the plane of the quadrant; but if it does not, the line of sight is inclined to the plane of the quadrant, and must be corrected as follows. If the direct and reflected horizons, when they coin-

cide, appear higher above the middle between the wires, than what the quantity of the depression of the horizon amounts to, the object end of the telescope is inclined from the plane of the quadrant, and must be altered by the adjustment made for that purpose; but if the two horizons appear to coincide in a lower part of the field of the telescope, the object end of the telescope is inclined towards the plane of the quadrant, and must be altered by the adjustment accordingly. Repeat these operations till the two horizons appear to coincide above the middle between the two wires, by the quantity of the depression of the horizon, and the axis of the telescope will be adjusted parallel to the plane of the quadrant. In order to find the angular interval between the wires, hold the quadrant perpendicular to the horizon, as in observing altitudes; and turn about the eye tube with the wires till they are parallel to, and include, the direct fore horizon and reflected back horizon between them. Move the index from 0 along the divided arch, at the same time raising or lowering the telescope by the motion of the quadrant, till the direct horizon appears to coincide with the upper wire, and the reflected back horizon with the lower wire; the number of degrees and minutes shown on the arch, increased by double the depression of the horizon, will be the angular interval of the wires; its proportion to the depression of the horizon will be therefore known; and hence the space in the field of the telescope answering to the depression of the horizon, may be easily estimated, near enough for adjusting the axis of the telescope in the manner before mentioned. The first of the two methods here given, for adjusting the position of the telescope, will probably be found most convenient; and the greater the distance of the sun and moon is, the more nearly may the adjustment be made, because the same deviation of the axis of the telescope will cause a greater error.

The telescope should be fixed by the instrument maker, so as to command a full field of view when the instrument is placed at 90° if the instrument be an octant, or 120° if it be a sextant; because the index glass then stands more oblique with respect to the incident and reflected rays, and consequently the field of view of the telescope, as far as it depends on the index glass, will be more contracted than in any other position of the index: but if there is a fair field of view in this case, there necessarily must be so in every other position of the index.

The two parallel wires will be very useful on many occasions, as well in the fore as the back observation. In taking the altitude of the sun, moon, or star, direct the sight towards the part of the horizon underneath, or opposite to the object, according as you intend to observe by the fore or back observation, and hold the quadrant that the wires may constantly appear perpendicular to the horizon, and move the index till you see the object come down towards the horizon in the fore observation, or up to it in the back observation, and turn the instrument in order to bring the object between the wires; then move the

index till the sun or moon's limb, or the star touch the horizon. The nearer the object is brought to an imaginary line in the middle between the wires, (it is indifferent what part of the line it is brought to), and the truer the wires are kept perpendicular to the horizon, the more exact will the observation be. In the fore observation, the object appears in its real position; but in the back observation, the object being brought through the zenith to the horizon, the real upper limb will appear the lowest; and the contrary. Either limb of the sun may be used in either observation; but it will be most convenient in general to make the sun appear against the sky, and not against the sea; and then the objects appearing inverted through the telescope, the sun will appear lowest, and the horizon highest. The observed altitude is to be corrected for dip, refraction, and the sun's semidiameter, as usual.

In taking the distance of the nearest limbs of the sun and moon, whether by the fore or back observation, having first set the index to the distance nearly, by the help of the nautical almanac, and brought the moon to appear any where on or near the diameter of the field of view of the telescope, which bisects the interval between the wires, give a sweep with the quadrant, and the sun and moon will pass by one another; if in this motion the nearest limbs, at their nearest approach, just come to touch one another, without lapping over, on or near any part of the diameter of the field of the telescope which bisects the interval between the wires, the index is rightly set: but if the nearest limbs either do not come to meet, or lap over one another, alter the index, and repeat the observation till the nearest limbs come to touch one another properly. This method of observing will be found much more easy and expeditious than without the wires, since in that case it would be necessary to make the limbs touch very near the centre of the telescope, but here it is only necessary to make them do so any where on or near the diameter of the field of the telescope which bisects the interval between the two wires. The same method may be used in taking the moon's distance from a fixed star.

It may not be amiss here to make some remarks on the rules that have been usually given for observing the sun's altitude, both with the fore and back observation, which have all been defective, and to point out the proper directions to be followed, when a telescope is not used with two parallel wires to direct the quadrant perpendicular to the horizon, and to show the principles on which these directions are founded.

Observers are commonly told, that in making the fore observation, they should move the index to bring the sun down to the part of the horizon directly beneath him, and turn the quadrant about upon the axis of vision; and when the sun touches the horizon at the lowest part of the arch described by him, the quadrant will show the altitude above the visible horizon. I allow that this rule

would be true, if a person could by sight certainly know the part of the horizon exactly beneath the sun; but, as this is impossible, the precept is incomplete. Moreover, in taking the sun's altitude in or near the zenith, this rule entirely fails, and the best observers advise to hold the quadrant vertical, and turn one's-self about upon the heel, stopping when the sun glides along the horizon without cutting it: and it is certain that this is a good rule in this case, and capable with care of answering the intended purpose. We have thus two rules for the same thing, which is a proof that neither of them is a universal one, or sufficient in all cases alone.

In taking the observation, observers have been advised either to turn the quadrant about upon the axis of vision, or, holding the quadrant upright, to turn themselves about upon the heel, indifferently. The true state of the case is this; that, in taking the sun's altitude, whether by the fore or back observation, these two methods must be combined together; that is to say, the observer must turn the quadrant on the axis of vision, and at the same time turn himself about on his heel, so as to keep the sun always in that part of the horizon glass which is at the same distance as the eye from the plane of the quadrant: for, unless the caution of observing the objects in the proper part of the horizon glass be attended to, it is evident that the angles measured cannot be true ones. In this way the reflected sun will describe an arch of a parallel circle round the true sun, whose convex side will be downwards in the fore observation, and upwards in the back observation, and, consequently, when, by moving the index, the lowest point of the arch in the fore observation, or the uppermost point of the arch in the back observation, is made to touch the horizon, the quadrant will stand in a vertical plane, and the altitude above the visible horizon will be properly observed.

The reason of these operations may be thus explained; the image of the sun being always kept in the axis of vision, the index will always show on the quadrant the distance between the sun and any object seen directly which its image appears to touch; therefore, as long as the index remains unmoved, the image of the sun will describe an arch every where equidistant from the sun in the heavens, and consequently a parallel circle about the sun, as a pole; such a translation of the sun's image can only be produced by the quadrant being turned about on a line drawn from the eye to the sun, as an axis; a motion of rotation on this line may be resolved into two, one on the axis of vision, and the other on a line on the quadrant perpendicular to the axis of vision; and consequently a proper combination of these two motions will keep the image of the sun constantly in the axis of vision, and cause both jointly to run over a parallel circle about the sun in the heavens; but when the quadrant is vertical, a line on it perpendicular to the axis of vision becomes a vertical axis; and, as a small

motion of the quadrant is all that is wanted, it will never differ much in practice from a vertical axis; therefore the observer, by properly combining and proportioning two motions, one of the quadrant on the axis of vision, and the other of himself on his heel, keeping himself upright (which gives the quadrant a motion on a vertical axis) will cause the image of the sun to describe a small arch of a parallel circle about the sun in the heavens, without departing considerably from the axis of vision.

If it should be asked, why the observer should be directed to perform two motions, rather than the single one equivalent to them, on a line drawn from the eye to the sun, as an axis? I answer, that we are not capable, while looking towards the horizon, of judging how to turn the quadrant about on the elevated line going to the sun as an axis, by any other means, than by combining the two motions abovementioned, so as to keep the sun's image always in the proper part of the horizon glass. When the sun is near the horizon, the line going from the eye to the sun will not be far removed from the axis of vision; and consequently the principal motion of the quadrant will be performed on the axis of vision, and the part of the motion made on the vertical axis will be but small. On the contrary, when the sun is near the zenith, the line going to the sun is not far removed from a vertical line, and consequently the principal motion of the quadrant will be performed on a vertical axis, by the observer's turning himself about, and the part of the motion made on the axis of vision will be but small. In intermediate altitudes of the sun, the motions of the quadrant, on the axis of vision and on a vertical axis, will be more equally divided. Hence appears the reason of the method used by the best observers, in taking the sun's altitude, when near the zenith, by holding the quadrant vertical and turning about on the heel, and the defects of the rules that have been commonly given for observing altitudes in other cases.

As it may conduce to the setting this matter in a still clearer light, I shall here describe in order the several motions that will be given to the reflected image, by turning the quadrant about on the axis of vision, a vertical axis, or the line drawn from the eye to the sun, successively. 1. If the quadrant is turned about on the axis of vision, the same being directed to the point of the horizon exactly beneath or opposite the sun, the image of the sun will move from right to left, or from left to right, across the horizon glass, the same way as the arch of the quadrant is carried, both in the fore and back observations, with a velocity, which is to the angular velocity of the quadrant, as the sine of the sun's altitude to the radius, describing an arch convex downwards in both cases; and when the motion of the sun in this arch is parallel to the horizon, the quadrant is held truly perpendicular to the horizon, and consequently in a proper position for taking the sun's altitude. But if the axis of vision be directed to, and turned round, a point in

the horizon beside the vertical circle passing through the sun, the sun's image, when its motion is parallel to the horizon, will be neither in the axis of vision nor the sun's vertical, but between both; at the same time the plane of the quadrant will not be vertical, and the altitude found by bringing the sun's image to touch the horizon, will not be the true altitude.

2. If the quadrant be held perpendicular to the horizon, and turned about on a vertical axis, or one nearly so, the sun will describe an arch convex downwards in the fore observation, and upwards in the back observation, the motion of the sun being the same way as the axis of vision is carried in both cases, and being to the angular motion of the quadrant, as the verse-sine of the sun's altitude to radius in the fore observation, but as the verse-sine of the supplement of the sun's altitude to 180°, to the radius, in the back observation. The sun therefore will move slower than the axis of vision in the fore observation, and consequently will be left behind, with respect to the axis of vision, or seem to move backwards; and the sun will move quicker than the axis of vision in the back observation, or will seem to get before it. When the motion of the sun in this arch is parallel to the horizon, the plane of the quadrant coincides with the vertical circle passing through the sun, and consequently the quadrant is in a proper position for taking the sun's altitude. But if the quadrant be held a little deviating from the perpendicular position to the horizon, and turned about on an axis, either vertical or nearly so, the arch described by the sun apparently will cut the horizon, but will never move parallel to it, and consequently the quadrant will not be brought into a proper position for observing the sun's altitude.

3. If the quadrant be turned on the line going to the sun, as an axis, the reflected sun will be kept constantly in the axis of vision, and will describe an arch of a parallel circle about the real sun, with a velocity which is to the angular motion of the quadrant, as the sine of the sun's altitude is to the radius; and when the motion of the reflected sun is parallel to the horizon, the quadrant is vertical.

Hence naturally arise the 3 methods of taking an altitude, which have been mentioned before. In the first, the axis of vision is supposed always directed to one and the same part of the horizon, namely that which is in the sun's vertical. In the 2d, the observer is required to hold the quadrant truly vertical, and to turn himself on a vertical axis; but it is evident neither of these motions can be accurately performed. In the 3d method, the observer is only required to move both himself and the quadrant, so as to keep the sun always in or near the axis of vision, which may be performed very well, because the axis of vision is a visible and certain direction for it. One exception, however, should be made to this general rule, namely, in taking the sun's altitude when very low, by the

back observation; in which case it will be best to use the 2d method, or else to hold the quadrant perpendicular by judgment; which will be much facilitated by using a telescope containing wires in its focus parallel to the plane of the quadrant, as described before: for, in this case, the perpendicular position of the quadrant cannot be attained so near, by the method of turning the quadrant on a line going to the sun as an axis, as it can by the other method.

It remains to treat of the errors which may arise from a defect of parallelism in the two surfaces of the index glass, and to point out the means of obviating them in the celestial observations. It is well known, that if a pencil of parallel rays fall on a glass, whose two surfaces are inclined to each other, and some of the rays be reflected at the fore surface, and others passing into the glass and suffering a reflection at the back surface, and two refractions at the fore surface, emerge again from the glass; these latter rays will not be parallel to those reflected at the fore surface, as they would have been if the surfaces of the glass had been parallel, but will be inclined to the same. I find that the angle of their mutual inclination, which may be called the deviation of the rays reflected from the back surface, will be to double the inclination of the surfaces of the glass, which is here supposed to be but small, as the tangent of the angle of incidence out of air into glass, is to the tangent of the angle of refraction. Hence, in rays falling near the perpendicular, the deviation will be about 3 times the inclination of the surfaces; and if the angles of incidence be 50°, 60°, 70°, 80°, or 85°, the deviations of the reflected rays will be about 4, 5, 7, 13, or 26 times the inclination of the surfaces respectively. Had the deviation been the same at all incidences of the rays on the index glass, no error would have been produced in the observation; because the course of the ray would have been equally affected in the adjustment of the instrument, as in the observation. But, from what has been just laid down, this is far from being the case, the deviation increasing according to the obliquity with which the rays fall on the index glass; so that in very oblique incidences of the rays, such as happen in measuring a large angle by the fore observation, or a small angle by the back observation, the least defect in the parallelism of the planes of the two surfaces of the index glass, may produce a sensible error in the observation.

What is here said, only takes place in the fullest extent, if the thickest or thinnest edge of the index glass, or, to express the same thing in other words, the common section of the planes of the surfaces of the index glass, stand perpendicular to the plane of the quadrant; but, if the common section of the plane be inclined to the plane of the quadrant, the error arising from the defect of the parallelism of the surfaces, will be lessened, in the proportion of the sine of the inclination to the radius; so that at last, when the common section becomes parallel to the plane of the quadrant, the error entirely vanishes. For

this reason, Mr. Hadley very properly directed the thickest and thinnest edges of the index glass to be placed parallel to the plane of the quadrant. But, as it may well be questioned whether this care is always taken by the instrument maker, and it cannot be supposed that the glasses can be ground perfect parallel planes, it would certainly be an advantage acquired to the instrument, could the error arising from a want of parallelism of the planes be removed, in whatever position the common section of the planes should be placed, with respect to the plane of the quadrant. This will be effected for celestial observations, if the upper part of the index glass be left unsilvered on the back, and made rough and blacked, the lower part of the glass being silvered as usual, which must be covered whenever any celestial observations are made. Then, if the telescope be sufficiently raised above the quadrant, it is evident that the observations will be made by the rays reflected from the fore surface of the upper part of the index glass, and consequently, if the quadrant be adjusted by making use of the same part of the index glass, the observations will be true, whether the two surfaces of the index glass are parallel planes or not. The sun or moon may be thus observed by reflection from the unsilvered parts of the index glass and horizon glass, so that a paler darkening glass will suffice, and they will appear much distincter than from an index glass wholly silvered with a deeper darkening glass; for though the surfaces of a glass may be parallel, yet there always arises some little confusion from the double reflection. Neither will the moon appear too weak by 2 unsilvered reflections, even when her crescent is very small, unless she should be hazy or clouded; and then the light may be increased by lowering the telescope, so as to take in part of the silvered reflection of the index glass, which in this case must be uncovered: the same is also to be understood with respect to the sun, should his light be too much weakened by haziness or thin clouds. The horizon glasses should be adjusted, or the error of adjustment found by the sun or moon; the first will be in general the best object for the purpose; and, as the sun or moon seen directly through the unsilvered part of the horizon glass, will be much brighter than the image of the same seen by 2 unsilvered reflections, it must be weakened by a darkening glass placed beyond the horizon glass, the reflected image being further weakened, if necessary, by a paler darkening glass placed, in the usual manner, between the index glass and the horizon glass.

If a quadrant was designed principally for taking the distance of the moon from the sun and fixed stars, and was not wanted for observing terrestrial angles, it would be the best way to have none of the glasses silvered, but to leave the horizon glasses entirely transparent, and to put a red glass for an index glass of the same matter with the darkening glasses, which would reflect light from the fore surface only.

The sun's altitude might also be observed with this instrument, either by the fore or back observation; and the altitude of the moon might be taken with it in the night. But the altitudes of stars could not be observed with it, nor the moon's altitude in the day time, which would however be no great inconvenience, as these observations might be well enough supplied by common quadrants.

The following rules for the size of the glasses and the silvering them, and the height of the telescope, may be of use. The index glass and 2 horizon glasses, should be all of equal height, and even with one another in height both at top and bottom. The telescope should be moveable parallel to itself, nearer to or farther from the plane of the quadrant, and the range of its motion should be such, that its axis, when at the lowest station, should point about $\frac{1}{10}$ of an inch lower than the top of the silvering of the horizon glasses, and when at the highest station should point to the height of the middle of the unsilvered part of the index-glass. The height of the glasses, and the quantity of parts silvered and parts unsilvered, should vary according to the aperture of the object glass, as in the following table; where the first column of figures shows the dimensions, in parts of an inch, answering to an aperture of the object glass of $\frac{3}{10}$ of an inch in diameter; the 2d column, what answer to an aperture of the object glass of $\frac{4}{10}$ of an inch in diameter; and the 3d, what are suitable to an aperture of the object glass of $\frac{5}{10}$ of an inch in diameter.

	Parts of an inch		
Diameter of aperture of object glass	.30	0.40	0.50
Height of glasses	.90	1.13	1.37
Height of silvered part of index glass	.50	0.63	0.77
Height of unsilvered part of ditto	.40	0.50	0.60
Height of silvered part of horizon glasses	.25	0.33	0.42
Height of unsilvered part of ditto	.65	0.80	0.95

If the telescope has a common object glass, the first aperture of $\frac{3}{10}$ of an inch will be most convenient; but if it has an achromatic object glass, one of the other apertures, of $\frac{4}{10}$ or $\frac{5}{10}$ of an inch, will be most proper. The field of view of the telescope should be 5 or 6 degrees, and the objects should be rendered as distinct as possible throughout the whole field, by applying 2 eye-glasses to the telescope. The breadth of the glasses should be determined as usual, according to the obliquity with which the rays fall on them, and the aperture of the object glass.

I shall conclude this paper with some easy rules, for finding the apparent angular distance between any 2 near land objects, by the Hadley's quadrant. To find the angular distance between 2 near objects by the fore observation: adjust the fore horizon glass by the object intended to be taken as the direct object; and the angle measured by the fore observation on the arch of the quadrant be-

tween this object, and any other object seen by reflection, will be the true angle between them, as seen from the centre of the index glass. But if the quadrant be already well adjusted by a distant object, and you do not chuse to alter it by adjusting it by a near one, move the index, and bring the image of the near direct object to coincide with the same seen directly, and the number of minutes by which 0 of the index stands to the right hand of 0 of the quadrant, on the arch of the excess, is the correction, which added to the angle measured by the arch of the quadrant, between this direct object and any other object seen by reflection, will give the true angular distance between them, reduced to the centre of the index glass.

To find the angular Distance between 2 near Objects by the back observation.

It is supposed that the horizon glass is truly adjusted; if it is not, let it be so. Observe the distance of the objects by the back observation, and take the supplement of the degrees and minutes standing on the arch of 180 degrees, which call the instrumental angular distance of the objects; this is to be corrected as follows. Keep the centre of the quadrant, or index glass, in the same place as it had in the foregoing observation, and observe the distance between the near object, which has been just taken as the direct object, and some distant object, twice; by making both objects to be the direct and reflected ones alternately, holding the divided arch upwards in one case and downwards in the other, still preserving the place of the centre of the quadrant. The difference of these two observations will be the correction, which added to the instrumental angular distance, found as above in the first observation, between the first object and any other object seen by reflection, will give the true angular distance between them, reduced to the centre of the index glass.

But if you should happen to be in a place where you cannot command a convenient distant object, the following method may be used. The back horizon glass being adjusted, find the instrumental angular distance between the objects; this is to be corrected by means of the following operations. Set up a mark at any convenient distance opposite, or nearly so, to the object which has been taken as the direct object, move the index of the quadrant, and bring the image of the mark to coincide with the direct object, and read off the degrees and minutes standing on the arch of the quadrant, which subtract from 180 degrees, if 0 of the index falls on the quadrantal arch; but add to 180 degrees, if it falls on the arch of excess; and you will have the instrumental angular distance of the object and mark. Invert the plane of the quadrant, taking care at the same time not to change the place of its centre, and looking at the same direct object as before, move the index of the quadrant, and bring the image of the mark to coincide again with the direct object, and read off the degrees and minutes stand-

ing on the arch, and thence also find the instrumental angular distance of the object and mark. Take the sum of this and the former instrumental angular distance; half of its difference from 360° will be the correction, which added to the instrumental angular distance first found, between the same direct object and the other object seen by reflection, will give the true angular distance between them, reduced to the centre of the index glass.

It is to be observed, that if the mark be set up at the same distance from the quadrant as the direct object is, there will be no occasion to invert the plane of the quadrant, but the observer need only make the image of the mark coincide with the direct object, then turn himself half round, and now taking the mark for the direct object, cause the image of the former direct object to coincide with the mark, the divided arch of the quadrant being kept upwards, and the place of the centre of the quadrant remaining also the same in both cases: half the difference of the sum of the two instrumental angles from 360 degrees, will be the correction of the adjustment as before. Should only one of the objects be near, and the other remote, viz. half a mile distant or more, let the distant object be taken for the direct one, and the near object for the reflected one; then the true distance of the objects, as seen from the centre of the index glass, will be obtained without requiring any correction, whether it be the back or fore observation that is made use of; only observing, as usual, to take the supplement, of what is shown on the arch, to 180 degrees, in the back observation.

XV. Account of the Irruption of Solway Moss, on Dec. 16, 1772. *By Mr. J. Walker, of Moffat.* p. 123.

It is not surprizing that this irruption has every where attracted the attention of the public; for though the cause of it is obvious, yet the alteration that it has produced on the face of the earth seems to be greater than any we have known in Britain, from natural causes, since the destruction of Earl Goodwin's estate. It happened on the 16th of December, when there fell such a deluge of rain, over all the north of England, as has not been known, for at least 200 years. There was a very great flood at Moffat, but Mr. W. thinks he has seen one or two greater, and certainly it was not so extraordinary here, as farther south.

The Solway flow contains 1300 acres of very deep and tender moss, which before this accident were impassable, even in summer, to a foot passenger. It was mostly of the quag kind, which is a sort of moss covered at top with a turf of heath and coarse aquatic grasses; but so soft and watery below, that if a pole is once thrust through the turf, it can easily be pushed, though perhaps 15 or 20 feet long, to the bottom. If a person ventures on one of these quags, it bends in waves under his feet; and if the surface breaks, he is in danger of sinking to

the bottom.* The surface of the flow was, at different places, between 50 and 80 feet higher than the fine fertile plain, between it and the river Esk. About the middle of the flow were the deepest quags, and there the moss was elevated higher above the plain, than in any part of the neighbourhood. From this, to the farm called the Gap, upon the plain there was a broad gully, though not very deep, through which a brook used to run. The moss, being quite overcharged with the flood, burst at these quags, about 11 o'clock at night, and finding a descent at hand, poured its contents through the gully into the plain. It surprized the inhabitants of 12 towns in their beds.† Nobody was lost, but many of the people saved their lives with great difficulty. Next morning, 35 families were found dispossessed, with the loss of most of their corn and some cattle.‡ Some of the houses were near totally covered, and others of them he saw standing in the moss, up to the thatch, the side walls being about 8 feet high.

In the morning, above 200 acres were entirely overwhelmed; and this body of moss and water, which was of such a consistency, as to move freely, continued to spread itself on all hands for several days. It was come to a stop, when Mr. W. saw it, and had covered 303 acres, as he was informed by a gentleman, who had looked over the plans of the grounds, with Mr. Graham the proprietor: but every fall of rain sets it again in motion, and it has now overspread above 400 acres. At the farthest part it had run within a musket shot of the post road leading from Moffat to Carlisle, when he saw it, but it is since flowed over the road, and reached the Esk. This river, which was one of the clearest in the world, is now rendered black as ink, by the mixture of the moss, and no salmon has since entered into it. A farmer also told him, that on removing the moss, to get at a well which it had covered, they found all the earth-worms lying dead on the surface of the ground. The land that is covered was all inclosed with hedges, bore excellent crops of wheat and turnips, and rented from 11 to 14 shillings, besides the taxes and tythes, which amounted to 4 shillings per acre.

* The surface was always so much of a quagmire, that in most places it was hardly safe for any thing heavier than a sportsman to venture on it, even in the driest summers. A great number of Scotchmen, in the army commanded by Oliver Sinclair, in the time of Henry 8th, lost their lives in it; and it is said that some people digging peats on it, met with the skeleton of a trooper and his horse in complete armour, not many years ago.—Orig.

† Those who were nearest the place of bursting were alarmed with the unusual noise it made; others not till it had entered their houses, or even, as was the case with some, not till they found it in their beds.—Orig.

‡ The case of a cow seems singular enough to deserve a particular mention. She was the only one of 8 in the same cow-house, that was saved, after having stood 60 hours up to the neck in mud and water. When she was got out, she did not refuse to eat, but water she would not taste, nor could even look at, without showing manifest signs of horror. She is now reconciled to it, and likely to recover.—Orig.

Mr. W. endeavoured to guess at the depth of the moss on the plain, by a large thorn, which stands in the middle of it, and which is buried to above the division of the branches. The farmers told him, that it stood upon a rising, more than 6 feet above the general level of the plain: and that it was upwards of 9 feet high, of clear stem. By this account, great part of the plain must be covered 15 feet deep with the moss: and near the farm called Gap, there were some considerable hollows, where they think the moss, at present, lies full 30 feet deep. The tallest hedges on the land are all covered over the top. The houses are not so much buried, because they stood mostly on the higher parts of the fields; and towards the extremities of the moss, he observed it, in many places, not above 3 or 4 feet deep, owing likewise to the rising of the ground.

The gut through which the whole of the moss flowed that covered the plain, is only about 50 yards wide, and the gully near a quarter of a measured mile long. The brook being stopped up by the moss, has now formed a lake.

About 400 acres of the flow, next the place of its evacuation, appear to have sunk from 5 to 25 feet: and this subsidence has occasioned great fissures on those parts of the moss which refused to sink. These fissures are from 4 to 8 feet wide, and as much in depth. The surface of the flow, consisting of heath and coarse grass, was torn away in large pieces, which still lie on the surface of the new moss, some of them from 20 to 50 feet long. But the greater part of the surface of the flow remained, and only subsided; the moss, rendered thin by the flood, running away from under it.

Looking over the Solway moss, at the village of Longtown, where there is a bridge on the Esk, they formerly saw only the tops of the trees at Gratney, a house of the Marquis of Annandale's, 4 miles distant; but now they see them almost to the ground. And looking over it, in another direction, they now see two farm-towns of Sir Wm. Maxwel's, which were not before visible. So that the ridge of the flow or moss seems to have subsided about 25 feet.

XVI. On a New Species of Oak. By John Zephaniah Holwel, Esq., F.R.S. Dated Exeter, Feb. 24, 1772. p. 128.

About 7 years back, Mr. Lucombe, of St. Thomas, sowed a parcel of acorns, saved from a tree of his own growth, of the iron or wainscot species: when they came up, he observed one among them that kept its leaves throughout the winter: struck with the phenomenon, he cherished, and paid particular attention to it, and propagated, by grafting, some thousands from it, which Mr. H. had the pleasure of seeing, 8 days ago, in high flourishing beauty and verdure, notwithstanding the severity of the winter. Its growth is straight, and handsome as a fir, its leaves evergreen, and the wood is thought, by the best judges, in hardness and strength to exceed all other oak. It makes but one shoot in the

year, viz. in May, and continues growing without interruption; whereas other oaks shoot twice, namely, in May and August; but the peculiar and estimable part of its character is, the amazing quickness of its growth, which Mr. H. imagines may be attributed, in some degree at least, to its making but one shoot in the year; for he believes all trees that shoot twice, are for some time at a stand, before they make the second. Mr. H. took the dimensions of the parent tree, 7 years old, and some of the grafts; the first measured 21 feet high, and full 20 inches in the girt; a graft of 4 years old, 16 feet high, and full 14 inches in the girt; the first he grafted is 6 years old, and has outshot its parent 2 feet in height. The parent tree seems to promise its acorns soon, as it blossoms, and forms its foot-stalk strong, and the cup on the foot-stalk with the appearance of the acorn, which, with a little more age, will swell to perfection. This oak is distinguished, in this country, by the title of the Lucombe oak; its shoots in general are from 4 to 5 feet every year, so that it will, in 30 or 40 years, out grow in altitude and girt the common oak at a hundred. Several gentlemen round this neighbourhood, and in Cornwall and Somersetshire, have planted them, and they are found to flourish in all soils.

XVII. An Account of the Death of a Person, occasioned by Lightning in the Chapel in Tottenham-Court-Road, and its Effects on the Building; as observed by Mr. Wm. Henly, Mr. Edward Nairne, and Mr. Wm. Jones. The Account written by Mr. Henly. Dated March 24, 1772. p. 131.

On Sunday last, exactly at 4 o'clock, P. M. part of a building erected by the late Rev. Mr. Whitfield, in Tottenham-court-road, commonly called the chapel or tabernacle, was struck by a flash of lightning. This part was an addition afterwards made to the original structure, but was greatly inferior to it in height. On its summit stood an ornament representing a pine apple carved in wood, which consisted of two pieces; the uppermost being connected with the lower by means of several iron spikes. It was supported by a strong plinth of wood covered with lead lapped over the edges and corners of its top, and there secured by large iron nails. This lead work was connected with that which covered the hips, and made a regular communication of metal, to the bottom of the slating, where it united with a leaden gutter which extended quite round the building. In this gutter was erected a small lantern, in which hung the bell of the clock. A little pipe of lead was soldered to, and extended perpendicularly a few inches above the surface of the gutter; through this pipe went a small iron wire consisting of many long links, connected with the tail of the hammer; passing thence within a few inches of the striking rod of the clock, to which it was tied by a strong hempen string 6 inches or more in length. The lightning first struck the pine apple, the upper part of which it shivered into very small frag-

ments, and threw them in all directions from the place, and melted off the end of one of the spikes. It left a smoky track on the under part of it, and then struck the edge of the lead on the plinth, which it melted in two places, quite through its substance. A little below these was a third spot; this was melted in a very regular and curious concave, about an 8th of an inch diameter, at the surface, with a small perforation at the bottom, through which might have been introduced one of the finest sort of sewing needles. The whole figure somewhat resembled a small funnel.* It passed thence by a regular communication of metal, till it reached the wire of the clock hammer before spoken of, melting it about half through its diameter, which, in this place, was less than the 12th part of an inch. The edge of the lead pipe, from which it leaped to the wire, was also much melted. The wire was melted at every juncture of the links; the packthread at the bottom was but little injured, but the electric matter leaped through a few inches of air to the striking rod of the clock, in which, near the end, it melted a large spot, whence it was conducted by the work of the clock to the upper part of the pendulum, in the axis of which it melted another large spot, and descended by the rod passing over the ball, which it melted in a most remarkable manner in 6 or 7 places (perhaps on the ball it might accumulate, and for want of a proper conveyance break out in different parts of it) and quitted it at the bottom of the nut, which it melted in 3 places. Here the electricity leaped through 8 inches of air, or passed in conductors of the worst kind, dry brick and wood, with a considerable cavity between them, till it reached the frame of a window, over the doors, where it broke the ceiling, and burnt the wood to a coal. Here it met with the point of a nail, driven upward into the window frame as a security to the centre bar. The point of this nail is melted off full half an inch; it was also melted in two large spots on the opposite sides near the head. Mr. Jones drew it from the bar, &c. This gentleman also took a sketch of the window, and an outline of the parts affected of the building. The lightning passed down the aforementioned bar, and by a bent iron, in contact with both, into another bar, whose point, which was greatly melted, came much nearer the upper bolt of the door. The lead-work, from the point of the bar, was melted, and a board nearly in contact with the staple of the bolt much blacked by the passing of the electricity. Here it struck the upper edge of the staple, which projected a little above the top of the bolt, melted it in a most extraordinary manner; the spot, and indeed several others, having run into a kind of spiral form, which is raised considerably. This effect was first observed by Mr. Nairne. When it quitted this bolt, it struck on a semicircular handle of iron (first tearing out a large piece from the door), the upper part of which has 3

* Quere, is not this a token of the stroke's being from the clouds downwards?—Orig.

melted spots, besides a single one at the upper edge of it. But, in quitting it, the electricity melted only one spot at the lower edge,* which, as Mr. Bell (a gentleman who was with us) observed, was a criterion by which to judge of the direction of the fluid. To the left of this door, at the distance of 11 feet 4 inches, came down a leaden pipe, which terminated at the ceiling, and there just entered a pitched trunk of fir, which indeed was the case with every leaden pipe about the building. Here the lightning exploded, rending the trunk, and doing other slight damage in and about a window, to which it was attracted by an interrupted and irregular communication of metal. Had this pipe of lead been continued to the bottom of the building, and thence conveyed into the earth, in the manner directed by Dr. Franklin, Mr. H. had no doubt but the whole contents of the explosions would have passed this way, have been conducted with perfect safety to the building, &c. and that no other part of it would have been at all affected. As the effects of this stroke so exactly correspond with those many times before observed by Dr. Franklin, Mr. H. thinks we shall hardly ever meet with a greater proof of the utility of his metallic conductors; and cannot help expressing a sincere wish, that builders, and persons engaged in the erection of public edifices, &c. might be prevailed with to make a regular communication of metal, from the top of such buildings to a considerable depth into the earth, and of such a diameter and kind, as may be sufficient to secure both the buildings, and the lives of those who may happen to be in them. The poor man destroyed by this accident, was sitting at the time on a short ladder, which lay horizontally on the pavement, with his back against the door. The lightning flew from the middle bolt, and struck him on and under his left ear, entered his neck, making a wound half an inch long, raised in a bur and burnt, passed down his back, which it turned black as ink, down his left arm, melting the stud in his shirt sleeve; the stone in which, as well as the silver, seems to be a little affected. Hence it flew into his body, which it burnt in a hard spot, resembling scorched leather, passing through it into his right leg, and breaking out a little above the ancle; making a large wound, and another bur, burnt as before, with two others smaller a little below it, and some still smaller in his feet. His clothes and hair were much burnt, but his stock, shoe, and knee-buckles, the metal buttons on his coat and waistcoat, a shilling, which he had in the left pocket of his breeches, and the metal clasps of a Common Prayer-Book, in his coat pocket, were all uninjured.† His death was truly instantaneous.

* Quere, is not this effect somewhat analogous to Mr. Lullin's electrical experiment with a card? —Orig.

† The corpse, after lying 2 or 3 days on a table, seemed not more disposed to putrefaction, than bodies at that time generally are, which die a natural death.—Orig.

XVIII. Observations on Atmospherical Electricity; in Regard to Fogs, Mists, &c. By Thomas Ronayne, Esq. With some Remarks by Mr. Wm. Henley. p. 137.

Some years ago Mr. R. discovered, by Mr. Canton's electrometer, described in the Phil. Trans., vol. 48, that the air of Ireland is, during the winter season, in almost a constant state of positive electricity; which however is so weak, that in order to observe it satisfactorily, he always found it necessary to have the cork balls suspended from threads of a middling fineness, 6 or 7 inches in length, quite straight, and to avoid as much as possible any interruption from the wind.

He also had frequent recourse to the following contrivance, by which he was enabled, within doors, to pursue his inquiries with greater accuracy and advantage: having procured a slender tapering piece of wood, about 5 feet long, to the smaller end of which an electrometer was affixed, by means of a small hook; he placed it out from an open garret window, and fastened the other end with a small hasp to one of the jambs: he had also at hand another piece of wood, in the ends of which, a small glass tube and a stick of sealing wax had been inserted. Either of these was occasionally excited, and applied near the cork balls, to determine more precisely the kind of electricity with which they might happen to be affected, and he was always careful in making his experiments on that side of the house where the wind had least power.

He found the air in winter, at a proper distance from buildings, trees, masts of ships, &c. very sensibly electrified, during a frosty or foggy state of the weather; and in mists too, but in a less degree: he also discovered small signs of it in calm and cloudy weather. The air in summer never showed any sign of electricity, except when a fog happened in the cool of the evening, or at night; in which case he always discovered manifest marks of electricity, sensibly weaker than those observed in winter-fogs, but precisely of the same kind, that is, positive. He often examined the state of the air at the time of an aurora borealis, and could not discover any indication of electricity, except when a fog had appeared at the same time; in which case, the electricity has been, in every respect, the same as that of a fog at any other time. Once indeed, during an aurora borealis on a remarkable serene night, he discovered some signs of a very weak positive electricity.

As the electricity of the air is generally positive, he never knew an exception but one, which presented itself during a fog on a winter day, that proved uncommonly warm), is it not reasonable to believe, that cold electrifies the atmosphere positively? and if so, may not one be led to imagine, that heat electrifies it negatively? But this he only offers as a conjecture, not being able to advance any thing decisive on the subject, and knowing that one sort of electricity may often be productive of the other, as is plain from Dr. Franklin's experi-

ments. If cold electrifies the air positively in this climate, which seems extremely probable, may it not electrify it negatively at and about the place of our antipodes? Does not a consideration of the effects discovered in the tourmalin favour this surmise?

The electricity of the air, in frosty, foggy, or misty weather, is not strong enough to yield any spark, even by insulating a sharp pointed wire in it, which however attracts very light bodies at a small distance; while on the contrary that of the clouds generally affords considerably strong sparks. When a fog becomes very thick, the cork balls approach; but when it returns to its former state, they open again at their first distance; and when it rained in foggy weather, the balls closed, and opened again on the fog's appearance anew, after the rain had ceased: there is however a certain degree of density necessary in a fog, in order that the balls might exert their greatest divergency.

Most, if not all, fogs partake of a smell much like that of an excited glass tube, and indeed so does the common air very frequently. As fogs sometimes appear in a very moist state of the air, Mr. R. was for some time at a loss to account on what principle they could retain their electricity: but having at length remarked that electrified bodies, insulated with sealing wax, preserved their electricity for a time in very damp air, he concluded that moisture is but a very slow conductor.

Having, on the contrary, observed that bodies, insulated with dried silk, had lost their electricity in a very short time, he attempted to render it a non-conductor, by having varnished it over with oil of turpentine, balsam of sulphur, and such like, but did not succeed; for silks so treated soon became a conductor, and increased considerably in weight, if the air happened not to be very dry;* so much indeed that ordinary silk, from its power of absorbing moisture from the air, may well serve as an occasional hygrometer, either by being put into a balance, or having an electrified body insulated with it.

When the density of fogs, floating near the earth, increases considerably, the balls always approach; but when they are situated high in air, the reverse generally happens. Mr. R. had an opportunity of remarking a struggle between breezes from the N.W. and S.E. at the same time, in which the one seemed sometimes to prevail, and afterwards the other. This contention was succeeded by a smoky haziness, which, like a fog, occasioned the balls to open: as the haziness † thickened, they opened wider, and still wider when it dissolved into rain; but their repelling power became greatest in proportion as the drops increased.

* Even glass attracts moisture to its surface, which makes it a conductor of electricity; and consequently not so convenient as sealing wax.—Orig.

† An electrical body, when contracted in its dimension, will have its electricity increased, as ap-

The electrometer placed out from a garret window has been frequently useful, in determining the nature of an approaching cloud, whose electricity, though generally strong, was for the most part uncertain, having been sometimes positive, and at other times negative. But as the wind or rain were frequent impediments to the accuracy of the experiments, the following methods of making observations with success, under shelter, occurred. Mr. R. has sometimes stood in an upper room, on a cake of wax, holding in his right hand, out at the window, a long slender piece of wood, round which a wire projecting a few inches had been twisted, and in his left hand an electrometer; an assistant had excited glass or wax in readiness. At other times he made use of a tapering tube of tin, 20 feet long, ending in a point; the greatest part of it stood out high in the air, and the thick end, from which an electrometer hung, was supported inside the window, sometimes with silk cords, and at other times with strong sticks of sealing wax, sustained at either end by hooks of iron wire. By either of these means he often discovered, that what seemed a single cloud, produced, in its passing over, several successive changes, from positive to negative and from negative to positive electricity, the balls coming together each time, and remaining in contact a few seconds, before they repelled each other again.

The permanence of either kind of electricity in the clouds, or the length of time in which neither can be discovered, is uncertain; sometimes the same electricity has returned, and at other times has been succeeded by the contrary; while either generally came on, and went off gradually. But changes were often made, very suddenly, by a flash of lightning, especially if the thunder-storm happened to be in the zenith. A branch of it over head, has frequently occasioned stronger electricity than he could discover, when the greatest part of the sky had been overcast; which perhaps might be accounted for from this consideration, that one kind of electricity acting alone, must exert more powerful effects than when counteracted by the other.

He once observed in a thunder-storm, during which he saw no lightning, that the balls, which hung from the tin tube, repelled and attracted each other, very rapidly, for the space of 10 or 12 seconds; at the same time Mr. Canton's electrometer, which he held at such a distance from the tube, as to have its balls opened to the distance of an inch, continued quiet in that state, and were not affected convulsively like the others. Hence he imagined, that the same kind of electricity went off, and came on, without being changed in contrarium;

pears by Dr. Franklin's curious experiment with the chain and silver can. I also have discovered from repeated trials, that a piece of flannel, silk, &c. excited, and suddenly twisted, not only struck at a greater distance than before, but sometimes emitted pencils of fire into the air. May we not hence infer why the electricity of vapour, &c. (when not in contact with the earth) increases by condensation?—Orig.

for when that circumstance happened, they were very evidently affected in the same manner. And found it more easy to discover the kind of electricity present in the tube, by approaching excited wax to the balls of an electrometer, held at a proper distance from the tube, than by applying it near the balls which hung from the tube; for they generally diverged so much, that it was very difficult to have in readiness a small tube of glass, or wax sufficiently excited to affect them.

It has sometimes happened that the balls of the tin tube, &c. perfectly at rest, have, in consequence of a flash of lightning, suddenly repelled each other, and immediately after closed. As this circumstance has frequently happened when the air was in a damp state, he sometimes imagined that the equilibrium between the earth and lower clouds had been quickly restored, on receiving the electricity of the higher ones; and at other times he supposed that it might be owing to the lateral effect of the explosion. If two or more persons, at a sufficient distance from each other, would correspond by signals, viz. a red flag for positive, and a blue one for negative electricity, we should probably obtain, in due time, more satisfactory certainty with regard to the electricity of the clouds, thunder, &c. than has hitherto been given, or is perhaps possible for any one man to acquire, without the aid of wires or chains, produced from different apparatusses, placed at different distances from each other.

Observations on the above, by Mr. Henley.—Mr. H. finds a fog (not very thick) soon after its appearance, strongly electrical. The balls open $\frac{1}{2}$ or $\frac{3}{4}$ inch, and close at the approach of excited wax, when brought within 10 inches of them; if the wax is brought within 3 or 4 inches, they diverge again: as the wax is withdrawn, they converge again, till it gets beyond the distance of its influence, when they begin to diverge again; and, as the wax is withdrawn still farther, they continue to open, in consequence of the electricity in the fog, till they reach their original distance from each other. There is very little disturbance by the wind, and the little there is, only wafts them in a small degree, but they keep separate. If they are held near the tiling, or brick-work, of a neighbouring house, they close; but begin to diverge again, at the distance of 3 or 4 feet from it; and their divergence increases, as they recede from the building, till they separate $\frac{1}{2}$ or $\frac{3}{4}$ inch, as at first.

October 3, 1771, Mr. H. tried the electricity of a thick fog, and, in at least 20 different trials, found the balls separated from $\frac{1}{4}$ to $\frac{3}{4}$ inch distance. Whenever he brought them near the building, or approached them with a stick of excited wax, they closed; and opened again, on removing it.

XIX. Observations on Different Kinds of Air. By Joseph Priestley, LL. D., F. R. S. p. 147.

This paper is reprinted in Dr. Priestley's works on different kinds of air, where it may be consulted.

XX. An Essay on the Periodical Appearing and Disappearing of certain Birds, at different Times of the Year. By the Hon. Daines Barrington, Vice Pres. R. S. p. 265.

See this paper, with some additions, in Mr. B.'s collected works.

XXII. ΚΟΣΚΙΝΟΝ ΕΡΑΤΟΣΘΕΝΟΥΣ; *or, the Sieve of Eratosthenes. Being an Account of his Method of finding all the Prime Numbers.* By the Rev. S. Horsley, F. R. S.* p. 327.

A prime number is such a one, as has no integral divisor but unity. A number which has any other integral divisor, is composite. Two or more numbers, which have no common integral divisor, besides unity, are said to be prime with respect to each other. Two or more numbers, which have any common integral divisor besides unity, are said to be composite with respect to each other.

To determine, whether several numbers proposed be prime or composite with respect to each other, is an easy problem. The solution of it is given by Euclid, in the first 3 propositions of the 7th book of the Elements, and is to be found in many common treatises of arithmetic and algebra. But to determine, concerning any number proposed, whether it be absolutely prime or composite, is a problem of much greater difficulty. It seems indeed incapable of a direct solution, by any general method: because the successive formation of the prime numbers does not seem reducible to any general law. And for the same reason, no direct method has hitherto been hit on, for constructing a table of all the prime numbers to any given limit. Eratosthenes, whose skill in every branch of the philosophy and literature of his times, rendered his name so famous among the sages of the Alexandrian school, was the inventor of an indirect method, by which such a table might be constructed, and carried to a great length, in a short time, and with little labour. This extraordinary and useful invention is at present little, if at all, known, being described only by two writers, who are seldom read, and by them but obscurely; by Nicomachus Gerasinus, a shallow writer of the 3d or 4th century, who seems to have been led into mathematical speculations, not so much by any genius for them, as by a fondness for the mysteries of the Pythagorean and Platonic philosophy; and by Boethius, whose treatise on numbers is but an abridgment of the wretched performance of Nicomachus.† I flatter myself therefore, that a succinct account of it will not be unacceptable to this learned society.

* Other tracts on the subject of prime and composite numbers, may be seen, collected with other pieces, with this learned prelate's edition of Euclid's Data, printed at Oxford in 1803.

† There are more pieces than one of this Nicomachus extant. That which I refer to is intitled Εισαγωγη Αριθμητικη.—Orig.

But before entering expressly on the subject, I must take the liberty to animadvert on a certain table, which, among other pieces ascribed to Eratosthenes, is printed at the end of the beautiful edition of Aratus published at Oxford in the year 1672, and is adorned with the title of Κοσκινον Ερατοσθενης. It contains all the odd numbers from 3 to 113 inclusive, distributed in little cells, all the divisors of every composite number being placed over it, in its proper cell, and the prime numbers are distinguished, so far as the table goes, by having no divisors placed over them. It has probably been copied either from a Greek comment on the Arithmetic of Nicomachus, preserved among the manuscripts of Mr. Selden in the Bodleian library, in which, though the manuscript is now so much decayed as to be in most places illegible, I find plain vestiges of such a table,* which might be more perfect 100 years ago, when the Oxford Aratus was published; or else, from another comment, translated from a Greek manuscript into Latin, and published in that language, by Camerarius, in which a table of the very same form occurs, extending from the number 3 to 109 inclusive. It may sufficiently skreen the editor of Aratus from censure, that he had these authorities to publish this table as the Sieve of Eratosthenes; especially as they are in some measure supported by passages of Nicomachus himself. But the Sieve of Eratosthenes was quite another thing.

The Oxford editor has annexed to his table, to explain the use of it, some detached passages, which he has selected from the text of Nicomachus, and from a comment on Nicomachus ascribed to Joannes Grammaticus. In these passages the difference between prime and composite numbers is explained, in many words indeed, but not with the greatest accuracy; and it is proposed to frame a kind of table of all the odd numbers, from 3 to any given limit, in which the composite numbers should be distinguished by certain marks.† The primes would consequently be characterised, as far as the table should be carried, by being unmarked. But, on what principles, or by what rule, such a table is to be constructed, is not at all explained. It is obvious that, in order to mark the composite numbers, it is necessary to know which are such. And, without some rule to distinguish which numbers are prime, and which are composite, independent of any table in which they shall be distinguished by marks, it is impossible to judge whether the table be true as far as it goes, or to extend it, if requisite, to a further limit. Now it was the rule by which the prime numbers and

* This manuscript seems to have contained the text of Nicomachus with Scholia in the margin. But the table evidently belongs to the Scholia, not to the text.—Orig.

† Nicomachus and Joannes Grammaticus propose that these marks should be such, as should not only distinguish the composite numbers, but likewise serve to express all the divisors of every such number. It will be shown, in a proper place, that this was no part of the original contrivance of the Sieve.—Orig.

the composite might be distinguished, not a table constructed we know not how, that was the invention of Eratosthenes, to which from its use, as well as from the nature of the operation, which proceeds, as will be shown, by a gradual extermination of the composite numbers from the arithmetical series 3, 5, 7, 9, 11, &c. infinitely continued, its author gave the name of the Sieve. I have thought it necessary to premise these remarks, to remove a prejudice, which I apprehend many may have conceived, as this beautiful and valuable edition of Aratus is in every one's hands, that this ill-contrived table, the useless work of some monk in a barbarous age, was the whole of the invention of the great Eratosthenes, and in justice to myself, that I might not be suspected of attempting to reap another's harvest.

I now proceed, to give a true account of this excellent invention; which, for its usefulness, as well as for its simplicity, I cannot but consider as one of the most precious remnants of ancient arithmetic. I shall venture to represent it according to my own ideas, not obliging myself to conform, in every particular, to the account of Nicomachus, which I am persuaded is in many circumstances erroneous. In stating the principles on which the operation of the sieve was founded, he hath added observations on certain relations of the odd numbers to one another, which are certainly his own, because they are of no importance in themselves, and are quite foreign to the purpose. Every thing of this kind I omit: and having stated what I take to have been the genuine theory of Eratosthenes's method, cleared from the adulterations of Nicomachus, I deduce from it an operation of great simplicity, which solves the problem in question with wonderful ease, and which, because it is the most simple that the theory seems to afford, I scruple not to adopt as the original operation of the sieve, though nothing like it is to be found in Nicomachus; though, on the contrary, Nicomachus, and all his commentators, would suggest an operation very different from it, and far more laborious. For the satisfaction of the curious and the learned, I have annexed a copy of so much of Nicomachus's treatise, as relates to this subject, with such corrections of the text, as it stands in the edition of Wichelius, printed at Paris ann. 1538, as the sense hath suggested to me, or I have thought proper to adopt, on the authority of a manuscript preserved among those of Archbishop Laud, in the Bodleian library; which, in this part, I have carefully collated. By comparing this with the account which I subjoin, every one will be able to judge how far I have done justice to the invention I have undertaken to explain.

PROBLEM.—*To find all the Prime Numbers.*

The number 2 is a prime number; but, except 2, no even number is prime, because every even number, except 2, is divisible by 2, and is therefore composite. Hence it follows, that all the prime numbers, except the number 2,

are included in the series of the odd numbers, in their natural order, infinitely extended; that is, in the series 3, 5, 7, 9, 11, 13, 15, 17, 19, 21, 23, 25, 27, 29, 31, 33, 35, 37, 39, 41, 43, 45, 47, 49, 51, &c.

Every number which is not prime, is a multiple of some prime number, as Euclid hath demonstrated (element 7, prop. 33). Therefore the foregoing series consists of the prime numbers, and of multiples of the primes. And the multiples, of every number in the series, follow at regular distances; by attending to which circumstance, all the multiples, that is, all the composite numbers, may be easily distinguished and exterminated.

I say, the multiples of all numbers, in the foregoing series, follow at regular distances. For between 3 and its first multiple in the series, 9, two numbers intervene, which are not multiples of 3. Between 9 and the next multiple of 3, (15), two numbers likewise intervene, which are not multiples of 3. Again, between 15 and the next multiple of 3 (21) two numbers intervene, which are not multiples of 3; and so on. Again, between 5 and its first multiple (15) four numbers intervene, which are not multiples of 5. And between 15 and the next multiple of 5 (25) four numbers intervene, which are not multiples of 5; and so on. In like manner, between every pair of the multiples of 7, as they stand in their natural order in the series, 6 numbers intervene, which are not multiples of 7. Universally, between every two multiples of any number n, as they stand in their natural order in the series $n - 1$ numbers intervene, which are not multiples of n.

Hence may be derived an operation for exterminating the composite numbers, which I take to have been the operation of the sieve, and is as follows.

The Operation of the Sieve.

Count all the terms of the series following the number 3, by threes, and expunge every 3d number. Thus all the multiples of 3 are expunged. The first uncancelled number that appears in the series, after 3, is 5. Expunge the square of 5. Count all the terms of the series, which follow the square of 5, by fives, and expunge every 5th number, if not expunged before. Thus all the multiples of 5 are expunged, which were not at first expunged, among the multiples of 3. The next uncancelled number to 5 is 7. Expunge the square of 7. Count all the terms of the series following the square of 7, by sevens,

3. 5. 7. 9. 11. 13. 15. 17. 19. 21. 23. 25. 27. 29. 31. 33. 35. 37. 39. 41. 43. 45. 47. 49. 51. 53. 55. 57. 59. 61. 63. 65. 67. 69. 71. 73. 75. 77. 79. 81. 83. 85. 87. 89. 91. 93. 95. 97. 99. 101. 103. 105. 107. 109. 111. 113. 115. 117. 119. 121. 123. 125. 127. 129. 131. 133. 135. 137. 139. 141. 143. 145. 147. 149. 151. 153. 155. 157.

and expunge every 7th number, if not expunged before. Thus all the multiples of 7 are expunged, which were not before expunged among the multiples of 3 or 5. The next uncancelled number which is now to be found in the series, after 7, is 11. Expunge the square of 11. Count all the terms of the series, which follow the square of 11, by elevens, and expunge every 11th number, if not expunged before. Thus all the multiples of 11 are expunged, which were not before expunged among the multiples of 3, 5, and 7. Continue these expunctions, till the first uncancelled number that appears, next to that whose multiples have been last expunged, is such, that its square is greater than the last and greatest number to which the series is extended. The numbers which then remain uncancelled are all the prime numbers, except the number 2, which occur in the natural progression of numbers from 1 to the limit of the series. By the limit of the series I mean the last and greatest number to which it is thought proper to extend it. Thus the prime numbers are found to any given limit.

Nicomachus proposes to make such marks over the composite numbers, as should show all the divisors of each. From this circumstance, and from the repeated intimations, both of Nicomachus, and his commentator Joannes Grammaticus,* one would be led to imagine, that the sieve of Eratosthenes was something more than its name imports, a method of sifting out the prime numbers from the indiscriminate mass of all numbers prime and composite, and that, in some way or other, it exhibited all the divisors of every composite number, and likewise showed whether two or more composite numbers were prime or composite with respect to each other. I have many reasons to think that this was not the case. I shall as briefly as possible point out some of the chief, for the matter is not so important, as to justify my troubling the society with a minute detail of them. First then, in the natural series of odd numbers, 3, 5, 7, &c. every number is a divisor of some succeeding number. Therefore if we are to have marks for all the different divisors of every composite number, we must have a different mark for every odd number. Therefore we must have as many marks, or systems of marks, as numbers; and I do not see that it would be possible to find any more compendious marks, than the common numeral characters. This being the case, it would be impracticable to carry such a table as Nicomachus proposes, and his commentators have sketched, to a sufficient length to be of use, on account of the multiplicity of the divisors of many numbers, and the confusions which this circumstance would create.† It is hardly to be supposed, that Eratosthenes could overlook this obvious difficulty,

* The comment of Joannes Grammaticus is extant in manuscript in the Savilian library at Oxford, to which I have frequent access, by the favour of the Rev. and learned Mr. Hornsby, the Savilian professor of astronomy.—Orig.

† The number 3465 hath no less than 22 different divisors.—Orig.

though Nicomachus hath not attended to it. Eratosthenes therefore could not intend the construction of such a table.

In the next place, such a table not being had, Eratosthenes could not but perceive, that the determining whether two or more numbers be prime or composite with respect to one another, is in all cases to be done more easily, by the direct method given by Euclid, than by the method of the sieve. And he could not mean to apply this method to a problem to which another was better adapted.

Lastly, Eratosthenes could not mean, that the method of the sieve should be applied to the finding of all the possible divisors of any composite number proposed, because he could not be unacquainted with a more ready way of doing this, founded on two obvious theorems, which could not be unknown to him. The theorems I mean are these: 1. If two prime numbers multiply each other, the number produced hath no divisors but the two prime factors. 2. If a prime number multiply a composite number, and likewise multiply all the divisors of that composite severally, the numbers produced by the multiplication of these divisors, will be divisors of the number produced by the first multiplication: And the number produced by the first multiplication will have no divisors, but the two factors, the divisors of the composite factor, and the numbers made by the multiplication of these divisors by the prime factor severally.

The method of finding all the divisors of any composite number, delivered by Sir Isaac Newton in the Arithmetica Universalis, and by Mr. Maclaurin in his Treatise of Algebra, may be deduced from these propositions, as every mathematician will easily perceive. This method requires indeed that the least prime divisor should be previously found; and if the least prime divisor should happen to be a large number, as it is not assignable by any general method, the investigation of it by repeated tentations may be very tedious. A table therefore of the odd numbers,* in which the composite numbers should each have its least prime divisor written over it, would be very useful. But Nicomachus's project of framing a table in which each composite number should have all its divisors written over it, is ridiculous and absurd, on account of the insuperable difficulties which would attend the execution of it.

The extracts from Nicomachus, and from Boethius, are omitted, as unnecessary to be retained here.

XXIII. On the Effects of Elder, in Preserving Growing Plants from Insects and Flies. By Mr. Christopher Gullet. p. 348.

This paper relates to the effects of elder; 1st. In preserving cabbage-plants

* A table of the odd numbers would be sufficient: for the number 2 is the least prime divisor of every even number; and it is easy, even in the largest numbers, to try whether they are divisible by 2. In our method of notation, this may always be known, by observing the last figure in the expression of the number proposed.—Orig.

from being eaten or damaged by caterpillars. 2d. In preventing blights, and their effects on fruit and other trees. 3d. In the preservation of crops of wheat from the yellows, and other destructive insects. 4th. Also in saving crops of turnips from the fly, &c. &c.

1st. Mr. G. was led to his first experiments, by considering how disagreeable and offensive to our olfactory nerves the effluvia emitted by a brush of green elder leaves are, and thence, reasoning how much more so they must be to those of a butterfly, which may be considered as being as much superior to us in delicacy as inferior in size. Accordingly he took some twigs of young elder, and with them whipped the cabbage plants well, but so gently as not to hurt them, just as the butterflies first appeared; from which time, for these two summers, though the butterflies would hover and flutter round them like gnomes or sylphs, yet he could never see one pitch, nor was there apparently a single caterpillar blown, after the plants were so whipped, though an adjoining bed was infested as usual.

2d. Reflecting on the effects abovementioned, and considering blights as chiefly and generally occasioned by small flies, and minute insects, whose organs are proportionably finer than the former, he whipped the limbs of a wall plum tree, as high as he could reach; the leaves of which were preserved green, flourishing, and unhurt, while those not 6 inches higher, and thence upwards, were blighted, shrivelled up, and full of worms. Some of these last he afterwards restored by whipping with, and tying up, elder among them. It must be noted that this tree was in full blossom at the time of whipping, which was much too late, as it should have been done once or twice before the blossom appeared. But he concluded from the whole, that if an infusion of elder was made in a tub of water, so that the water might be strongly impregnated with it, and then sprinkled over the tree, by a hand engine, once every week or fortnight, it would effectually answer every purpose that could be wished, without any risk of hurting the blossoms or fruit.

3d. What the farmers call the yellows in wheat, and which they consider as a kind of mildew, is in fact occasioned by a small yellow fly with blue wings, about the size of a gnat. This blows in the ear of the corn, and produces a worm, almost invisible to the naked eye; but being seen through a pocket microscope, it appears a large yellow maggot of the colour and gloss of amber, and is so prolific that Mr. G. distinctly counted 41 living yellow maggots or insects, in the husk of one single grain of wheat, a number sufficient to eat up and destroy the corn in a whole ear. He intended to have tried the following experiment sooner; but the dry hot weather bringing on the corn faster than was expected, it was got and getting into fine blossoms ere he had an opportunity of ordering as he did; but however the next morning at daybreak, two servants took two bushes of elder, and went one on each side of the ridge from end to end, and so back again,

drawing the elder over the ears of corn of such fields as were not too far advanced in blossoming. Mr. G. conceived, that the disagreeable effluvia of the elder would effectually prevent those flies from pitching their tents in so noxious a situation; nor was he disappointed, for he was firmly persuaded that no flies pitched or blowed on the corn after it had been so struck. But he had the mortification of observing the flies, the evening before it was struck, already on the corn, 6, 7, or 8, on a single ear, so that what damage accrued, was done before the operation took place; for, on examining it last week, he found the corn which had been struck pretty free of the yellows, very much more so than what was not struck. One of those yellow flies laid at least 8 or 10 eggs, of an oblong shape, on his thumb, only while carrying by the wing across 3 or 4 ridges, as appeared on viewing it with a pocket microscope.

4th. Crops of turnips are frequently destroyed, when young, by being bitten by some insects, either flies or fleas; this he flatters himself may be effectually prevented, by having an elder bush spread so as to cover about the breadth of a ridge, and drawn once forward, and backward by a man over the young turnips. He was confirmed in this idea, by having struckan elder bush over a bed of young cauliflower plants, which had begun to be bitten, and would otherwise have been destroyed by those insects; but after that operation it remained untouched.

In support of his opinion, Mr. G. mentions the following fact from very credible information, that about 8 or 9 years before, this county was so infested with cock chaffers or oakwebs, that in many parishes they eat every green thing, except elder; nor left a green leaf untouched besides elder bushes, which alone remained green and unhurt, amid the general devastation of so voracious a multitude. On reflecting on these several circumstances, a thought suggested itself to him, whether an elder, now esteemed noxious and offensive, might not be one day seen planted with, and entwisting its branches among fruit trees, to preserve the fruit from destruction of insects: and whether the same means which produced these several effects, might not be extended to a great variety of other cases, in the preservation of the vegetable kingdom. The dwarf elder (ebulus) he apprehends emits more offensive effluvia, than common elder, therefore must be preferable to it in the several experiments.

XXIV. A Letter from John Call, Esq., to Nevil Maskelyne, F. R. S., Astron. Royal, containing a Sketch of the Signs of the Zodiac, found in a Pagoda, near Cape Comorin, in India. p. 353.

This sketch, fig. 2, pl. 7, Mr. Call drew with a pencil, as he lay on his back resting himself during the heat of the day, in a journey from Madurah to Twinwelly, near Cape Comorin. After such a discovery, he searched in his travels many other pagodas, or choultrys, for similar carvings, but never found above one

more equally complete, which was on the ceiling of a temple, in the middle of a tank before the pagoda of Teppecolum, near Mindurah, of which tank and temple Mr. Ward, painter, in Broad-street, near Carnaby-market, has a drawing; but Mr. Call often met with the several parts in detached pieces.

From the correspondence of the signs of the zodiac which we at present use, and which we had, he believes, from the Arabians or Egyptians, he is apt to think that they originally came from India, and were in use among the Bramins, when Zoroaster and Pythagoras travelled thither, and consequently adopted and used by those travellers: and as these philosophers are still spoken of in India, under the names of Zerdhurst and Pyttagore, he hazards another idea, that the worship of the cow, which still prevails in India, was transplanted from thence to Egypt. But this is only conjecture; and it may with almost equal probability be said, that Zoroaster or Pythagoras carried that worship to India. However, he thinks there is an argument still in favour of India for its antiquity, in point of civilization and cultivation of the arts and sciences; for it is hardly doubted that all these improvements came from the east to the west; and, if we may be allowed to draw any conclusions from the immense buildings now existing, and from the little of the inscriptions, which can be interpreted on several of the choultrys and pagodas, he thinks it may safely be pronounced, that no part of the world has more marks of antiquity for arts, sciences, and civilization, than the peninsula of India, from the Ganges to Cape Comorin; nor is there in the world a finer climate, or face of the country, nor a spot better inhabited, or filled with towns, temples, and villages, than this space is throughout, if China and parts of Europe are excepted.

Mr. Call thinks the carvings on some of the pagodas and choultrys, as well as the grandeur of the work, exceeds any thing executed now, not only for the delicacy of the chissel, but the expence of construction, considering, in many instances, to what distances the component parts were carried, and to what heights raised. Mr. Call also commits to Mr. M.'s inspection the * manuscripts of Mr. Robins, which he gave at his death; Mr. C. believes most of them have been printed, but if there are any which have not, or that can amuse or instruct others, you are welcome to use them as you please: I only wish they may contain any thing useful. While he lived, says Mr. C. I pursued those studies; but soon after his death new scenes arose, and engaged me more in practical service, than allowed me time for theory, or experiments.

The sketch, fig. 2, pl. 7, was from the ceiling of a choultry at Verdapettah,

* These I communicated to the R. S., together with this letter; but being examined by myself, Mr. Raper, Mr. Cavendish, and Mr. Horsley, at the desire of the society, they were not found to contain any thing material, more than has been already printed; excepting a treatise on military discipline: which, if it should be thought of use, may be inserted in the next edition of his works. N. M.—Orig.

in the Madurah country, taken July 8, 1764. Here A is symbol of the universal deity. BB two hooks of iron, to suspend a kind of throne, on which the deity or swamy often sat, when exhibited to the adorers.

XXV. An Account of the Flowing of the Tides in the South Sea, as observed on board His Majesty's Bark the Endeavour. By Lieut. J. Cook, Commander. p. 357.

Mr. Cook says, that from many circumstances and observations, he is fully convinced that the flood comes from the southward, or rather from the S. E.

Names of places where observed.	Lat. south.		Long. west.		New and full moon. High water.		Rise & fall.	
Success bay in strait le Maire	54	45	66	4	4h	30m	5f.	6in.
Lagoon island	18	47	139	28	0	30	......	
Matavai bay, Otaheita	17	29	149	30	0	30	0	11
Tolago bay, east coast of New Zealand	38	22	181	14	6	0	5	6
Mercury bay, N. E. ditto	36	48	184	4	7	30	7	0
River Thames, ditto	37	12	184	12	9	0	10	0
Bay of Islands, ditto	35	14	185	36	8	0	7	0
Queen Charlotte's Sound, Cook's strait, New Zealand	41	0	184	45	9	30	7	6
Admiralty bay, in ditto	41	45	185	12	10	0	7	0
Botany bay, coast of New South Wales	34	0	208	37	8	0	4	6
Bustard bay, ditto	24	30	208	20	8	0	8	0
Thirsty sound, ditto	25	5	210	24	11	0	16	0
Endeavour river, ditto	15	26	214	48	9	30	9	0
Endeavour's strait, which divides New Guinea from New Holland	10	37	218	45	1	30	11	0

XXVI. An Account of a New Electrometer, contrived by Mr. Wm. Henly, and of several Electrical Experiments made by him, in a Letter from Dr. Priestley, F. R. S., to Dr. Franklin, F. R. S. p. 359.

In my history of electricity, and elsewhere, says Dr. P., I have mentioned a good electrometer, as one of the greatest desiderata among practical electricians, to measure both the precise degree of the electrification of any body, and also the exact quantity of a charge before the explosion, with respect to the size of the electrified body, or the jar or battery with which it is connected; as well as to ascertain the moment of time, in which the electricity of a jar changes, when, without making an explosion, it is discharged by giving it a quantity of the contrary electricity. All these purposes are answered, in the most complete manner, by an electrometer of this gentleman's contrivance, a drawing of which I send you along with the following description.

The whole instrument is made of ivory or wood, exhibited in fig. 3, pl. 7, is an exceedingly light rod, with a cork ball at the extremity, made to turn on the centre of a semicircle B, and so as always to keep pretty near its limb, which is graduated: C is the stem that supports it, and may either be fixed to the prime

conductor, or be let into the brass knob of a jar or battery, or set in a stand, to support itself.

The moment that this little apparatus is electrified, the rod A is repelled by the stem C, and consequently begins to move along the graduated edge of the semicircle B; so as to mark with the utmost exactness, the degree in which the prime conductor, &c. is electrified, or the height to which the charge of any jar or battery is advanced; and as the materials of which this little instrument is made, are very imperfect conductors, it will continue in contact with any electrified body, or charged jar, without dissipating any of the electricity.

If it should be found, by trial in the dark, that any part of this instrument contributes to the dissipation of the electric matter, which, when the electrification was very strong, I once observed mine to do, it should be baked* a little, which will presently prevent it. If it be heated too much, it will not receive electricity readily enough; and then the motion of the index will not correspond with sufficient exactness, to the degree in which the body to which it is connected is electrified; but this inconvenience is easily remedied, by moistening the stem and the index, for the semicircle cannot be too dry.

I find by experience, that this electrometer answers all the purposes I have mentioned, with the greatest ease and exactness. I am now sure of the force of any explosion before a discharge of a jar or battery, which I had no better method of guessing at before, than by presenting to them a pair of Mr. Canton's balls, and observing their divergency at a given distance: but the degree of divergency was still to be guessed at by the eye, and the balls can only be applied occasionally; whereas this instrument being constantly fixed to the prime conductor or the battery, shows, without any trouble, the whole progress of the charge; and, remaining in the same situation, the force of different explosions may be ascertained with the utmost exactness before the discharge.

If a jar be loaded with positive electricity, and I want to know the exact time when, by attempting to charge it negatively, it first becomes discharged, I see every step of its approach to this state by the falling of the index; and the moment I want to seize, is the time when it has got into a perpendicular situation, which may be observed, without the least danger of a mistake. Accordingly, I find that, in this case, not the least spark is left in the jar. If I continue the operation, the index, after having gained its perpendicular position, begins to advance again, and thereby shows the exact quantity of the opposite electricity that it has acquired.

Considering the admirable simplicity, as well as the great usefulness of this instrument, it is something surprising that the construction should not have oc-

* Warmed a little, to dry off the damps, particularly from the index.—Orig.

curred to some electrician before this time. Nollet's and Mr. Wait's invention of threads, projecting shadows on a graduated board, resembled this apparatus of Mr. Henly's, but was a poor and awkward contrivance in comparison with it; nor was Richman's gnomon, though a nearer approach to this construction, at all comparable to it; and the ingenious author of it had no knowledge of either of those methods when he hit on this.

Many of the effects of my battery, in breaking of glass, and tearing the surface of bodies, Mr. Henly performs by a single jar, only increasing the weight with which the bodies are pressed, while the explosion is made to pass close under them. By this means he raises exceedingly great weights, frequently 6 pounds Troy, and shatters strong pieces of glass into thousands of the smallest fragments; he even reduces thick plate glass by this means to an impalpable powder. But what is most remarkable is, that when the pieces of glass are thick, and strong enough to resist the shock, they are marked by the explosion with the most lively and beautiful colours, generally covering the space of about an inch in length, and half an inch in breadth.

In some of the pieces which he was so obliging as to send me, these colours lie all intermixed and confused; but in others I observe them to be disposed in prismatic order, in lines parallel to the course of the explosion, and in some I have counted 3 or 4 distinct returns of the same colour. He has lately informed me, that, since he sent me this piece, he has struck these prismatic colours into another mass of glass, in a still more vivid and beautiful manner, the colours shooting into one another. This effect, he says, was produced by making a 2d explosion, without moving any of the apparatus after the first. When the glass in which these colours are fixed is examined, it is evident that the surface is shattered into thin plates, and that these give the colours, the thickness of them varying regularly, as they recede from the path of the explosion.

Besides these improvements, Mr. Henly has likewise, in a very ingenious manner, diversified several of the more entertaining experiments in electricity, particularly in his imitation of the effects of earthquakes by the lateral force of explosions; and he has also hit on several curious facts, that, unknown to him, had been observed before by others: the following particular, however, I believe is new, exciting a stick of sealing wax, and using a piece of tin foil for the rubber; he found that it would electrify positively, as well as glass rubbed with silk and amalgama.

XXVII. Meteorological Observations at Ludgvan in Mount's Bay, Cornwall, 1771. By Wm. Borlase, D. D., F. R. S. Communicated by Dr. J. Milles, Dean of Exeter, and F. R. S. p. 365.

This is a register, similar to former ones, of the barometer, thermometer,

weather, and rain, the whole quantity of which, from the year 1771, was 30.153 inches.*

XXVIII. Account of several Quadrupeds from Hudson's Bay.† *By Mr. John Reinhold Forster, F. R. S.* p. 370.

1. Arctic Fox, Penn. Synops. of Quad. p. 155, n. 113. Canis Lagopus, Linn. Place, Severn river.

A most beautiful specimen, in its snowy winter fur; this animal seems to be lower on its legs than the common fox, and is well secured against the intense cold of the climate, by the thickness and length of its hairs, which are at the same time as soft as silk. The account sent along with it from Severn river says, that these white foxes are silly, inoffensive animals; and are known to stand by, while a trap is baited for them, into which they put their heads immediately: they will, when pinched by hunger, devour those of their own kind, which are already caught in these traps. But the most curious circumstance is, their migration to the northward and the eastern coasts of the bay; for though a few of them are caught every year near York fort and Churchill river, yet, once in 3 or 4 years, they come in great numbers; and several hundred of their furs are sent to England in plentiful seasons, which always begin in November and end in April. The specimen sent is full grown, and its fur quite in season.

2. Lesser Otter. Penn. Syn. Quad. p. 239, n. 174. Mustela Lutreola, Linn. Syst. Nat. 66. Faun. Suec. N° 13. Place, Severn river.

It is still dubious, whether this animal ought to be considered as the same with the lesser otter of Europe and Asia; many circumstances seem to prove this identity; but some, such as the want of webs, which could not be discovered between the toes, and the white spot on the neck, will not admit of it. The natives of Hudson's Bay call this quadruped jakash; Mr. Graham from Severn river says, that it harbours about creeks, and lives on fish, like the otter;

* This is the last paper of this kind, which the Society will receive from the excellent author of the Natural History of Cornwall, and several other learned works; death having, though at an advanced age, put a period to a life divided between the pursuit of useful and experimental knowledge, and the faithful discharge of every moral and religious duty. M. M.—Orig.

† Among the occasional advantages, which the observations of the last transit of Venus have procured, that of receiving useful informations from, and settling correspondencies in, several parts of the world, is not the least considerable. From the factory at Hudson's Bay, the R. S. were favoured with a large collection of uncommon quadrupeds, birds, fishes, &c. together with some account of their names, place of abode, manner of life, uses, by Mr. Graham, a gentleman belonging to the settlement on Severn river; and the governors of the Hudson's Bay Company have most obligingly sent orders, that these communications should be from time to time continued. The descriptions contained in the following papers were prepared and given by Mr. Forster, before his departure on an expedition, which will probably open an ample field to the most important discoveries. M. Maty. —Orig.

it travels very slowly, and has from 4 to 7 young at a time; in size it equals the marten; its length is about 16 inches; its whole body is covered with shining dark brown hairs, which lie very close, and seem perfectly convenient for an amphibious animal; under these brown hairs the woolly hairs are tawny, the whole under-jaw is encompassed by a stripe of white hairs, and a little irregular spot of the same colour appears in the middle of the throat; the feet are quite covered with hair to the very nails, which are small, 5 on each foot, and of a whitish semipellucid colour; the tail is pretty well beset with hair, though not bushy, and much blacker than the rest of the body; it is about half as long as the whole animal.

3. Pine Marten. Penn. Syn. Quad. p. 216, n. 155. Mustela Martes (Abietum). Linn. Severn river, male and female.

These seem to be a variety of the yellow breasted marten, Br. Zool. I. 81; their colour, especially in the females, being much paler than that described in Mr. Pennant's works. The male is of a chestnut brown, the female a bright tawny yellow; the former has here some dark brown hairs, the latter in the same manner has some bright bay hairs. They both have white cheeks, and white tips of the ears. Their furs are very full of hair, proper to preserve them from the cold. The tail in both sexes is bushy, and darker than the rest of the body; in the female indeed it is tawny, with a black tip; in both it is shorter than described by Mr. Pennant, Mr. Brisson, and others, and was perhaps mutilated. This species feeds on mice, rabbits, &c. though it will not touch a dead mouse which is put as a bait in a trap, and therefore the inhabitants are obliged to make use of a partridge's head, or the like, for that purpose. If pursued with noise, it immediately gets up into a tree. Some gentlemen have unsuccessfully attempted to tame these creatures, and those kept in cages with that view have been observed to be troubled with epileptic fits. Numbers of them are caught at Hudson's Bay in traps made of small sticks. They burrow under ground, and bring forth from 4 to 7 young at a time.

4 Stoat and Ermine. Penn. Syn. Quad. p. 212, n. 151, α, β, Mustela Erminea. Linn. Severn river, Albany Fort.

One in the summer and another in the winter dress. The natives about Albany call them sic-cuse-sue, but it is not known why they give them that name. They feed on mice, small birds, all sorts of fish, flesh, and fowl.

5. Common Weesel. Penn. Syn. Quad. p. 211, n. 150. Mustela Nivalis. Linn.

One in its winter dress, length 7 inches, tail about 1 inch, perhaps mutilated; it is quite white, but the coat is mixed here and there with a brownish hair, especially in the tail. Another in the summer coat, the same as our weesel.

6. Skunk. Penn. Syn. Quad. p. 233, n. 167. Kalm's Travels, I. 273, tab. 1.

It answers to Mr. Pennant's description, except that the white stripe on the head is not connected with that on the back, and that the brown area, which is left between the two white stripes on the back, is broader than he describes it.

7. Canada Porcupine. Penn. Syn. Quad. p. 266, n. 196. Hystrix dorsata, Linn. Severn river.

It agrees perfectly with the descriptions. These animals live among the pine trees, of which the bark is their food in winter, as willow tops and the like are in summer. They copulate in September, and bring forth only one young the first week in April. During winter they seldom travel above 500 yards, so that one is always sure of finding a porcupine, as soon as one meets with a tree that has been fresh stripped of its bark. The longest quills of an old porcupine are about 5 inches long. The Europeans are very fond of the flesh of these animals, as it tastes, when roasted, exactly like that of a sucking pig. Their bones in winter have a greenish yellow colour, perhaps owing to their continually feeding on the bark of pine trees. It is known that the bones of animals will become red by their feeding on madder.

8. Beaver. Penn. Syn. Quad. p. 255, n. 190. Castor Fiber. Linn. Churchill river, N° 1.

A most beautiful specimen, in high preservation, and in full season; the fur is of a fine jetty black: the skull of another has likewise been sent. There is a great similarity in the conformation of the cutting teeth of this and the preceding quadruped (the porcupine); only the latter has them longer.

9. Musk-Beaver. Penn. Synn. Quad. p. 259, n. 121. Castor Zibethicus. Linn. Musquash. Severn river.

It frequents the plains, builds a house like the beaver, brings forth from 5 to 7 young at a time, and feeds on poplars, willows, and grass.

10. Alpine Hare. Penn. Syn. Quad. p. 249, n. 185. Lepus timidus. Linn. Kalm's Trav. into N. America. III. p. 59. York fort.

A fine specimen, in its complete winter fur, being quite white, except the ears, which have black tips. It is much larger than the following animal. The common hare, Penn. Syn. Quad. does not seem to be a native of America.

11. American Hare, called Rabbit at Hudson's Bay. Kalm's Trav. into N. Amer. I. 105, II. 45. Severn and Churchill rivers.

This species, which has been improperly called rabbit, perhaps because it is less than the hare, is certainly new, and was never described before, except by Kalm in his Travels through North America, vol. I. 105, II, 45. The account he there gives, corresponds with that of Mr. Graham, and with the specimen now in the Royal Society's collection. These animals are numerous at Hudson's Bay; they do not burrow under ground, but live summer and winter under windfalls and roots of trees. They do not migrate, but always keep about the

same place, unless disturbed. They breed once or twice a year, and have 5 to 7 young at a time: their weight is from 3 to 4½ pounds. Their flesh is not so white and delicate as that of the common rabbit, but yet is good food in summer and winter. Great numbers of them are annually caught in the following manner: as they always are used to go on one particular path, the English and natives lay young trees across it, forming a hedge, in which there is an opening for the creature to go through; in this place they fix a snare, made of brass wire, packthread, or the like, fastened with a slipping knot to a cross piece, the end being tied to an elastic pole; so that when the animal puts its head into the snare, the knot is drawn from the cross piece above, and the pole flying up, immediately suspends the animal in the air.

The proper characteristics of this species seem to be, 1. Its size, which is somewhat larger than a rabbit's, but less than that of the alpine or lesser hare. 2. The proportion of its limbs, its hind feet being longer in proportion to the body than those of the rabbit and the common hare. Vide the Hon. Daines Barrington's letter on this new species of hare, in this volume, p. 6. 3. The tips of the ears and tail, which are constantly grey, not black. Kalm's Trav. II, p. 45. Perhaps some other characters might be ascertained, if the animal was brought over in its perfect summer fur; for all the specimens in the Royal Society's Museum are either entirely in their winter dress, or in a changing condition. Mr. Kalm mentions, that those which are found in New Jersey, where the climate is much more mild than at Hudson's Bay, keep the same grey colour both summer and winter; that in spring they breed in hollow trees, whence they are driven out by crooked sticks, smoke, &c. lastly, that they do much mischief to cabbage fields and orchards, by eating the cabbage plants, and the bark of the apple trees, feeding only by night, as the common hare.

12. Quebec Marmot. Penn. Syn. Quad. p. 270, n. 199. Churchill river, N° 5.

This creature is called a ground squirrel, at Churchill fort; it differs much in size from that described in the Syn. Quad. being much less than a rabbit, perhaps it is a young one. The nose is blunt, the ears are short and roundish, the top of the head chestnut, back all over sprinkled with whitish, black, and yellowish brown: the legs and whole underside of the animal are of a bright ferruginous colour; the tail is very short, and black at the tip. The length of the animal from the nose to the beginning of the tail is about 11 inches, that of the tail 3 inches. Its toes on the fore feet 4, hind feet 5.

13. Common Squirrel. Penn. Syn. Quad. p. 279, n. 206. Sciurus vulgaris. Linn.

A variety of the common species, being somewhat inferior in size, having a ferruginous back and grey belly, a shorter tail than the common European sort,

of a fine ferruginous red, edged only with black. This animal lives in pine trees, of which the cones are its food; it lies dormant the greater part of the winter.

14. Greater Flying Squirrel. Severn river.

It is equal in size, if not larger than the common squirrel; has pretty long hairs, dusky at bottom, tawny brown at the very tips only; and disposed so that the back appears wholly of that reddish brown colour; the tail is very bushy, somewhat compressed, but not pinnated (i. e. with the hairs disposed horizontally on each side of it, as for example in the common squirrel), it is brownish on the upperside with a dusky tip, of a yellowish white below; the whole underside of the animal has the same yellowish white colour. The membrane reaches from the fore feet to the hind feet, without extending to the ears: it is found in James's Bay, about 51° north latitude. This is perhaps Linnæus's Sciurus volans, and the same with the flying squirrel of the arctic parts of Europe. Mr. Brisson seems to have confounded this and the little Virginian squirrel together, and his quotations are quite confused. Linnæus's Mus volans certainly is a variety of the little flying squirrel, of the milder parts of North America, New-York, Pennsylvania, Virginia, which is vastly different from this in size and colour.

15. A small animal, called a Field Mouse. Churchill river.

A specimen in very bad preservation, wanting legs, tail, &c. which makes it impossible to determine of what species it is; its size is somewhat superior to that of a mouse, its colour dusky, mixed with tawny brown, and dirty white on the belly; its head is broad, like that of the short-tailed field mouse, and has a dusky line in the middle between the eyes, which extends, though rather indistinctly, all along the back; its ears are very small and roundish.

16. This is likewise a very bad mutilated specimen, less than the common mouse, dusky, and brown above, and whitish below; its ears are pretty large and prominent.

17. Field Mouse. Penn. Syn. Quad. p. 302, n. 230. Mus sylvaticus, Linn.

Two specimens; the descriptions answer pretty well; the ears are large and round; the tail is very long, and whitish below.

18. Short-tailed Mouse. Penn. Syn. Quad. p. 305, n. 233. Mus terrestris, Linn. Le Campagnol de Buffon.

Mr. Pennant's admeasurements do not quite answer, but M. D'Aubenton's coincide.

19. Fetid Shrew. Penn. Syn. Quad. p. 307, n. 235. Sorex Araneus, Linn.

The specimen is much blacker on the back than the European shrew, its sides are reddish brown.

20. Shrew; two specimens. The colour is of a dusky grey above, and a dirty white or yellowish below; the nose is very long and slender; the length from the nose to the tail, in the one specimen is 2¼, in the other almost 2 inches; the tail

is about an inch and half long, thinly beset with hairs, brown above, and yellowish below. If this species had no tail, he would take it to be the minute shrew, which the Rev. Mr. Laxman found in Siberia, and which is the sorex minutus. Linn.

XXIX. An Account of the Birds sent from Hudson's Bay; with Observations relative to their Natural History; and Latin Descriptions of some of the most uncommon. By J. Forster, F. R. S.,* p. 382.

1. *Land-Birds.* 1. Accipitres, Rapacious. Faun. Am. Sept.

1. Falco, Falcon. 1. Columbarius. 128, 21. Pigeon Hawk. Faun. Am. Sept. p. 9. Catesby l. t. 3. Epervier de la Caroline. Brisson I. p. 378. Severn river, N° 19.

This species is called a small bird hawk at Hudson's Bay. It is migratory, arriving near Severn river, in May, breeding on the coast, and then retiring to a warmer climate in autumn. It feeds on small birds; and, on the approach of any person, will fly in circles, making a hideous shrieking noise. The breast and belly are yellowish, with brown streaks, which are not mentioned by the ornithologists, though their descriptions answer in other respects. It weighs 6 ounces and a half, its length is 10½, the breadth 22½. Catesby's figure is a very indifferent one.

Falco, 2. Spadiceus. New species. Chocolate falcon. Faun. Am. Sept. p. 9. This species at first sight bears some resemblance to the European moor buzzard, or æruginosus, Linn. but is much less, and wants the light spots on the head and shoulders. No number or description was sent along with it.

Falco, 3. Sacer, Brisson, I. p. 337. Sacre de Buffon, oiseaux, (edition in 12mo.) Tom. II. p. 349, t. 14. Faun. Am. Sept. p. 9. Severn river, N° 16.

Speckled partridge hawk, at Hudson's Bay. The name is derived from its feeding on the birds of the grous tribe, commonly called partridges, at Hudson's Bay. Its irides are yellow, and the legs blue. It comes nearest the sacre of Brisson, Buffon, and Belon; but Buffon says it has black eyes, which is very indistinct; for the irides are black in none of the falcons, and in few other birds; and the pupil, if he means that, is black in all birds. It is said by Belon to come from Tartary and Russia, and is therefore probably a northern bird. It is very voracious and bold, catching partridges out of a covey, which the Europeans are driving into their nests. It breeds in April and May. Its young are ready to fly in the middle of June. Its nests, as those of all other falcons, are built in unfrequented places; therefore the author of the account from Severn river

* The birds described by Mr. Foster being all introduced into Mr. Latham's ornithological volumes, under the same titles, it becomes unnecessary to give the Latin descriptions, which are therefore here omitted.

could not ascertain how many eggs it lays; however, the Indians told him it commonly lays 2. It never migrates, and weighs $2\frac{1}{2}$ pounds; its length is 22 inches, its breadth 3 feet.

2. Strix, owl. 4. Brachyotos. The short-eared owl. Brit. Zoology, folio, plate B. 3. 8vo, I. p. 156. Faun. Am. Sept. 9. Severn river, N° 17 and 64.

Mouse hawk at Hudson's Bay. It answers the description and figure in the British Zoology; but its ears or long feathers do not appear, The smallness of the head has probably given occasion to call it a hawk, though it does not fly about in quest of prey, like other hawks (as the account from Severn river says); it sits quiet on the stumps of trees, waiting mice with all the attention of a domestic cat, being an inveterate enemy of those little animals. It migrates southward in autumn; and breeds along the coast. Its irides are yellow. Its weight is 14 oz.; its length 16 inches, the breadth 3 feet.

Strix, 5. Nyctea. 132, 6. Snowy owl. Faun. Am. Sept. 9. Churchill river, N° 7. White owl.

It seems to be in its winter dress, as it is entirely white. The feet are covered with long white hair-like feathers to the very nails, but there are none on the soles or under parts of the toes.

Strix, 6. Funerea. 133, 11. Canada owl. Faun. Am. Sept. 9. Severn river, N° 13. Churchill river, N° 11.

Cabeticuch, or cabaducutch, is the Indian name of this bird. Linnæus's description answers perfectly. The male, which in the class of birds of prey is generally smaller, is however in this species larger than the female, according to the account from Severn river. Its colour is likewise much blacker, and the spots more distinct. The eyes are large and prominent; the irides of a bright yellow. The weight is 12 ounces; its length 17 inches, the breadth 2 feet. It has only 2 young at one hatching.

Strix, 7. Passerina. 133, 12. Little owl. Brit. Zool. Faun. Am. Sept. 9.

The number belonging to this bird is lost, but it is most probably that from Severn river, N° 15, called shipomospish by the natives. The crown of the head is speckled with white, as in the strix funerea.

Strix, 8. Nebulosa. New species. The grey owl. Severn river, N° 36.

This fine non-descript owl feeds on hares, ptarmigans, mice, &c. It has 2 young at a time. The specimen sent over is said to be one of the largest. It is not described by any author. Its weight is 3 pounds, length 16 inches, breadth 4 feet.

3. Lanius, shrike. 9. Excubitor. 135, 11. Great butcher-bird. Brit. Zool. Cinereous shrike. Faun. Am. Sept. Severn river, N° 11.

White Whiskijohn at Hudson's Bay. The specimen is a male; it weighs 2 ounces and a half, is seldom found on the coast, but frequent about a hundred

miles inland; and feeds on small birds. It corresponds with ours in every respect.

II. Picæ, pies. Faun. Am. Sept.

4. Corvus, crow. 10. Canadensis. 158, 16. Cinereous crow. Faun. Am. Sept. 9. Severn river, N° 9 and 10.

These birds are called whiskijohn and whiskijack at the Hudson's Bay. They weigh 2 ounces; and are 9 inches long, and 11 broad. Their eyes are black, and their feet of the same colour. Their characters correspond with the Linnæan description. They breed early in spring; their nests are made of sticks and grass, and built in pine trees; they have 2, rarely 3, young ones at a time; their eggs are blue; they fly in pairs; the male and female are perfectly alike; they feed on black moss, worms, and even flesh. When near habitations or tents, they are apt to pilfer every thing they can come at, even salt meat; they are bold, and come into the tents to eat victuals out of the dishes. They watch persons baiting the traps for martins, and devour the bait as soon as they turn their backs. These birds lay up stores for the winter, and are seldom seen in January, unless near habitations; they are a kind of mock bird; when caught, they pine away and die, though their appetite never fails them.

Corvus, 11. Pica. 157. 13. Magpie. Brit. Zool. Faun. Am. Sept. 9. Albany Fort, N° 5.

It is called oue-ta-kee aske, i. e. heart-bird, by the Indians. It is a bird of passage, and rarely seen; it agrees in all respects with the European magpie, on comparison.

5. Picus, woodpecker. 12. Auratus. 174. 9. Gold wing woodpecker. Faun. Am. Sept. 10. Catesby, I. 18. Albany Fort, N° 4. the large woodpecker.

The natives of America call this bird ou-thee-quan-nor-now, from the yellow colour of the shafts of the quill and underside of the tail feathers. It is a bird of passage; visits the neighbourhood of Albany fort in April, leaves it in September; lays from 4 to 6 eggs in hollow trees; feeds on small worms and other insects. Its descriptions answer exactly.

Picus, 13. Villosus, 175. 16. Hairy woodpecker. Faun. Am. Sept. 10. Catesby I. 19. Severn river, N° 56.

The specimen sent over is a female, by its wanting the red on the head. The descriptions of Linnæus and Brisson agree; only the two middlemost feathers are black, the next are of the same colour, but have a white rhomboidal spot near the tip; the next are black, with the upper half obliquely white, the very tip being black; the next after that are white, with a round black spot on the inner side close to the base, and the lower part of the shaft is black, the outermost feathers are quite white, the shaft only at the base being black.

14. Tridactylus. 177. 21. Three-toed woodpecker. Faun. Am. Sept. Severn river, N° 8.

A female, weight 2 ounces, length 8 inches, breadth 13; eyes dark blue, legs black. It builds its nest in trees, lives in woods upon worms picked out of trees, is not very common at Severn river. The descriptions answer.

III. Gallinæ, gallinaceous. Faun. Am. Sept.

6. Tetrao, grous. 15 Canadensis, 274. 3. Canaca, 275. 7. Faun. Am. Sept. 10. Spotted grous. Gelinotte du Canada, male et femelle, Pl. enl. 131 et 132. Buffon Oiseaux II. p. 279, 4to. Brisson I. p. 203. t. 20. f. 1, 2, and p. 201. app. 10. Edwards, t. 118 and 71. Severn river, N° 5. Woodpartridge.

These birds are all the year long at Hudson's Bay, and never change the colour of their plumage. The accounts from Hudson's Bay say, there is no material difference between the male and female; which must be a mistake, as they are really very different. Linnæus's descriptions of the tetrao canadensis, and canace, both answer to the specimens sent over, so that after comparing them, Mr. F. finds they are only one and the same species. He supposes the dividing them into two was occasioned by Brisson's and Edwards's description, being taken from specimens sent from different parts of the continent of America, and perhaps caught at different seasons. Mr. de Buffon has the same opinion, and by comparing the drawings of Edwards, with those of the planches enluminées, it is put beyond a doubt. These birds are very stupid, may be knocked down with a stick, and are frequently caught by the natives with a stick and a loop. In summer they are good eating; but in winter they taste strongly of the pine spruce, on which they feed during that season, eating berries in summer. They live in pine woods, their nests are on the ground; they generally lay but 5 eggs.

Tetrao, 16. Lagopus, 274. 4. White grous. Faun. Am. Sept. 10. Ptarmigan. Br. Zool. Lagopede de la Baye de Hudson. Buffon Oiseaux II. p. 276. Edw. t. 72. Severn river, N° 1—4. Willow partridges.

The Hudson's Bay ptarmigan has been separated from the European in the British Zoology, and afterwards by M. de Buffon: however Mr. F. cannot yet find the differences which they assign to these species. They contend that the Hudson's Bay bird, figured by Edwards, is twice as large as the European ptarmigan; Mr. Edwards does not intimate this, when he says, the bird is of a middle size, between partridge and pheasant; he on the contrary supposes them to be the same species. The British Zoology, after Willoughby, says, the ptarmigan's length is 13¾ inches. The account from Severn river says it is 16¼ inches. The breadth in the British Zoology is said to be 23 inches. Willoughby's ptarmigan weighed 14 ounces; that in the British Zool. illustr. t. 13. 19 ounces; that from the Hudson's Bay (1½ lb.) 24 ounces. These differences are of little

consequence, and far from increasing the Hudson's Bay bird to double the size of the European. The British Zoology says there is a difference in the summer colours; but Mr. Edwards informs us, that he compared the Hudson's Bay bird with the descriptions of former ornithologists, and found them to answer; he likewise assures us he had the same bird from Norway. Therefore Mr. F. cannot help dissenting from the British Zoology, in this one particular, and thinking, with Linnæus and Brisson, that the European and Hudson's Bay ptarmigans are the same, especially as the colours vary very much in the different sexes and at different seasons. To this may be added the testimony of a gentleman well versed in natural history, who, having had opportunities of comparing numbers of Hudson's Bay and European ptarmigans, assured him that he did not see any difference between them. They go together in great flocks in the beginning of October, living among the willows, of which they eat the tops, whence they have got the name of willow partridges: about that time they lose their beautiful summer plumage, and exchange it for a snowy white dress, most providently adapted by its thickness to screen them against the severity of the season, and by its colour against their enemies the hawks and owls, against whose attacks they would otherwise find no shelter. Each feather is double, that is, a short one under a long one, to keep them warm. In the latter end of March, they begin again to change their plumage, and have got their full summer dress by the end of June. They breed every where along the coast, and have from 9 to 11 young at a time; making their nests on the ground, generally on dry ridges. They are excellent eating, and so plentiful that ten thousand have been taken at Severn, York, and Churchill Forts. The method of netting or catching them, is as follows: a net made of jack twine, 20 feet square, is laced to 4 long poles, and supported in front with the sticks, in a perpendicular situation; a long line is fastened to these supports, one end of it reaching to a place where a person lies concealed; several men drive the ptarmigans (which are as tame as chickens, especially on a mild snowy day), towards the net, which they run to as soon as they see it. The person concealed draws the line, by which means the net falls down, and catches 50 or 70 ptarmigans at once. They are sometimes rather wild, but grow better humoured, as Mr. Graham says, by being driven about, for they seldom forsake those willows which they have once frequented.

Tetrao. 17. Togatus, 275. 8. Shoulder-knot grous. Grosse gelinotte du Canada. Pl. enl. 104. Briss. I. 207. t. 21. f. 1. Buffon Oiseaux II. p. 287. Severn river, N° 60 and 61. Albany Fort 1 and 2.

This bird answers the descriptions given of it by the ornithologists in all respects, and perfectly resembles the figure in Brisson, and in the planches enluminées. It differs from Edwards's ruffed heathcock, t. 248, or Linnæus's tetrao umbellus, as the latter has not the shining black axillar feathers, or shoulder-

knot, but a ferruginous one is much less, and has brighter colours. M. de Buffon however thinks they are the same, and suspects at the same time, that the bird which he calls la grosse gelinotte du Canada, and which is the same with the Society's specimens, is the female of Mr. Edwards's bird, t. 248. This conjecture is destroyed by the specimens now sent from Hudson's Bay, which by the accounts from thence are expressly said to be males. The shoulder-knot grouses bear the Indian name of puskee, or puspuskee, at Hudson's Bay, on account of the leanness and dryness of their flesh, which is extremely white, and of a very close texture, but when well prepared is excellent eating. They are pretty common at Moose Fort, Henly House, but are seldom seen at Albany Fort, or to the northward of the above places. In winter they feed on juniper tops, in summer on gooseberries, raspberries, currants, &c. They are not migratory, staying all the year at Moose Fort; they build their nests on dry ground, hatch 9 young at a time, to which the mother clucks, as our common hen does; and on the least appearance of danger, or in order to enjoy a comfortable degree of warmth, the young ones retire under the wings of their parent. N. B. A specimen, which is supposed to be either a young bird or a female, wants the bluish black shoulder-knot; but it is the same in all other respects.

Tetrao, 18. Phasianellus. Linn. Syst. Nat. Ed. x. p. 160. n. 5. Edw. 117. Longtailed grous. Faun. Am. Septentr. 10. Severn river, N° 6 and 7. Albany Fort, N° 3.

This bird, which Mr. Edwards has drawn plate 117, was by Linneus, in the 10th edition of his System, ranged as a new species of grous or tetrao, by the specific name of phasianellus, alluding to the name of pheasant which it bears at Hudson's Bay, and likewise to its pointed tail. He afterwards, in the new or 12th edition of the System, p. 273, makes it a variety of the great cock of the wood, or tetrao urogallus, probably from the account in Mr. Edwards, that the male struts very upright, is in general of a darker colour than the female, and has a glossy neck. These circumstances however are not sufficient to bring them under the same species, for it is known that the males of all the grous tribe, and indeed of most of the gallinaceous birds, are used to strut in a very stately manner, and that the colours of their plumage are much more distinct than those of the females. But the specific difference alone, which Linnæus assigns to the cock of the wood, absolutely excludes our Hudson's Bay species; he calls it tetrao pedibus hirsutis, cauda rotundata, axillis albis. Whoever examines Mr. Edwards's figure, and the specimens now in the Society's possession, will find the tail very short, but pointed, the two middle feathers being half an inch longer than the rest, (Mr. Edwards says 2 inches) and the axillæ, or shoulders, by no means white: besides this difference, the colour and size of the Hudson's Bay bird are likewise vastly different from those of the cock of the wood. Its

length is 17 inches, its breadth 24, and as Mr. Edwards justly says, it is somewhat larger than the common pheasant. The great cock of the wood is as large as a turkey; and its female, which is much less, however far exceeds our bird, it being 26 inches long, and 40 broad. See British Zool. 8vo, p. 200. The figures given of the female of the T. urogallus, or great cock of the wood, in the Br. Zool. folio, plate M,* and the planche enluminée 75, will serve on comparison as a convincing proof of the vast difference there is between the Hudson's Bay pheasant grouse and the European cock of the wood. The figure, which Mr. Edwards has given of the former bird, does not exactly correspond with the Society's specimen, as he has represented the marks on the breast half-moon shaped, though they are heart-shaped as those on the belly in the dried bird; that is, they are white spots, with a pale brownish yellow cordated brim. Nor can he agree with Mr. Edwards, when he calls this bird the long-tailed grous from Hudson's Bay; for its tail is really very short, in comparison with that of other grouse, and its smallness and acuteness afford one of the most distinguishing characters of the species.

The native Indians call these pheasant grouses oc-kiss-cow: they are found all the year long, among the small juniper bushes, of which the buds are their principal food, as also the buds of birch in winter, and all sorts of berries in summer. They never vary their colours; nor is there any great difference between the male and female, except in the caruncula or comb over the eye, which in the male is an inch long, and $\frac{3}{8}$ of an inch high. The account from Albany Fort adds, that the colour of the male is somewhat browner, and almost a chocolate on the breast. Their flesh is of a light brown, exceedingly juicy, and they are very plump. They lay from 9 to 13 eggs; their young can run almost as soon as they are hatched: they make a piping noise somewhat like a chicken. The cock has a shrill crowing note, not very loud; but when disturbed, or while flying, he makes a repeated noise of cuck, cock. They are most common in winter at Albany Fort.

Before leaving the genus of grouses, he observes that their feet have a peculiarity, taken notice of by few authors; the toes, in several species, have on each side a row of short flexible teeth, like those of a comb; so that the toes appear pectinated. The species, which are known to have such pectinated toes, are, 1. The great cock of the wood, tetrao urogallus, Linn. 2. The black cock, T. tetrix, Linn. 3. The spotted grous, t. canadensis, and t. canace, Linn. 4. The ruffed grous, t. umbellus, Linn. 5. The shoulder-knot grous, t. togatus, Linn. 6. The pheasant grous, t. phasianellus. 7. The hazel hen, t. bonasia, Linn. 8. The pyrenæan grous, t. alchata, Linn.

This is a circumstance which ought to be attended to in all other species of

grouses, as it may in time afford a distinguishing character for a division in this great genus: the ptarmigan, or t. lagopus, Linn. is without these teeth.

IV. Columbæ, columbine. Faun. Am. Sept.

7. Columba, pigeon. 19. Migratoria. 285. 36. Migratory pigeon, Catesb. I. 23. Kalm II. p. 82. t. Passenger pigeon, Faun. Am. Sept. 11. Severn river, N° 63. Wood-pigeon.

These pigeons are very scarce so far northward as Severn river, but abound near Moose-fort, and further inland to the southward. Their common food are berries and juniper buds in winter; they fly about in great flocks, and are reckoned good eating. This account is confirmed by Kalm in his travels (English edition) vol. ii. p. 82 and 311. They hatch only 2 eggs at a time, and their nests are built in trees. Their eyes are small and black, the irides yellow, the feet red: the neck finely glossed with purple, brighter in the male. They weigh 9 ounces.

V. Passeres, passerine. Faun. Am. Sept.

8. Alauda, lark. 20. Alpestris. 289. 10. Klein, Hist. of Birds, 4to. p. 73. Shore lark, Faun. Am. Sept. 12. Catesb. I. 32. Albany Fort, N° 6.

This species is indifferently described by Linnæus, who says that all the tail-feathers on their inner web are white, (rectricibus dimidio interiore albis); though it does not appear that he saw a specimen of it himself. Both the quill and tail-feathers are dusky, and in both the outermost feather only has a white exterior margin. The coverts of the tail are of a pale ferruginous colour, and 2 of them are nearly as long as the tail itself. The scapulars are ferruginous; in the male, the head and whole back have a tinge of the same colour, marked with dusky streaks; in the female the back is grey, and the dusky stripes of a darker hue. The crown of the head is black in the male, dusky in the female; the forehead is yellow, the bill and feet are black, the belly of a dirty reddish white. These larks are migratory, they visit the environs of Albany Fort in the beginning of May, but go farther northward to breed: they feed on grass seeds, and buds of the sprig birch; run into small holes, and keep close to the ground, whence the natives give them the name of chi-chup-pi sue.

9. Turdus, thrush. 21. Migratorius, 292. 6. American fieldfare, Kalm II. p. 90. Faun. Am. Sept. II. Catesby I. 29. Severn river, N° 59. Albany Fort, 7, 8, 9.

The descriptions of these birds in various authors coincide with the specimens; at Severn river they appear at the beginning of May, and leave the environs before the frost sets in. At Moose Fort, in the north latitude 51°, they build their nest, lay their eggs, and hatch their young in the space of 14 days; but at York Fort and Severn settlement this is done in 26 days: they build their nests in trees, lay 4 beautiful light-blue eggs, feed on worms and carrion: when at

liberty they sing very prettily, but confined in a cage, they lose their melody. There is no material distinction between the male and female. Their weight is $2\frac{1}{2}$ ounces, the length 9 inches, and the breadth 1 foot; they are called red birds at Hudson's Bay; their Indian name is pee-pee-chue.

Turdus, 22. Severn river, N° 54 and 55, male and female.

From the striking similarity with our blackbird, the English at Hudson's Bay have given this bird the same name. However, on a close examination, the difference is very great between our European blackbird, and the Hudson's Bay or American one. The plumage of the male, instead of being deep black without any gloss, as in ours, has a shining purple cast, not unlike the plumage of the gracula quiscula, Linn. or shining gracule, Faun. Am. Sept.: or the maize thief, of Kalm. The female indeed is very like our female blackbird, being of a dusky colour on the back, and a dark grey on the breast. The feet and bill are quite black in both sexes; the former has the back claw almost as long again as any of the other claws. There are no vestiges of yellow palpebræ in either the male or the female; the bill in both is strong, smooth, and subulated; the upper mandible being carinated, but very little arched, and without any tooth or indenture whatever, on the lower side. The nostrils are as in other thrushes. This bird has no bristles at the base of its bill, its feet have such segments as Scopoli in the Annus I Historico-Naturalis attributes to the stares. Instead of being solitary and living retired like the European blackbirds, these American ones come in flocks to Severn river in June, live among the willows, build in all kinds of trees, and return to the southward in autumn. They feed on worms and maggots; their weight is $2\frac{1}{4}$ ounces, and they are 9 inches long, and 1 foot broad. One that was kept 12 months in a cage pined away, and died. Notwithstanding these circumstances, Mr. F. cannot help remaining undetermined with regard to this bird, which at first sight is like the blackbird, has the bill of a thrush, and the feet and gregarious nature of a stare. It is to be hoped that future accounts from Hudson's Bay may inform us further of the nature of this bird, its time of incubation, the number of eggs it lays, and the colour of those eggs, together with the note of the bird, the difference and characteristic marks of both the male and female, and other circumstances, which may serve to determine to what genus and species we are to refer this bird.

10. Loxia, grosbeak. 23. Curvirostra, 299, 1. Crossbill. Br. Zool. Faun. Am. Sept. 11. The small variety. Severn river, N° 27 and 28.

This bird comes to Severn river the latter end of May, breeds more to the northward, and returns in autumn, in its way to the south, departing at the setting in of the frost. The irides in the male are of a beautiful red, in the female yellow: the weight is said to be 10 ounces, probably by mistake for 1

ounce, as it is impossible so small a bird should weigh more, the length is 6 inches, the breadth 10.

24. Enucleator, 299. 3. Pine grosbeak, Br. Zool. and Faun. Am. Sept. Edw. 123, 124. Pl. enl. 135, f. 1. Severn river, N° 29, 30.

It answers to the descriptions and figures of the ornithologists pretty well; only Edwards's female has the red too bright, which is rather orange in our specimen, on the head, neck, and coverts of the tail. This bird only visits the Hudson's Bay settlements in May, on its way to the north, and is not observed to return in autumn; its food consists of birch willow buds, and others of the same nature; it weighs 2 ounces, is 9 inches long, and 13 broad.

11. Emberiza, bunting, 25. Nivalis, 308, 1. Greater brambling, Br. Zool. Snowbird snowflake, ibid. Snow-bunting. Faun. Am. Sept. 11. Severn river, N° 24-26.

The bird, in summer dress, corresponds exactly with the description of the greater brambling, Br. Zool. The description of the snowflake, or the same bird in winter dress, ibid, vol. 4, p. 19, is somewhat different, perhaps owing to the different seasons the birds were caught in, as it is well known they change their colour gradually. They are the first of the migratory birds, which come in spring to Severn settlement; in the year 1771 they appeared April the 11th, stayed about 4 or 5 weeks, and then proceeded farther northward in order to breed there; they return in September, stay till the cold grows severe in November, then retire southward to a warmer climate. They live in flocks, feed on grass-seeds, and about the dunghills, are easily caught under a small net, some oatmeal being strewed under it to allure them; they are very fat, and fine eating. Their weight is 1 ounce and 5 drachms, the length $6\frac{1}{2}$ inches, and the breadth 10 inches.

Embriza. 26. Leucophrys. New species. White crowned bunting. Severn river, N° 50. Albany Fort, 10.

This elegant little species of bunting is called a hedge sparrow at Hudson's Bay, and has not hitherto been described. It visits Severn settlement in June, and feeds on grass-seeds, little worms, grubs, &c. It weighs $\frac{3}{4}$ of an ounce, and is $7\frac{1}{2}$ inches long, and 9 inches broad; the bill and legs are flesh-coloured; the male is not materially different from the female, its nests are built in the bottom of willow bushes, it lays 3 eggs of a chocolate colour. It visits Albany Fort in May, breeds there, and leaves it in September.

12. Fringilla, Finch, 27. Lapponica, 317, 1. Faun. Suec. 235. Severn river, N° 52.

It is called tecurmashish, by the natives at Hudson's Bay. The description in Linnæus's Fauna Suecica coincides exactly with the specimen; that in his system

answers very nearly; Mr. Brisson's description (though he quotes Linnæus, and Linnæus quotes him) is widely different. The specimen sent over is a female; the males have more of the ferruginous colour on the head; the eyes are blue, the legs dark brown. It is only a winter inhabitant near Severn river, appears not before November, and is commonly found among the juniper trees; it weighs $\frac{1}{2}$ of an ounce, its length is 5 inches, and its breadth 7.

Fringilla, 28. Linaria, 322, 29. Lesser red-headed linnet. Br. Zool. Severn river, N° 23.

The descriptions of Linnæus, Brisson, and the British Zoology, answer perfectly well. The figure in Planche enluminée 151, f. 2, has a quite ferruginous back, contrary to all the descriptions and the specimen before us, in which all the feathers on the back are dusky, edged with dirty white.

29, Montana, 324, 37. Mountain sparrow, Tree sparrow, Br. Zool. Edw. 269. Brisson III, p. 79. Faun. Am. Sept. Severn river, N° 20.

This seems to be a variety, as its tail is rather longer than usual, and forked; it answers nearly to the descriptions given by the ornithologists, and seems to be a female, as it has no black under the throat and eyes, and no white collar. The bill and legs are black, the eyes blue. At Severn settlement it arrives in May, goes to breed farther northwards, and returns in autumn: the weight is $\frac{3}{4}$ of an ounce, the length $6\frac{1}{2}$ inches, and breadth 10. Mr. F. was inclined to make this bird a new species, on account of the many differences between it and the mountain sparrow; but considering the specimen sent over was not in the best order, and might be a female, he thought it best to leave it where it is, till we are better informed.

Fringilla, 30, Hudsonia. New specimen. Severn river, N° 18.

This is certainly a nondescript species; it only visits Severn settlements in summer, not being seen there before June, when it stays about a fortnight, goes farther to the northward to breed, and passes by Severn again in autumn on its return south. It is very difficult to procure, and therefore it could not be determined whether the specimen was a male or female. It frequents the plains, and lives on grass-seeds; it weighs $\frac{1}{2}$ an ounce, is $6\frac{1}{4}$ inches long, and 9 inches broad: it has a small blue eye, and a whitish bill faintly tinged with red; the whole body is blackish, or of a soot colour, the belly alone with the two outermost tail feathers on each side being white.

13, Muscicapa, flycatcher. 31, Striata. New species. Striped flycatcher. Severn river, N° 48 and 49. Male and female.

This species visits Severn river only in summer, feeding on grass-seeds, &c.; it weighs $\frac{1}{2}$ an ounce, is 5 inches long, and 7 broad; the male is widely different from the female: this species is entirely nondescript.

14. Motacilla, Wagtail. 32. Calendula. 337. 47. Ruby crowned Wren.

Edw. 354. Faun. Am. Sept. The number belonging to this bird is lost; however, it is most probably that sent from Severn river, N° 53.

It answers to the descriptions and the figure of Edwards; its weight is 4 drachms, its length 4 inches, and its breadth 5. It migrates, feeds on grass seeds and the like, and breeds in the plains; the number of eggs not known.

15. Parus, Titmouse. 33. Atricapillus. 341. 6. Black Cap Titmouse. Albany fort, N° 11.

The description given by Linnæus answers, and so does M. Brisson's in most particulars, except that the quill feathers are not white on the inside. These birds stay at Albany fort all the year, yet seem most numerous in the coldest weather; probably being then more in want of food, they come nearer the settlements, in order to pick up all remnants. They feed on flies and small maggots, and likewise on the buds of the sprig birch, in which they perhaps only search for insects; they make a twittering noise, from which the natives call them Kiss-kiss-ke-shish.

Parus. 34. Hudsonicus. New Species. Hudson's Bay Titmouse. Severn river, N° 12.

This new species of titmouse, is called Peche-ke-ke-shish, by the natives. They are common about the juniper bushes, of which the buds are their food; in winter they fly about from tree to tree in small flocks, the severest weather not excepted. They breed about the settlements, and lay 5 eggs; they have small eyes, with a white streak under them, and black legs: the male and female are quite alike; they weigh half an ounce, are $5\frac{1}{8}$ inches long, and 7 inches broad.

16. Hirundo, Swallow. 35. Severn river, N° 58.

The swallows build under the windows, and on the face of steep banks of the river, they disappear in autumn; and the Indians say, they were never found torpid under water, probably because they have no large nets to fish with under the ice. The specimen sent answers in some particulars to the description of the martin, hirundo urbica, Linn. but seems to be smaller, and has no white on the rump. Mr. F. therefore thought it best to leave the species undetermined, till further informations are received from Hudson's Bay, on this subject.

2. *Water Birds.*—6. Grallæ, Clovenfooted. Faun. Am. Sept.

17. Ardea, Heron. 36. Canadensis. 234. 3. Edward 133. Canada Crane. Faun. Am. Sept. 14. Severn river, N° 35. Blue Crane. The account from Severn settlement says, there is no material difference between the male and female; however, the specimen sent over, seems to be a female, as its plumage is in general duller than that figured by Edwards, and as the last row of white coverts of the wing are wanting. These cranes arrive near Severn in May, have only 2 young at a time, retire southward in autumn; frequent lakes and ponds, and feed

on fish, worms, &c. They weigh 7 pounds and a half, are $3\frac{1}{4}$ feet long, and 3 feet 5 inches broad; the bill is 4 inches long, the legs 7 inches, but the leg and thigh 19.

Ardea. 37. Americana, 234. 5. Hooping Crane. Edw. 132. Catesby, l. 75. Faun. Am. Sept. 14. York fort.

Edwards's figure is very exact; Catesby's is not so good, as it represents the bill too thick towards the point.

38. Stellarius, 239. 21. Varietas. The Bittern, Br. Zool. Edw. 136. Faun. Am. Sept. page 14.* Severn river, N° 54.

At first sight, Mr. F. thought the specimen sent from Hudson's Bay, was a young bird; but on nearer examination, and comparing it with Mr. Edwards's account and figure, he takes it to be a variety of the common bittern peculiar to North America; it is smaller, but on the whole very much resembles our bittern. Mr. Edwards's measurements and drawings correspond very well with the specimen. This bird appears at Severn river the latter end of May, lives chiefly among the swamps and willows, where it builds its nest, and lays only two eggs at a time; it is very indolent, and, when roused, removes only to a short distance.

18. Scolopax, Woodcock. 39. Totanus. 245. 12. Spotted Woodcock. Faun. Am. Sept. 14. Albany fort, N° 16.

This bird is called a yellow leg at Albany fort, from the bright yellow colour of the legs, especially in old birds; a circumstance in which it varies from the descriptions of Linnæus and Brisson, probably because they described from dried specimens, in which the yellow colour always changes into brown. It agrees in other respects perfectly well with the descriptions: it comes to Albany fort in April or beginning of May, and leaves it the latter end of September. It feeds on small shell fish, worms, and maggots; and frequents the banks of rivers, swamps, &c. It is called by the natives sa-sa-shew, from the noise it makes.

Scolopax. 40. Lapponica. 246. 15. Red Godwit. Br. Zool. Faun. Am. Sept. 14. Ed. 138. Churchill river, N° 13.

Linnæus describes this bird very exactly in his Systema Naturæ: the middle of the belly has no white in the society's specimen, as that had from which the description in the Br. Zool. octavo 1, p. 353, 354, was taken. All the other characters correspond.

Scolopax. 41. Borealis. New Species. Eskimaux Curlew. Faun. Am. Sept. 14. Albany fort, N° 15.

This species of curlew, is not yet known to the ornithologists; the first

* In the Faunula Americæ Septentrionalis, p. 14. the synonym of Ardea Hudsonia, Linn. has by mistake been annexed to the bittern, and likewise pl. 135 of Edwards has been quoted instead of plate 136. They are two very different birds.—Orig.

mention made of it is in the faunula Americæ septentrionalis, or catalogue of North American animals. It is called wee-kee-me-nase-su, by the natives; feeds on swamps, worms, grubs, &c. visits Albany fort in April or beginning of May; breeds to the northward of it, returns in August, and goes away southward again the latter end of September.

19. Tringa, Sand-piper. 42. Interpres. 248. 4. Turnstone. Edward 141. Faun. Am. Sept. 14. Severn river, N° 31 and 32. This species is well described by the ornithologists; its weight is $3\frac{1}{4}$ ounces, the length $8\frac{3}{4}$ inches, and the breadth 17 inches; it has 4 young at a time; its eyes are black, and the feet of a bright orange: this bird frequents the sides of the river.

43. Helvetica. 250. 12. Brisson. Av. v. p. 106, t. 10, f. 2.

The number was lost, perhaps it is N° 17, from fort Albany; on that supposition the account is as follows: "the natives call it waw-pusk-abrea-shish, or white bear bird; it feeds on berries, insects, grubs, worms, and small shell fish; visits and leaves Albany fort at the same time with the scolopax tetanus, and borealis."

This bird answers very well to its description; the throat, breast, and upper part of the belly are blackish, as in the descriptions, but mixed with white lunulated spots, which are neither described nor expressed in M. Brisson's figure, and may be owing to the difference of sex, or climate.

7. Anseres, webbed-footed. Faun. Am. Sept.

29. Anas, Duck, 44. Marila. 196. 8. Scaup Duck. Br. Zool. Faun. Am. Sept. 17. Severn river, N° 44 and 45. Fishing Ducks.

Linnæus's description, and the figure in the Br. Zoology, folio, plate Q, p. 153, agree perfectly well with the specimens. The female, as Linnæus observes, is quite brown, the breast and upper part of the back being of a glossy reddish brown; the speculum of the wing and the belly are white. The eyes of the male have very bright yellow irides; those of the female are of a faint dirty yellow. The female is 2 ounces heavier than the male, which weighs 1 pound and a half, is $16\frac{1}{2}$ inches long, and 20 inches broad.

Anas. 45. Nivalis. Snow Goose. Faun. Am. Sept. p. 16. Lawson's Carolina. Anser niveus Briss. VI. 288. Klein. Anser nivis. Schwenkfeld, Marsigli. Danub. p. 802. t. 49. Severn river, N° 40, and a young one, N° 41, white Goose.

These white geese are very numerous at Hudson's Bay, many thousands being annually killed with the gun, for the use of the settlements. They are usually shot while on the wing; the Indians being very expert at that exercise, which they learn from their youth; they weigh 5 or 6 pounds, are $2\frac{2}{3}$ feet long, and $3\frac{1}{2}$ broad; their eyes are black, the irides small and red, the legs likewise red; they feed along the sea, and are fine eating; their young are bluish grey, and do not

attain a perfect whiteness till they are a year old. They visit Severn river first in the middle of May, on their journey northward, where they breed; return in the beginning of September, with their young, staying at Severn settlement about a fortnight each time. The Indian name is way-way, at Churchill river. Linnæus has not taken notice of this species.

Anas. 46. Canadensis. 198.14. Canada Goose. Faun. Am. Sept. 16. Edw. 151. Catesby 1, 92, &c. Severn river, N° 42.

The Canada geese are very plentiful at Hudson's Bay, great quantities of them are salted, but they have a fishy taste. The specimen sent over agrees perfectly with the descriptions and drawings. At Hudson's Bay this species is called the small grey goose. Besides this, and the preceding white goose, Mr. Graham, the gentleman who sent the account from Severn settlement, mentions 3 other species of wild geese to be met with at Hudson's Bay, he calls them, 1. The large grey goose. 2. The blue goose. 3. The laughing goose.

The first of these, the large grey goose, he says, is so common in England, that he thought it unnecessary to send specimens of it over. It is however presumed, that though Mr. Graham has shown himself a careful observer, and an indefatigable collector; yet, not being a naturalist, he could not enter into any minute examination about the species to which each goose belongs, nor from mere recollection know that his grey goose was actually to be met with in England. A natural history, by examination, often finds material differences, which would escape a person unacquainted with natural history. The wish, therefore, of seeing the specimens of these species of geese, must occur to every lover of that science. Mr. Graham says, the large geese are the only species that breed about Severn river. They frequent the plains and swamps along the coast. Their weight is 9 pounds.

The blue goose is as large as the white goose; and the laughing goose is of the size of the Canada or small grey goose. These last two species are very common along Hudson's Bay to the southward, but very rare to the northward of Severn river. The Indians have a peculiar method of killing all these species of geese, and also swans. As these birds fly regularly along the marshes, the Indians range themselves in a line across the marsh from the wood to high water mark, about musket shot from each other, so as to be sure of intercepting any geese which fly that way. Each person conceals himself, by putting round him some brush wood; they also make artificial geese of sticks and mud, placing them at a short distance from themselves, in order to decoy the real geese within shot: thus prepared, they sit down, and keep a good look out; and as soon as the flock approaches, they all lie down, imitating the call or note of geese, which these birds no sooner hear, and perceive the decoys, than they go straight down towards them; then the Indians rise on their knees, and discharge 1, 2, or 3

guns each, killing 2 or even 3 geese at each shot, for they are very expert. Mr. Graham says, he has seen a row of Indians, by calling round a flock of geese, keep them hovering among them, till every one of the geese was killed Every species of geese has its peculiar note or call, which must greatly increase the difficulty of enticing them.

Anas. 47. Albeola. 199. 18. The Red Duck. Faun. Am. Sept. 17. Edw. t. 100. Sarcelle de la Louisiane. Brisson VI. t. 41, f. 1. Severn river, N° 37 and 38. Fishing Birds.

The descriptions and figures answer very well with the male, except that the 3 exterior feathers are not white on the outside, but all dusky. The female is not described by any one of the ornithologists; and therefore deserves to be noticed, to prevent future mistake. The whole bird is dusky, a few feathers on the forehead are rusty, and some about the ears of a dirty white; the breast is grey, the belly and speculum in the wings white; the bill and legs are black. They visit Severn settlement in June, build their nests in trees, and breed among the woods, and near ponds; the weight of the female is 1 pound, its length 14 inches, and its breadth 21.

Anas. 48. Clangula. 201. 23. Golden Eye. Br. Zool. Faun. Am. Sept. 16. Severn river, N° 51.

These birds frequent lakes and ponds, and breed there: they eat fish and slime, and cannot rise off the dry land. The legs and irides are yellow; their weight is $2\frac{1}{8}$ pounds, and their measure 19 inches in length, and 2 feet in breadth. The specimen sent is the male.

Anas. 49. Perspicillata. 201. 25. Black Duck. Faun. Am. Sept. 16. Edw. 155. Churchill river, N° 14.

This species is exactly described, and well drawn by Edwards. The Indians call it she-ke-supartem. It ought to come into the first division of Linnæus's ducks, "rostro basi gibbo," as its bill is really very unequal at the base.

Anas. 50. Glacialis. 203. 30, and Hyenalis, 202. 29. Edw. t. 156. Swallow-tail. Br. Zool. Faun. Am. Sept. 17. Churchill river, N° 12.

At Churchill river the Indians call this species, har-har-vey; it corresponds with Edwards's description and drawing, pl. 156, but differs much from Linnæus's inexact description of the anas hymalis, to which he however quotes Edwards. On the whole, it is almost without a doubt that the bird represented by Edwards, plate 280, and Br. Zool. folio, plate Q, 7, and quoted by Linnæus for his anas glacialis, is the male, and that the bird figured by Edwards t. 156, and quoted by Linnæus for the anas hyemalis, is the female, of one and the same species. Linnæus mentions a white body, in his anas hyemalis, which in Edw. tab. 156, and in the society's specimen, is all brown and dusky, except the belly, temples, a spot on the back of the head, and the sides of the rump, which are white.

Linnæus says, that the temples are black; in the specimen now sent over, and in Mr. Edwards's figure, which Linnæus quotes, they are white; the breast, back, and wings, are not black as he says, but rather brown and dusky. A further proof, that Linnæus's anas glacialis and hyemalis are the same, is that the feet in both t. 156 and 280 of Edwards are red, and the bill black, with an orange spot.

Anas. 51. Crecca. 204. 33. Varietas. Teal. Br. Zool. Faun. Am. Sept. 17. Severn river, N° 33, 34. Male and female.

This is a variety of the teal, for it wants the two white streaks above and below the eyes; the lower one indeed is faintly expressed in the male, which has also a lunated bar of white over each shoulder; this is not to be found in the European teal. This species is not very plentiful near Severn river; they live in the woods and plains near little ponds of water, and have from 5 to 7 young at a time.

Anas. 52. Histrionica 204. 35. Harlequin Duck. Faun. Am. Sept. 16. Edw. t. 99. This bird had no number fixed to it; it agrees perfectly with Edwards's figure.

Anas. 53. Boschas. 205. 40. Mallard Drake. Faun. Am. Sept. Br. Zool. Severn river, N° 39.

It is called stock drake at Hudson's Bay, and corresponds in every respect with the European one, upon comparison.

21. Pelicanus, Pelican. 54. Onocrotalus 251. 1. A variety. York fort.

This variety of the pelican, agrees in every particular with Linnæus's oriental pelecan (pelecanus onocrotalus orientalis), but has a peculiar tuft or fringe of fibres in the middle of the upper mandible, something nearer the apex than the base. This tuft has not been mentioned by any author, and is also wanting in Edwards's pelican, t. 92, with which the society's specimen corresponds in every other circumstance. The P. onocrotalus occidentalis, Linn. or Edw. t. 93 American pelican, is very different from it: the chief differences are the colour, which in our Hudson's Bay bird is white, but in Edwards's is of a greyish brown; and the size, which in the white bird is almost double of the brown one. The quill feathers are black, and the shafts of the larger ones white. The alula, or bastard wing, is black. The bill and legs are yellow.

* 22. Colymbus, Diver, 55. Glacialis. 221. 5. Northern Diver. Br. Zool. Faun. Am. Sept. 16. Churchill river, N° 8. called a Loon there.

This bird is well described and drawn in the British Zoology, in folio.

* * Grebe. 56. Auritus, *α*. 222. 8. Edw. 145. Eared Grebe. Faun. Am. Sept. 15. Severn river. N° 43.

This is exactly the bird drawn by Edwards, t. 145. The specimen sent over is a female. It differs much from our lesser crested grebe. Br. Zool. octavo 1, p. 396, and Br. Zool. illustr. plate 77, fig. 2, and Ed. 96. fig. 2. However, in

both these works, it is considered only as a variety, or different in sex. Mr. Graham has the same opinion. It lives on fish, frequenting the lakes near the sea coast. It lays its eggs in water, and cannot rise off dry land. It is seen about the beginning of June, but migrates southward in autumn. It is called sekeep, by the natives. Its eyes are small, the irides red; it weighs 1 pound, and measures 1 foot in length, and one-third more in breadth.

23. Larus, Gull. 57. Parasiticus. 226. 10. Arctic Gull. Br. Zool. Faun. Am. Sept. 16. Edw. 148. 149. Churchill river, N° 15.

This species is called a man of war, at Hudson's Bay. It seems to be a female, by the dirty white colour of its plumage below; it agrees very well with Edwards's drawing, and that in the Br. Zool. illustr.

24. Sterna, tern. 58. Hirundo (variety), 227. 2. The greater tern. Br. Zool. Faun. Am. Sept.

The number belonging to this bird is lost, perhaps it is N° 17, from Churchill river, called 'a sort of gull, called egg-breakers, by the natives.' The feet are black; the tail is shorter and much less forked than that described and drawn in the Br. Zool. The outermost tail-feather also wants the black, which that in the British Zoology has. In other respects it is the same.

XXX. Geometrical Solutions of Three Celebrated Astronomical Problems. By the late Dr. Henry Pemberton, F.R.S. Communicated by Matthew Raper, Esq., F.R.S. p. 434.

Lemma.—To form a triangle with two given sides, that the rectangle under the sine of the angle contained by the two given sides, and the tangent of the angle opposite to the lesser of the given sides, shall be the greatest that can be.

Let the two given sides be equal to AB, and AC fig. 4, pl. 7: round the centre A, with the interval AC, describe the circle CDE, and produce BA to B; take BF a mean proportional between BE and BC, and erect the perpendicular FG, and complete the triangle AGB.

Here the sine of BAG is to the radius, as FG to AG; and the tangent of ABG to the radius, as FG to FB: therefore the rectangle under the sine of BAG and the tangent of ABG, is to the square of the radius, as the square of FG, or the rectangle EFC, to the rectangle under AG or AC and FB. But, EB being to BF as BF to BC, by conversion, EB is to EF as BF to FC, and also, by taking the difference of the antecedents and of the consequents, EF is to twice AF as BF to FC; and twice AFB is equal to EFC.

Now, let the triangle BAH be formed, where the angle BAH is greater than BAG. Here, the perpendicular HI being drawn, the rectangle under the sine of BAH and the tangent of ABH, will be to the square of the radius, as the rectangle EIC, to the rectangle under AC, IB. But IF is to FB as 2AFI to 2AFB, or

EFC; and 2AFI is greater than $AF^2 - AI^2$; also $AF^2 - AI^2$ together with EFC, is equal to EIC; therefore by composition, the ratio of IB to BF is greater than that of EIC to EFC; and the ratio of AC × IB to AC × FB greater than that of EIC to EFC: also, by permutation, the ratio of AC × IB to EIC greater than the ratio of AC × FB to EFC. But the first of these ratios is the same with that of the square of the radius to the rectangle under the sine of BAH and the tangent of ABH; and the latter is the same with that of the square of the radius to the rectangle under the sine of BAG and the tangent of ABG; therefore the latter of these two rectangles is greater than the other.

Again, let the triangle BAK be formed, with the angle BAK less than BAG, and the perpendicular KL be drawn. Then the rectangle under the sine of BAK and the tangent of ABK, is to the square of the radius, as the square of KL to the rectangle under AC, BL. Here, FL being to FB as 2AFL to 2AFB or EFC, and 2AFL less than $AL^2 - AF^2$, by conversion, the ratio of LB to FB will be greater than the ratio of ELC to EFC; therefore, as before, the rectangle under the sine of BAG and the tangent of ABG is greater than that under the sine of BAK and the tangent of ABK.

Corol. 1. BF is equal to the tangent of the circle from the point B; therefore BF is the tangent, and AB the secant, to the radius AC, of the angle, whose cosine is to the radius as AC to AB. Therefore AF is the tangent, to the same radius, of half the complement of that angle; and AF is also the cosine of the angle BAG to this radius.

Corol. 2. The sine of the angle composed of the complement of AGB, and twice the complement of ABG, is equal to 3 times the sine of the complement of AGB. Let fall the perpendicular AH, (fig. 5), cutting the circle in I; continue GF to K, and draw AK. Then BF^2 = EBC = GBL. Therefore GB : BF :: BF : BL, and the triangles GBF, FBL are similar. Consequently FL is perpendicular to GB, and parallel to AH; whence GH being equal to HL, GM is equal to MF, and MK equal to 3 times GM.

Now the arc IK = 2IC + GI; and the angle IAK = 2IAC + GAI; also GM is to MK as the sine of the arc GI to the sine of the arc IK, that is, as the sine of the angle GAI to the sine of the angle IAK. Therefore the sine of the angle IAK (= 2IAC + GAI) is equal to 3 times the sine of the angle GAI; but GAI is the complement of AGB, and IAC the complement of ABG.

Corol. 3. If (fig. 6) any line BN be drawn to divide the angle ABG, and AN be joined, also AO be drawn perpendicular to BN, and continued to the circle in P, the sine of the angle composed of NAP and 2PAC will be less than 3 times the sine of the angle NAP. Draw NQR perpendicular to AB, cutting AP in S; join AR, and draw QT perpendicular to BN, and parallel to AO; then BQ^2 = NBT. But BQ^2 is greater than the rectangle EBC, that is, greater than the rectangle NBV,

under the 2 segments of the line BN drawn from B, to cut the circle in N and V: therefore TB is greater than VB, and NO greater than OT. Consequently NS is greater than SQ. Hence RS is less than 3 times NS; and therefore the sine of the angle PAR (= NAP + 2PAC) is less than 3 times the sine of NAP.

PROB. I.—*To find in the Ecliptic the Point of Longest Ascension.*

ANALYSIS.—Let (fig. 7) ABC be the equator, ADC the ecliptic, BD the situation of the horizon, when D is the point of longest ascension. Let EFG be another situation of the horizon. Then the ratio of the sine of EB to the sine of FD is compounded of the ratio of the sine of BG to the sine of GD, and of the ratio of the sine of AE to the sine of AF; but the angles B and E being equal, the arcs EG, GB together make a semicircle; and, by the approach of EG towards GB, the ultimate magnitude of BG will be a quadrant, and the ultimate ratio of EB to FD will be compounded of the ratio of the radius to the sine of DG (that is, the cosine of BD) and of the ratio of the sine of AB to the sine of AD. Draw the arc DH perpendicular to AB. Then, in the triangle BDH, the radius is to the cosine of BD, as the tangent of the angle BDH to the co-tangent of HBD. Also, in the triangle BDA, the sine of AB is to the sine of AD, as the sine of the angle BDA (or BDC) to the sine of ABD; therefore the ultimate ratio of BE to DF is compounded of the ratio of the tangent of BDH to the cotangent of ABD, and of the ratio of the sine of BDC to the sine of ABD; which two ratios compound that of the rectangle under the tangent of BDH and the sine of BDC, to the rectangle under the cotangent and the sine of the given angle ABD.

But when D is the point of longest ascension, the ratio of BE to DF is the greatest that can be; therefore then the ratio of the rectangle under the tangent of BDH and the sine of BDC, to the given rectangle under the cotangent and sine of the given angle ABD, must be the greatest that can be; and consequently the rectangle under the tangent of BDH and the sine of BDC, must be the greatest that can be.

In the triangle BDA, the sine of BDH is to the sine of HDA, as the cosine of ABD to the cosine of BAD. Now, in the preceding lemma, let the angle BAG of the triangle AGB be equal to the spherical angle BDC: then will the sum of the angles ABG, AGB be equal to the spherical angle BDA. And, if AG in the triangle AGB, be to AB, as the cosine of the spherical angle DBA to the cosine of DAB, that is, as the sine of BDH to the sine of HDA, the angle ABG, in the triangle, will be equal to the spherical angle BDH; and the angle AGB, in the triangle, equal to the spherical angle HDA. Therefore, by the first corollary of the lemma, that the rectangle under the tangent of the spherical angle BDH and the sine of BDC, be the greatest that can be, the cosine of BDC must be equal to the tangent of half the complement of the angle, whose cosine is to the radius, as AG

to AB, in the triangle, or as the cosine of the spherical angle ABD, to the cosine of the spherical angle BAD.

If IK be the situation of the horizon, when the solstitial point is ascending, in the quadrantal triangle AIK, the cosine of KIC is to the radius, as the cosine of IKA (= DBA) to the cosine of IAK. Therefore the cosine of BDC, when D is the point of longest ascension, is equal to the tangent of half the complement of the angle which the ecliptic makes with the horizon, when the solstitial point is ascending.

But the sine of the angle composed of DAB and twice ABD, must be less than 3 times the sine of the angle BAD. In the spherical triangle ABD, the angles BAD, ABD together exceed the external angle BDC. Therefore, in the 3d corol. of the lemma, let the angle BAN be equal to the sum of the spherical angles BAD, ABD: but here, AN is to AB as the cosine of the spherical angle ABD to the cosine of BAD; and AN is also to AB as the sine of ABN to the sine of ANB, that is, as the cosine of BAP to the cosine of NAP; consequently, since the angle BAN is equal to the sum of the spherical angles BAD, ABD, the angle NAP is equal to the spherical angle BAD, and the angle BAP equal to the spherical angle ABD; but the sine of the angle composed of NAP and twice PAB is less than three times the sine of NAP: therefore the sine of the angle composed of the spherical angle BAD and 2ABD will be less than three times the sine of the angle BAD; otherwise no such triangle DBA, as is here required, can take place, but the point A will be the point of longest ascension.

If the sine of the angle A be greater than $\frac{1}{3}$ of the radius, the point A can never be the point of longest ascension; but when the sine of this angle is less, the angle compounded of BAD and twice ABD, may be greater or less than a quadrant; and therefore the magnitude of the angle ABD, that A be the point of longest ascension, is confined within 2 limits, of which the double of one added to the angle A, as much exceeds a quadrant, as the double of the other added to that angle falls short of it; therefore double the sum of those two angles, together with twice A, makes a semicircle; and the single sum of those two angles added to A makes a quadrant.

PROB. II.—*To find when the Arc of the Ecliptic Differs Most from its Oblique Ascension.*

ANALYSIS.—If (fig. 8) BD be the situation of the horizon, when CD differs most from CB, as before, the ultimate ratio of BE to DF, will be compounded of the ratio of the radius to the sine of DG (or the cosine of DB) and of the ratio of the sine of CB to the sine of CD: but when CD differs most from CB, BE and DF are ultimately equal; therefore then the cosine of BD is to the radius as the sine of CB to the sine of CD.

Draw the arc CHI of a great circle, that DH be equal to DB; then, BH being double BD, half the sine of BH is to the sine of BD or DH, as the cosine of BD to the radius, therefore half the sine of BH is to the sine of DH as the sine of CB to the sine of CD; but the sine of the angle BCH is to the sine of BH as the sine of the angle CHB to the sine of CB; whence, by equality, half the sine of BCH is to the sine of DH as the sine of CHB to the sine of CD: but as the sine of CHB to the sine of CD, so, in the triangle CHD, is the sine of DCH to the sine of HD: consequently the sine of DCH is equal to half the sine of BCH. Hence, the difference of the angles BCH, DCH being given, those angles are given, and the arc CHI is given by position.

Further, in the triangle BCH, the base BH being bisected by the arc CD, the sine of the angle CHD is to the sine of the given angle CBD, as the sine of the given angle HCD to the sine of the given angle BCD; therefore the angle CHB is given: because that in the triangle CBH all the angles are given. The sum of the sines of the angles BCH, DCH is to the difference of their sines, as the tangent of half the sum of those angles to the tangent of half their difference; therefore the tangent of half the sum of BCH, DCH is 3 times the tangent of half BCD.

In (fig. 9) the isosceles triangle ABC, let the angle BAC be equal to the spherical angle BCD, and let AE be perpendicular to BC; also, CF being taken equal to CB, join AF: then EF is equal to 3 times EB; and as EF to EB, so is the tangent of the angle EAF to the tangent of EAB; but EAB is equal to half the spherical angle BCD: therefore the angle EAF is equal to half the sum of the spherical angles BCD, BCH; and consequently the angle CAF equal to the spherical angle DCH. Here AF is to CF as the sine of the angle ACF to the sine of CAF: and CB is to AB as the sine of the angle BAC to the sine of ACB: therefore, CF being equal to CB, and the sine of ACF to the sine of ACB, by equality, AF is to AB as the sine of the angle BAC to the sine of CAF, that is, as the sine of the spherical angle BCD to the sine of the spherical angle DCH.

Let (fig. 10) the triangle AGB have the angle ABG equal to the spherical angle CBD, and the side AG equal to AF. Then, AG is to AB as the sine of the spherical angle BCD to the sine of the spherical angle DCH, that is, as the sine of the spherical angle CBH to the sine of the spherical angle CHB: but AG is to AB also as the sine of the angle ABG to the sine of AGB; therefore, the angle ABG being equal to the spherical angle CBH, the angle AGB is equal to the spherical angle CBH: and also, when the angle ABG is greater than ABF, that is, when the spherical angle CBH is greater than the complement of half BCD, the 3 angles ABG, AGB and BAC together exceed 2 right angles.

Hence, (fig. 11) towards the equinoctial point C, where the angle CBD is obtuse, a situation of the horizon, as BD, may always be found, wherein CD more exceeds CB than in any other situation; and when the acute angle DBA is greater

than the complement of half BCD, another situation of the horizon, as KLM, may be found, toward the other equinoctial point A, wherein the arc of the ecliptic CK will be less than the arc of the equator, and their difference be greater than in any other situation. But if the angle CBA be not greater than the complement of half BCD, the arc of the ecliptic, between C and the horizon, will never be less than the arc of the equator, between the same point C and the horizon. In the two situations of the horizon, the angles CHB and KMA are equal.

Schol. 1. To find the point in the ecliptic, where the arc of the ecliptic most exceeds the right ascension, is a known problem: that point is, where the cosine of the declination is a mean proportional between the radius and the cosine of the greatest declination.

In the preceding figure, supposing the angle CBD to be right, then, because when CD most exceeds CB, the cosine of BD is to the radius as the sine of CB to the sine of CD, and, in the triangle CBD, the sine of CB is to the sine of CD as the sine of the angle CDB to the radius, also the sine of CDB is to the radius as the cosine of BCD to the cosine of BD; therefore the cosine of BD is to the radius as the cosine of the angle BCD to the cosine of the same BD, and the cosine of BD is a mean proportional between the radius and the cosine of BCD.

Schol. 2. In any given declination of the sun, to find when the azimuth most exceeds the angle which measures the time from noon, is a problem analogous to the preceding.

PROB. 3.—*The Tropic found, by Dr. Halley's method,* without any consideration of the parabola.*

The observations are supposed to give the proportions between the differences of the sines of 3 declinations of the sun near the tropic; but the sine of the sun's place is in a given proportion to the sine of the declination; therefore the same observations give equally the proportion between the differences of the sines of the sun's place, in each observation.

Now, (fig. 12), let ACE be the ecliptic, AE its diameter between ♈ and ♎, and its centre F; let B, C, D be 3 places of the sun; BG, CI, DH the sines of those places respectively. Draw CK, BL parallel to AE, which may meet HD in N and M. Then, by the observations, the ratio of DM to DN is given. Therefore, if BD be drawn to meet KL in O, the ratio of BD to OD is given; and the ratio of BD to DC is also given, these being the chords of the given angles BFD, CFD: hence the ratio of CD to DO, in the triangle CDO, is given; and consequently the angle COD will be given: which angle is the distance of the tropic from the middle point of the ecliptic between B and D: for, FPR being perpendicular to OC, and FQS perpendicular to DB, the angle QFP is equal to QOP, the points O, P, Q, F, being in a circle.

* Vide Phil. Trans. N° 215.

The Calculation.—DN : DM :: S. $\frac{1}{2}$ BFD : S. $\frac{1}{2}$ CFD :: rad. t. $< \chi$; rad. :: t. $(< \chi \backsim 45^{\circ})$ :: t. $\frac{1}{4}$ BFC : t. $\frac{1}{2}$ COD $\backsim$ $\frac{1}{2}$ DCO. If $\chi > 45^{\circ}$, $<$ COD $>$ DCO; and if $\chi < 45^{\circ}$, $<$ COD $<$ DCO.

If the intervals between the observations be so small, that the sines differ not much from the arches, the arches BC, CD may be counted in time, and the calculation may be abbreviated thus: DM : DN :: arc. BD : z (for DO); DC + z : 2DC :: $\frac{1}{4}$BC : SR., or DM $\times$ DC + DN $\times$ BD : DM $\times$ DC :: $\frac{1}{2}$ BC : SR.

XXXI. On the Digestion of the Stomach after Death. By John Hunter, F. R. S., and Surgeon to St. George's Hospital.* p. 447.

Reprinted in Mr. Hunter's Observations on the Animal Economy.

* Mr. John Hunter is a remarkable instance of the eminence which the human intellect is sometimes capable of attaining in particular pursuits of science, without the previous aid of a good general education, and even after a large portion of the youthful period of life has been suffered to pass away in vacancy and inattention.

He was brother to the celebrated Dr. William Hunter, (an account of whom is inserted in the 8th volume of these Abridgments) and was born at Long Calderwood in 1728. His father dying when he was about 10 years old, he was left to the care of his mother, who, in consequence of his dislike to school, suffered him to remain at home in idleness; so that from the time of his father's death to the age of 20, the cultivation of his mind appears to have been neglected, and he was without any regular occupation or pursuit.

At length, however, having heard much of his brother's celebrity and success in London, he expressed a desire to study anatomy. This desire was readily seconded by Dr. W. Hunter, to whose house Mr. J. H. accordingly came in 1748. It was now found that Mr. J. H. possessed talents, which only wanted a proper stimulus and direction. He soon became competent to the office of assistant dissector. While he was engaged in anatomical pursuits, he did not lose sight of surgery; a knowledge of which he acquired by attending Chelsea, Bartholomew, and St. George's hospitals; to the last of which he became house-surgeon's pupil in 1754, and house-surgeon in 1756. The year before his brother admitted him to a partnership in the anatomical lectures. His health becoming much impaired by his close application to dissections, and the making of anatomical preparations, he was advised to go abroad; and accordingly in 1760 he went as surgeon on the staff with the army to Belleisle, and afterwards to Portugal. It was in this situation that he acquired his knowledge of gun-shot wounds. In 1763 he returned to London and resumed his labours in anatomy and surgery. In 1767 he was chosen F. R. S. and some years after the same honour was confirmed on him by the Royal Society of Medicine and the Royal Academy of Surgery at Paris. In 1769 he was elected one of the surgeons to St. George's hospital. Some years after he was appointed surgeon extraordinary to his Majesty, and inspector general of hospitals, and surgeon general to the army.

Previously to the attainment of these honours, he had distinguished himself by various papers inserted in the Phil. Trans., relating to anatomy and physiology; also by some publications of a larger kind, such as his Natural History of the Teeth. It was not till a later period that he published his Treatise on the Venereal Disease, and his Observations on the Animal Economy; which last work consists chiefly of papers which had before been printed in the Phil. Trans. His Treatise on the Blood, Inflammation, and Gunshot Wounds, did not appear till after his decease; being edited by his relation Mr. Home, and accompanied with a biographical account of the author; from which account most of the particulars abovementioned have been extracted.

Mr. J. Hunter contributed largely to the advancement of physiology and comparative anatomy,

XXXII. Experiments and Observations on the Waters of Buxton and Matlock, in Derbyshire. By T. Percival, of Manchester, M. D., F.R.S.* p. 455.

Reprinted in this author's collected works.

not only by his lectures and writings, but also by the number and variety of preparations of which his museum consisted. The formation of this collection, which subjected him to vast labour and expence, was a favourite and principal object of his life. It was (as has been well remarked by Mr. Home) a grand attempt to expose to view the gradations of nature, from the most simple state in which life is found to exist, up to the most perfect and most complex of the animal creation,—man himself. The public will hear with pleasure that this valuable collection has recently (1807) been purchased by government and presented to the College of Surgeons for their use.

The various pursuits relative to anatomy, physiology, and surgery, in which Mr. J. Hunter was engaged, were followed with so much assiduity as to prove injurious to his health. After several previous attacks of the gout, he was seized in 1773 and 1776; and at irregular periods, for some years afterwards, with violent and alarming symptoms, apparently spasmodic, but proceeding (as it afterwards appeared) from an organic affection of the heart: and in one of these attacks he died suddenly, while he was at St George's hospital, in Oct. 1793, being then in the 65th year of his age.

In all his writings Mr. J. Hunter was truly original, deriving his knowledge, not from books (for he rarely consulted them) but from actual experiment and observation. Whatever may be thought of some of his opinions, we cannot sufficiently admire that talent for investigation, by which he was enabled to make the most interesting discoveries relative to the animal economy; discoveries which give him a just claim to be placed in the very first rank of those philosophers, who, in this country, have particularly contributed to the advancement of comparative anatomy and physiology.

* The following particulars concerning this distinguished medical and moral writer, are taken from the Biographical Memoirs prefixed to the edition of his works in 4 vols. 8vo. recently published (1807) by his son.

Dr. Thos. Percival was born in 1740 at Warrington, where he received his grammatical education. At the age of 21, he went to study physic at Edinburgh. He afterwards removed to Leyden, at which university he took his degree of M. D., in 1765, and visited Paris, before he returned to England. After 2 years spent in his native town, he at length decided on removing to Manchester, where he established himself in 1767 as a practising physician. About this time he published the first volume of his Essays, Medical and Experimental, some of which had been previously inserted in the Transactions of the Royal Society, of which he had been elected a member 2 years before. From this time he continued to extend his reputation as an author by various publications on subjects connected with philosophy, physic, and morality. For his ingenious communications in these several departments of science, he was elected a member of the Royal Societies of Paris and Edinburgh, and of the American Philosophical Society, &c. For many years preceding his death, Dr. P. was deprived of his eye-sight, in consequence of which he was ever afterwards obliged to employ an amanuensis. He was also subject to periodical attacks of severe head-ache; but his habitual serenity of mind was never discomposed in the slightest degree by these bodily afflictions. He died in 1804, being then in the 64th year of his age. On the monument, erected to his memory, in Warrington church, is engraved an elegant Latin inscription, written by the Rev. Dr. Samuel Parr.

Of Dr. P.'s writings on physic, the principal are his Essays beforementioned, and his Medical Ethics: those on philosophy consist of Dissertations inserted in the Trans. of the R. S., and in the Memoirs of the P. S. of Manchester; and among those which relate to Morality, not the least valuable are the Essays entitled "A Father's Instructions," &c. These, with several other compositions, constitute the 4 vols. of his works collected and edited by his son.

It was to Dr. P.'s fondness for literary and scientific intercourse that the P. S. of Manchester, (over

XXXIII. Some Account of a Body lately found in Uncommon Preservation, under the Ruins of the Abbey at St. Edmund's Bury, Suffolk; with some Reflections on the Subject. By Charles Collignon, M. D., F. R. S., and Prof. of Anat. at Cambridge. p. 465.

In February 1772, some workmen, digging among the ruins of the above abbey, discovered a leaden coffin, supposed, from some circumstances, to contain the remains of Thos. Beaufort, Duke of Exeter, uncle to king Henry the 5th. As it certainly was buried before the dissolution of the abbey, it must have been there between 2 and 300 years. It was found near the wall, on the left-hand side of the choir of the chapel of the blessed Virgin; not inclosed in a vault, but covered over with the common earth. On examining the appearance of the body, the following circumstances were remarkable, as communicated by an ingenious surgeon, on the spot, Mr. Thomas Cullum.

"The body was inclosed in a leaden coffin, surrounding it very close, so that you might easily distinguish the head and feet. The corpse was wrapped round with 2 or 3 large layers of cere-cloth, so exactly applied to the parts, that the piece, which covered the face, retained the exact impression of the eyes and nose. The dura mater was entire. The brain was of a dark ash-colour, with some remaining appearance of the medullary part. The coats of the eye were still whole, and had not totally lost their glistening appearance. There was about half a pint of a bloody black water in the thorax; and a mass that seemed to be part of the lungs. The pericardium and diaphragm were quite entire. The abdominal viscera had been taken out very clean, and the integuments and muscles stuck very close to the vertebræ of the back. This cavity looked fresher than that of the thorax. I cut into the psoas magnus, where there were evident marks of red muscular fibres. The other muscles had lost all their red colour, and were become of a dark brown. The tendons were still strong and retained their natural appearance. The hands, which are preserved in spirits, retain the nails. There were some very small holes in the coffin, out of which had run some bloody water, of an offensive smell. All the principal blood-vessels must have been cut through, in taking out the abdominal viscera: and if no ligature was made on the vessels, their contents would escape, particularly as assisted by the pressure of the cere-cloth, which is of considerable weight, and doubtless put on hot. This fluid running out of the coffin, on its being moved, might occasion the suspicion of the body being put in pickle."

We have undoubted accounts of bodies found very little changed after long

which he presided 20 years) owed its origin. He was also a chief promoter of another literary institution, the Manchester Academy, which, however, did not long flourish. And he exerted himself with much assiduity in support of the Fever Wards, and other measures that have been adopted at Manchester within these few years, for stopping the progress of infectious fevers.

interment, where there was no appearance of any art having been used. And doubtless some constitutions are more prone to putrefaction after death than others; these circumstances may be dependant on age, sex, and last disease; to which predisposing causes, thus attending persons to the grave, are to be added the soil and situation in which they are deposited. Could we be masters of all these particulars, in the few dead bodies hitherto discovered greatly free from the usual putrefaction, it would lead perhaps to the probable cause of the phenomenon, and point out a proper method of imitation. And till that is done, it is difficult to know how much merit is to be assigned to the art or mystery of embalming, and how much to the power of natural causes.

XXXIV. A Letter from Richard Pulteney, M. D., F. R. S., to Wm. Watson, M. D., F. R. S., concerning the Medicinal Effects of a Poisonous Plant exhibited instead of the Water Parsnep. p. 469.

Some circumstances having lately come to my knowledge (says Dr. P.) relating to the effect of a poisonous plant, I thought them rather too remarkable not to merit further notice; and, I address them to you with the more propriety, as you have already laid before the public some observations* concerning the deleterious qualities of the plant in question, which holds a distinguished place among the poisonous ones that are indigenous in Britain.

Mr. H——n, an attorney of this place, now upwards of 40, at the age of 15, began to be affected, after taking cold on violent exercise, as he thinks, with what is usually called a scorbutic disorder; which showed itself more particularly on the outsides of his arms, about the elbows, and on the outsides of his legs, from the knees to the ancles, as well as in botches on other parts of his body. It had the appearance of a dry branny scab or scurf, which every night fell off, more or less in scales, as is usual in leprous cases. At times it pushed out more than usual, and thickened the integuments of the limbs considerably, after which the separation of scales would become very abundant.

For several years past he had been trying a variety of things commonly recommended in such cases, particularly the quack medicine known by the name of Marcdant's drops, which he continued for near a year, without finding the least sensible relief: also an electuary of flos sulphuris and cremor tartari, which he had persevered in for near 3 years, without finding any other alteration, than that of its preventing costiveness, to which he was habitually subject. In the winter 1770, this disorder increased very rapidly, without being able to assign any reason, from any accident that had happened to him, or from any irregularity of his own in point of regimen, in which he was always very exact. At this

* See Phil. Trans. vol. 44, p. 227, and vol. 50, p. 856, of these Abridgments, vol. ix. p. 256, and vol. xi. p. 311.

time, besides the farther spreading of the irruption itself, the integuments of the legs thickened very much, and the limbs swelled to such a degree, as to render him unable to walk. The quantity of branny scurf and scales thrown off, at this time, was very great; he says, "handfuls might have been taken out of his bed every morning."

In this unhappy situation, even loathsome to himself, it was recommended to him to take the juice of water parsnep, in the quantity of one common table-spoonful every morning, fasting, mixed with 2 spoonfuls of white mountain wine. Accordingly, about the middle of January 1771, he procured a half-pint phial of what was so called, by means of the person who had recommended it, and who had assured him that he had been greatly relieved, in a similar disorder, by it. The first spoonful he took did not begin to give any great uneasiness for 2 hours, but after that time, his head began to be affected in a very extraordinary manner; a violent sickness soon succeeded, and violent vomiting; and, after he was put to bed, there came on cold sweats, and a very strong and long continued rigor, so that the people about him thought him dying for some time; but, in a few hours, all these symptoms wore off.

Such, however, had been the inveteracy of his disorder, and so strong his desire to find relief, that he determined not to desist; and, after having omitted his medicine for one day, he repeated it, in nearly the same dose, and with similar effects as to sickness and vomiting, though the uncommon sensation in his head, and the succeeding rigor, were by no means so violent. He had resolution enough to continue this dose every other morning, for more than a fortnight, and then reduced it to 3 tea-spoonfuls, which was just the half of the first dose.

Before he had taken this juice one month, he was sensible of a very great change for the better; encouraged therefore by these appearances, he persevered in its use till the middle of April, by which time his skin, though not quite cleared, yet had ceased to throw off any more scurf, was become soft, clean, and well conditioned, and, as he has repeatedly assured me, he got then into a much better conditioned state than he had experienced for many years before. From first to last, this juice never purged him; though he says, even in its reduced dose, it never failed to occasion a dizziness of the head, a nausea and sickness, which were not unfrequently succeeded by a vomiting, that always instantly relieved his head.

From the middle of April to the middle of June, he desisted from the use of the juice, but, in its stead, drank every morning for breakfast, the infusion of the leaves of the same plant, which, he says, is like common bohea tea. The infusion seldom occasioned nausea, or sickness, but always brought on a small

degree of vertigo, and in a slight manner produced the effects of intoxication from liquor.

In June he went to Harrowgate, as he had designed in the summer before. On first drinking and bathing there, he thought himself worse; and his eruptions having gradually increased during the 2 months that he staid in that place, he was convinced that those waters were of no real service to him. On his coming home he returned to the use of the infusion, and he assures me, that he again found, even by that weak preparation, a very speedy alteration for the better. From that time he continued it ever since, till his stock of the herb was exhausted; his skin is now so very little affected, that he has but here and there, on his arms and legs, a very small appearance of his disorder.

On questioning him as to the sensible qualities of this medicine, he says again, that he particularly remembers that it never once purged him; not even the first dose, which had so nearly poisoned him. He does not think that it increased the sensible perspiration, but is convinced that it was diuretic; and adds, that he thinks it occasioned, besides the increased flow of urine, a copious sediment in it, and which he believes was always wanting before. This is the plain narrative of the fact. He has assured me that no medicine or regimen, among the great variety that he has tried, ever had any sensible effect on his disorder before; and that nothing but the very early and sensible relief he experienced from this juice, could have induced him to persevere in its use, under such uneasy feelings as it never failed to produce. Indeed, he makes nothing of the lighter effects of the infusion, from which however he thinks he has likewise reaped no small benefit.

This case, the nature and inveteracy of his disorder being well known among his neighbours, was much talked of, and raised the curiosity of many people. When I first heard of it, and was informed of the smallness of the dose, and its virulent operation, I could scarcely doubt that the juice of some other plant had been administered instead of that of the water parsnep, which we know to be a safe and harmless vegetable; medical writers having directed its juice to be drunk, even to the quantity of 4 ounces for a dose: and as I know the oenanthe crocata, hemlock dropwort, to be exceedingly plentiful in this country, so much as to be more easily procured than the water parsnep itself; I thought it probable that that plant had been used in its stead. On getting a specimen, it appeared that this had been indeed the case; as also, on further inquiry, that it was the juice of the root only, and not of the leaves and stalks, that had been administered. I might here observe, that the expression from the root is not to be depended on after the plant is advanced towards its flowering state, as the root then becomes light, spongy, and almost destitute of juice.

P. S.—Mr. H——— is desirous that it should be known, that he "tried very

fruitlessly, among other methods, the drinking of tar-water and sea-water, of each of which, he says, he did not drink less than a hogshead."

XXXV. Experiments on two Dipping Needles, made after a Plan of the Rev. Mr. Mitchel, F. R. S., Rector of Thornhill, and executed for the Board of Longitude by Mr. Edw. Nairne, of Cornhill, London. p. 476.

The magnetic needles were 12 inches long, and their axes, the ends of which were of gold allayed with copper, rested on friction wheels of 4 inches diameter, each end on 2 friction wheels, which wheels were balanced with great care. The ends of the axes of the friction wheels were likewise of gold allayed with copper, and moved in small holes made in bell-metal; and opposite the ends of the axes of the needles, and the friction wheels, were flat agates, finely polished. Each magnetic needle vibrated in a circle of bell-metal, divided into degrees and half degrees, and a line passing through the middle of the needle to the ends pointed to the divisions. The minutes set down in the experiments were, by estimation, as the third of half a degree is counted 10 minutes. The instruments were carefully placed, so that the needles vibrated exactly in the magnetic meridian. The 2 needles were nearly balanced before they were made magnetical; but, by a curious contrivance of Mr. Mitchell, of a cross fixed on the axes of the needles (on the arms of which were cut very fine screws, to receive small buttons, that might be screwed nearer to or farther from the axis) the needles could be adjusted both ways, to a great nicety, after they were made magnetical, by reversing the poles, and changing the sides of the needle.

First set of experiments made April 21, 1772, by Edw. Nairne, at his house, N° 20, Cornhill.	72°	20′
	72	20
	72	20
	72	20
	72	20
	72	20
Second set of experiments, with that side of the instrument to the east, which was to the west in the first observation.	72	10
	72	15
	72	45
Here the ends of the axis touched the agates	72	45
	72	5
	72	0
Third set of experiments, in which the poles of the needle were reversed, but the same side of the instrument to the east, as in the second set of experiments, and the needle rather more magnetical, being touched with a larger set of magnets.	72	30
	72	30
	72	30
	72	30
	72	30
	72	30

Fourth set of experiments, viz. the same side of the instrument to the east, as in the first set of experiments.	72°	10′
	72	10
	72	15
	72	10
	72	10
	72	10

Fifth experiment, viz. the same end of the needle made north, as in the first set of experiments, and also the same side of the instrument to the west, as in the first set of experiments, 72° 20′.

Experiments made April 22, 1772, with the other dipping needle, the instrument being put in the same place, and with great care, in the magnetic meridian,	72°	15′
	72	10
	72	20

the needle pointed as annexed. On the 2d of these, the poles of the needle changed. And in the 3d, the side of the instrument to the east, which in the first observation was to the west.

Lest any thing magnetical should have affected the needle in Mr. Nairne's house, he took this instrument, and placed it in the middle of a large room belonging to the London Assurance in Birchin-lane,	72°	10′ or 15′
	72	20
	72	30
	72	10

and then the needle pointed as annexed. At the 3d of these, the poles of the needle changed. And at the 4th, the side of the instrument to the east, which in the first observation was to the west.

The dipping needle brought back to Mr. Nairne's, and put in the same place as before, stood at 72° 10′ +.

In the foregoing experiments, the needle was raised to an horizontal position, and left to vibrate. It was between 8 or 9 minutes before the vibration ceased. The needle brought to an horizontal position, and one grain and a half laid on the extremity of the south end, was not sufficient to keep it in that position; but the north end pointed to 35° 30′. One grain and three quarters laid on the extremity of the south end of the needle, was more than sufficient to keep it in the horizontal position, the south end then pointing 6° 45′ below 0.

END OF THE SIXTY-SECOND VOLUME OF THE ORIGINAL.

I. Discovery of the Manner of making Isinglass in Russia; with a particular Description of its Manufacture in England, from the Produce of British Fisheries. By Humph. Jackson, Esq., F.R.S. Anno* 1773. *Vol. LXIII.* p. 1.

All authors, who have hitherto delivered processes for making ichthyocolla, fish-glue or isinglass, have greatly mistaken both its constituent matter and pre-

paration. To prove this assertion, it may not be improper to recite what Pomet says on the subject, as he appears to be the principal author whom the rest have copied. After describing the fish, and referring to a cut engraven from an original in his custody, he says: "As to the manner of making the isinglass, the sinewy parts of the fish are boiled in water, till all of them be dissolved that will dissolve; then the gluey liquor is strained, and set to cool. Being cold, the fat is carefully taken off, and the liquor itself boiled to a just consistency, then cut to pieces, and made into a twist, bent in form of a crescent, as commonly sold, then hung on a string, and carefully dried." From this account, it might be rationally concluded that every species of fish which contained gelatinous principles would yield isinglass: and this seems to have given rise to the hasty conclusions of those who strenuously vouch for the extraction of isinglass from sturgeon; but as that fish is easily procurable, the negligence of ascertaining the fact by experiment seems inexcuseable. Every traveller, as well as author, who mentions isinglass, observes that it is made from certain fish found in the Danube and rivers of Muscovy. Willughby and others inform us, that it is made of the sound of the beluga; Caspar Newman, that it is made of the huso germanorum and other fish, which he has seen frequently sold in the public markets of Vienna. These circumstances make it appear the more extraordinary, that a perfect account of the manufacture of such an essential article of commerce should remain so long unrevealed.

In Mr. J.'s first attempts to discover the constituent parts and manufacture of

* Mr. Jackson died at Tottenham, June 29, 1801, at 84 years of age, where it is said he kept by him for some time before his death, a patent coffin to be interred in, and used at times to lie down in it, to show his acquaintances how it fitted him. Mr. J. kept a chemist's shop about Tower hill, London, where it seems speculating on schemes how at once to make a great fortune, he fell on that of brewing porter by certain drugs substituted as materials instead of malt and hops. With these he set up as a general instructor of the brewers, to initiate them into these new mysteries, for saving malt and hops, by giving private lessons in the art, at an enormous premium. This art it seems they have, in most instances, practised ever since in so extensive a manner, as to have produced a general complaint, that the ancient national malt liquor is miserably degenerated, with universal execrations on the memory of the man who could be so wicked as to introduce a practice, in consequence of which the natural beverage of the country has been ruined for ever. Among other pupils of Mr. J. was the late Mr. Thrale, the great brewer in the Borough, from whom alone it seems this charlatan extracted an ample fortune, as mentioned by Mr. T.'s widow, now Mrs. Piozzi, in her anecdotes of the life of Dr. Johnson,

After having by such means, in a short time, amassed an immense fortune, he was mean enough to retire to Woolwich, where he built a house, having one very large room, on purpose to practise as an ignorant trading justice, extorting the shillings for oaths, and the paltry fines for the harmless offences of the miserable poor around the parish. After thus continuing for a number of years the meanest and dirtiest practices of the worst of his profession, till his abuses of office had rendered the place too hot for his longer residence, he disposed of his property at Woolwich, and removed to carry on his operations at Tottenham, where he died, as above mentioned.

isinglass, relying too much on the authority of some chemical authors, whose veracity he had experienced in many other instances, he found himself constantly disappointed. Glue, not isinglass, was the result of every process; and though in the same view, a journey to Russia proved fruitless, yet a steady perseverance in the research proved not only successful as to this object, but, in the pursuit to discover a resinous matter plentifully procurable in the * British fisheries, which has been found, by ample experience, to answer similar purposes. It is now no longer a secret that our † lakes and rivers in North America are stocked with immense quantities of fish, said to be the same species with those in Muscovy, and yielding the finest isinglass, the fisheries of which, under due encouragement, would doubtless supply all Europe with this valuable article.

No artificial heat is necessary to the production of isinglass, neither is the matter dissolved for this purpose; for as the continuity of its fibres would be destroyed by solution, the mass would become brittle in drying, and snap short asunder, which is always the case with glue, but never with isinglass. The latter indeed may be resolved into glue with boiling water, but its fibrous recomposition would be found impracticable afterwards, and a fibrous texture is one of the most distinguishing characteristics of genuine isinglass. The reproduction of leather might with equal reason be attempted from the former.

A due consideration that an imperfect solution of isinglass, by the brewers called fining, possessed a peculiar property of clarifying malt liquors, induced him to attempt its analysis in cold subacid menstruums. One ounce and a half of good isinglass, steeped a few days in one gallon of stale beer, was converted into good fining, of a remarkable thick consistence: the same quantity of glue, under similar treatment, yielded only a mucilaginous liquor, resembling diluted gum-water, which, instead of clarifying beer, increased both its tenacity and turbidness, and communicated other properties in no respect corresponding with those of genuine fining. On mixing 3 spoonfuls with a gallon of malt liquor, in a tall cylindrical glass, a vast number of curdly masses became presently formed, by the reciprocal attraction of the particles of isinglass and the feculencies of the beer, which, increasing in magnitude and specific gravity, arranged themselves accordingly, and fell in a combined state to the bottom, through the well-known laws of gravitation; for, in this case, there is no elective attraction, as some

* Upwards of 40 tons of British isinglass have been manufactured and consumed since this discovery was first made.—Orig.

† As the lakes of North America lie nearly in the same latitude with the Caspian sea, particularly Lake Superior, which is said to be of greater extent, it was conjectured they might abound with the same sorts of fish, and, in consequence of public advertisements distributed in various parts of North America, offering premiums for the sounds of sturgeon, and other fish, for the purpose of making isinglass, several specimens of fine isinglass, the produce of fish taken in these parts, have been lately sent to England, with proper attestations as to the unlimited quantity which may be procured.—Orig.

have imagined, which bears the least affinity with what frequently occurs in chemical decompositions.

These phenomena are adduced here as correlative proofs of the impracticability of making isinglass by the previous reduction of the sinewy parts of fish into jelly; and it seems evident, that the clarifying action of isinglass depends principally on a crude minute division, not solution of its parts, which is still further confirmed, by diluting a few drops of fining with fair water in a glass; for thus the slender filaments become conspicuous to the eye, especially when assisted with a double convex lens, but these immediately disappear on an addition of hot water. As the general processes for making isinglass appear from hence illusive and erroneous, the long concealed principles of its manufacture, into the various common forms and shapes, become more obvious and comprehensive. If what is commercially termed long or short stapled isinglass be steeped a few hours in fair cold water, the entwisted membranes will expand, and re-assume their original beautiful hue,* and by a dextrous address may be perfectly unfolded. By this simple operation, we find that isinglass is actually nothing more than certain membranous parts of fishes, divested of their native mucosity, rolled and twisted into the forms above mentioned, and dried in the open air.

The sounds, or air-bladders of fresh-water fish, in general, are preferred for this purpose, being the most transparent, flexible, delicate substances. These constitute the finest sorts of isinglass; those called book and ordinary staple, are made of the intestines, and probably the peritonæum, of the fish. The Beluga yields the greatest quantity, being the largest and most plentiful fish in the Muscovy rivers; but the sounds of all fresh-water fish yield, more or less, fine isinglass, particularly the smaller sorts, found in prodigious quantities in the Caspian sea, and several hundred miles beyond Astracan, in the Wolga, Yaik, Don, and even as far as Siberia, where it is called kle or kla by the natives, which implies a glutinous matter; it is the basis of the Russian glue, which is preferred to all other kinds for its strength.

The anatomy and uses † of the sound in fish seems not yet adjusted by ichthyologists. Dossie, in his Memoirs of Agriculture, will have it to be the mesentery of the fish; but the celebrated Gouan, the latest, and perhaps the most accurate author on ichthyology, gives a more satisfactory and comprehensive account of it, under the title of La Vesicule Aërienne. Yet, if the identity of the air-bladder,

* If the fine transparent isinglass be held in certain positions to the light, it frequently exhibits beautiful prismatic colours.—Orig.

† Fishermen have a dextrous art in perforating the sound of fresh-taken cod fish with a needle, in order to disengage the inclosed air. Without this operation, the fish could not be kept under water in the well-boat, consequently could not live; but if by accident the operator wounds an artery, the fish presently dies, through the discharge of blood, to the loss of the proprietor, who thus can seldom bring it sweet to market.—Orig.

and what in English is called sound, be admitted, which seems particularly ascertained in a certain genus, viz. the asellus of Willugby, or gadus of Artedi, his description is a little erroneous with respect to its termination near the vesica urinaria; for in cod and ling, the continuation of the sound, or air-bladder, may be easily traced from thence to the last vertebra adjoining the tail. The sounds which yield the finer isinglass, consist of parallel fibres, and are easily rent longitudinally; but the ordinary sorts are found composed of double membranes, whose fibres cross each other obliquely, resembling the coats of a bladder; hence the former are more readily pervaded and divided with subacid liquors; but the latter, though a peculiar kind of interwoven texture, are with great difficulty torn asunder, and long resist the power of the same menstruum; yet, when duly resolved, are found to act with equal energy in clarifying liquors.

Isinglass receives its different shapes in the following manner. The parts of which it is composed, particularly the sounds, are taken from the fish while sweet and fresh, slit open, washed from their slimy sordes, divested of every thin membrane which invelops the sound, and then exposed to stiffen a little in the air. In this state, they are formed into rolls about the thickness of a finger, and in length according to the intended size of the staple: a thin membrane is generally selected for the centre of the roll, round which the rest are folded alternately, and about half an inch of each extremity of the roll is turned inwards. The due dimensions being thus obtained, the two ends of what is called short staple are pinned together with a small wooden peg; the middle of the roll is then pressed a little downwards, which gives it the resemblance of a heart shape, and thus it is laid on boards, or hung up in the air to dry. The sounds which compose the long staple, are larger than the former; but the operator lengthens this sort at pleasure, by interfolding the ends of one or more pieces of the sound with each other. The extremities are fastened with a peg, like the former; but the middle part of the roll is bent more considerably downwards; and in order to preserve the shape of the three obtuse angles thus formed, a piece of round stick, about a quarter of an inch diameter, is fastened in each angle with small wooden pegs, in the same manner as the ends. In this state it is permitted to dry long enough to retain its form, when the pegs and sticks are taken out, and the drying completed; lastly, the pieces of isinglass are colligated in rows, by running packthread through the peg holes, for convenience of package and exportation.

The membrane of the book sort, being thick and refractory, will not admit a similar formation with the preceding; the pieces therefore, after their sides are folded inwardly, are bent in the centre, in such manner, that the opposite sides resemble the cover of a book; whence its name; a peg being run across the middle, fastens the sides together, and thus it is dried like the former. This

sort is interleaved, and the pegs run across the ends, the better to prevent its unfolding.

That called cake isinglass is formed of the bits and fragments of the staple sorts, put into a flat metalline pan, with a very little water, and heated just enough to make the parts cohere like a pancake, when it is dried; but frequently it is over-heated, and such pieces, as before observed, are useless in the business of fining. Experience has taught the consumers to reject them.

Isinglass is best made in the summer, as frost gives it a disagreeable colour, deprives it of weight, and impairs its gelatinous principles; its fashionable forms are unnecessary, and frequently injurious to its native qualities. It is common to find oily putrid matter and exuviæ of insects between the implicated membranes, which, through the inattention of the cellar-man, often contaminate wines and malt liquors in the act of clarification. These peculiar shapes might, probably, be introduced originally with a view to conceal and disguise the real substance of isinglass, and preserve the monopoly; but, as the mask is now taken off, it cannot be doubted to answer every purpose more effectually in its native state, without any subsequent manufacture whatever, especially to the principal consumers, who hence will be enabled to procure sufficient supply from the British colonies. Until this laudable end can be fully accomplished, and as a species of isinglass, more easily producible from the marine fisheries, may probably be more immediately encouraged, it may be manufactured as follows.

The sounds of cod and ling bear great analogy to those of the acipenser genus of Linnæus and Artedi, and are in general so well known, as to require no particular description. The Newfoundland and Iceland fishermen split open the fish as soon as taken, and throw the back bones, with the sounds annexed, in a heap; but previous to incipient putrefaction, the sounds are cut out, washed from their slimes, and salted for use. In cutting out the sounds, the intercostal parts are left behind, which are much the best; the Iceland fishermen are so sensible of this, that they beat the bone upon a block with a thick stick, till the pockets, as they term them, come out easily, and thus preserve the sound entire. If the sounds have been cured with salt, that must be dissolved by steeping them in water, before they are prepared for isinglass; the fresh sound must then be laid upon a block of wood, whose surface is a little elliptical, to the end of which a small hair brush is nailed, and with a saw-knife, the membranes on each side of the sound must be scraped off. The knife is rubbed on the brush occasionally, to clear its teeth; the pockets are cut open with scissars, and perfectly cleansed of the mucous matter with a coarse cloth: the sounds are afterwards washed a few minutes in lime-water, in order to absorb their oily principle, and lastly in clear water. They are then laid upon nets, to dry in the air; but, if intended to resemble foreign isinglass, the sounds of cod will only admit of that called

book, but those of ling both shapes. The thicker the sounds are, the better the isinglass, colour excepted; but that is immaterial to the brewer, who is its chief consumer.

This isinglass resolves into fining, like the other sorts, in subacid liquors, as stale beer, cyder, old hock, &c. and in equal quantities produces similar effects on turbid liquors, except that it falls speedier and closer to the bottom of the vessel, as may be demonstrated in tall cylindrical glasses; but foreign isinglass retains the consistency of fining preferably in warm weather, owing to the greater tenacity of its native mucilage. Vegetable acids are, in every respect, best adapted to fining: the mineral acids are too corrosive, and even insalubrious in common beverage.

It is remarkable that, during the conversion of isinglass into fining, the acidity of the menstruum seems greatly diminished, at least to taste, probably not on account of any alkaline property in the isinglass, but by its inveloping the acid particles. It is likewise reducible into jelly with alkaline liquors, which indeed are solvents of all animal matters; even cold lime-water dissolves it into a pulpous magma. Notwithstanding this is inadmissible as fining, on account of the menstruum, it produces an admirable effect in other respects: for, on commixture with compositions of plaster, lime, &c. for ornamenting walls exposed to vicissitudes of weather, it adds firmness and permanency to the cement; and if common brick mortar be worked up with this jelly, it soon becomes almost as hard as the brick itself: but for this purpose, it is more commodiously prepared, by dissolving it in cold water, acidulated with vitriolic acid; in which case, the acid quits the jelly, and forms with the lime a selenitic mass, while, at the same time, the jelly being deprived, in some measure, of its moisture, through the formation of an indissoluble concrete among its parts, soon dries, and hardens into a firm body; whence its superior strength and durability are easily comprehended.

It has long been a prevalent opinion, that sturgeon, on account of its cartilaginous nature, would yield great quantities of isinglass; but on examination, no part of this fish, except the inner coat of the sound, promised the least success. This being full of rugæ, adheres so firmly to the external membrane, which is useless, that the labour of separating them supersedes the advantage. The intestines, however, which in the larger fish extend several yards in length, being cleansed from their mucus, and dried, were found surprizingly strong and elastic, resembling cords made with the intestines of other animals, commonly called cat-gut, and from some trials, promised superior advantages, when applied to mechanic operations.

II. On the Cavern of Dunmore Park, near Kilkenny, in Ireland. By Mr. Adam Walker. p. 16.

This cavern is situated in a fine plain, rising indeed here and there into small hills. The country all round abounds with limestone, and quarries of beautiful black marble, variegated with white shells. Different from those of Derbyshire and Mendip, this cave descends perpendicularly 30 yards, from the top of a small hill, through an opening 40 yards in diameter. The sides of this pit are limestone-rock, whose chinks nourish various shrubs and trees, down which the inspector must descend with great caution. In this descent, he is amused with flights of wild pigeons, and jackdaws from the cave below. When he reaches the bottom, he sees one side of this pit supported by a natural arch of rock, above 25 yards wide, under which he goes horizontally, and sees two subterraneous openings to the right and left. If he turns to the right, he makes his way over rocks and stones, coated with spar in the most whimsical shapes, and formed from the dropping roof, just as the dripping of a candle would cover a pebble. These knobs take a fine polish, are transparent, and variegated with the wildest assemblage of colouring. The Earl of Wandesford had one of them sawn into a slab, and it is as beautiful as a moco. When these petrefactions are tried with an acid, the effervescence is excessively strong; and as the earth all round is calcarious, and the stones limestone, probably the icicle figures depending from the roof, and these knobs, are thus formed. The rains that fall on the hill over this cavern, oozing through an okery calcarious earth, and the limestone roof, imbibe or dissolve their fine particles in their descent; and as this mixture can only filter through the rock exceedingly slow, the water hanging on the roof is soon dissolved by the air, and the stony particles are left behind. Hence are formed the icicle-shaped cones that hang from the roof; these growing perpetually longer, have, in many parts of the cave, met the knobs from the bottom, and formed a number of fantastic appearances, like the pillars of a Gothic cathedral, organs, crosses, &c. When the rain filters pretty fast through the roof, it falls on the rocks below, and grows there into knobs and cones, whose vertex points to those that impend from the roof.

A spectator, viewing these, cannot but conceive himself in the mouth of a huge wild beast, with ten thousand teeth above his head, and as many under his feet. The scene is indeed both pleasing and aweful; the candles burning dim, from the moisture in the air, just served to show a spangled roof perpetually varnished with water, in some places upwards of 20 yards high; in other places they crawled on all-four, through cells that will admit only one at a time. After having scrambled about 500 yards into this right hand part of the cave, they returned to day light, and then proceeded to view the left hand part. Here

were many different branches of the cavern; they tied one ball of packthread to another, as they went forward, that they might more easily find their way back. This branch is not so horizontal as the other; it declines downwards, and the openings in it are vastly wider, some being at least 100 yards wide, and above 50 high. A small rill accompanied them, which, by its different falls, formed a sort of rude harmony, well suited to the place. In a standing part of this brook, and near a quarter of a mile from the entrance, they found the bones of a hundred at least of the human race; some were very large, but when taken out of the water they crumbled away. As they could find nothing like an inscription, or earth for a burying place, they conjectured that some of the civil wars, perhaps that of 1641, might have driven the owners of these bones into this place. The tradition of the neighbourhood threw no light upon it.

Many of the rocks, on the roof and sides of this cavern, are black marble, full of white spots of a shell-like figure; and the whole neighbourhood is full of quarries of this beautiful stone, which takes a fine polish, and is used through the three kingdoms for slabs, chimney pieces, &c. In some deep and wet parts of these quarries, this elegant fossil is seen in the first stages of its formation; the shells are real, but so softened by time and their moist situation, as to be susceptible of receiving the stony particles into their pores, by whose cohesive quality, they in time become those hard white curls that give value to the marble: and it is very remarkable, and a proof that these white spots have been real shells, and thus formed, that the longer a chimney piece or slab is used, the more of those spots ripen into view.

III. On some Specimens of Native Lead found in a Mine of Monmouthshire. By Michael Morris, M.D., F.R.S. p. 20.

About the middle of July 1772, Dr. M. received 3 specimens of lead ore from Valentine Morris, Esq. of Piercefield in Monmouthshire. They were dug up in one of his fields, on making some drains, at no considerable depth; they were marked N° 1, 2, 3. On reducing to powder an ounce and a half of the ore, marked N° 3, in order to assay it, he perceived that several small bits were flatted by the pestle, which, on a further examination, proved to be native lead. Though the bits of lead are inconsiderable, yet, as they are the first that have been publicly seen in England, or perhaps in Europe, some of the best and latest writers on mineralogy declaring that they have not met with any, he thought it his duty to acquaint the R. S. with the fact, that the first account of native lead may appear in the Phil. Trans., as well as the first account of native tin.

IV. Further Remarks on a Denarius of the Veturian Family, with an Etruscan Inscription on the Reverse, formerly considered. By the Rev. John Swinton, B.D., F.R.S. p. 22.

Some years before, Mr. S. offered his thoughts on an inedited Samnite denarius,* with some Samnite Etruscan letters, as he then apprehended, on the reverse. But as the last two letters were ill preserved, or rather in part defaced, he was not entirely satisfied with his reading of the inscription to which they appertained. He has however since met with the same coin, finely preserved, in the valuable cabinet of the Rev. Dr. Milles, Dean of Exeter, with 3 letters, in the place of the two supposititious ones, on it, perfectly formed: by the assistance of which, he has been enabled to give the true reading of the inscription, and to arrive, he flatters himself, at a full and complete interpretation of it. He is now fully convinced, from the Samnite, or Samnite-Etruscan, inscription, formerly visible on the reverse of the dean's denarius, that the true legend exhibited by his coin is NI. LVFII, or LVVII, MER, equivalent to NI. LVFIVS, or LVVIVS, MERRISS, MERRIX, or MEDDIX; who seems not to have been one of the Italian generals in the social war, as he formerly supposed, but one of the chief magistrates, either of the Oscans or the Samnites, coëval with that war; there having been 2 such magistrates, answering to the 2 Roman consuls, and the 2 Carthaginian Suffetes, in both those nations.

V. A Catalogue of the Fifty Plants from Chelsea Garden, presented to the Royal Society by the Company of Apothecaries, for the Year 1771, *pursuant to the Direction of the late Sir Hans Sloane, Bart., &c.* p. 30.

This is the 50th presentation of this kind, and completes the catalogue to the number of 2500 different plants.

VI. Extract of a Letter from Mr. Ebenezer Kinnersley to Ben. Franklin, LLD., F.R.S., on some Electrical Experiments made with Charcoal. p. 38.

The conducting quality of some sorts of charcoal is indeed very remarkable. I have found oak, beech, and maple, to conduct very well; but tried several pieces of pine coal, without finding one that would conduct at all: perhaps they were made in a fire not hot enough, or not continued in it long enough. A strong line drawn on paper with a black lead pencil, will conduct an electrical shock pretty readily; but this perhaps may not be new to you.

On July the 12th, 1770, three houses in this city, and a sloop at one of the wharfs, were, in less than an hour's time, all struck with lightning. The sloop, with two of the houses, were considerably damaged; the other was the dwelling-

* Vol. xii. p. 562 of these Abridgments.

house of Mr. Joseph Moulde, in Lombard-street, which was provided with a round iron conductor, half an inch thick, its several lengths screwed together, so as to make very good joints, and the lower end 5 or 6 feet under ground; the lightning leaving every thing else, pursued its way through that, melted off 6 inches and a half of the slenderest part of a brass wire fixed on the top, and did no further damage within doors, or without.

VII. Of an Experiment made with a Thermometer, whose Bulb was painted Black, and exposed to the Direct Rays of the Sun. By Richard Watson, D. D., F. R. S.* p. 40.

During the hot weather, in the latter end of June and the beginning of July last (1772), Dr. W. made the following experiments at Cambridge. He exposed the bulb of an excellent thermometer to the direct rays of the sun, when the sky was perfectly free from clouds: the mercury rose to 108° of Fahrenheit's scale, and continued stationary. A fancy struck him, to give the bulb a black covering; this was easily effected by a camel's hair pencil and Indian ink; the mercury sunk a few degrees during the application of the coating, and the evaporation of the water; but presently after rose to 118°, or 10° in consequence of the black coat with which he had covered that part of the bulb which was exposed to the sun. If the bulbs of several corresponding thermometers were painted of different colours, and exposed at the same time to the sun, for a given period, some conjectures, respecting the disposition of the several primary colours for receiving and retaining heat, might be formed, which could not fail of being in some degree interesting.

VIII. A Report of the Committee appointed by the R. S., to Consider of a Method for Securing the Powder Magazines at Purfleet. p. 42. *Dated Aug.* 21, 1772.

The society being consulted by the Board of Ordnance, on the propriety of fixing conductors for securing the powder magazines at Purfleet rom lightning, and having done us the honour of appointing us a committee, to consider the same, and report our opinion; we have accordingly visited those buildings, and examined, with care and attention, their situation, construction, and circumstances, which we find as follows:

They are 5 in number, each about 160 feet long, and about 52 feet wide, built of brick, arched under the roof, which in one of them is slated, with a coping of lead 22 inches wide on the ridge from end to end; and the others, as we were informed, are soon to be covered in the same manner. They stand parallel to each other at about 57 feet distance, and are founded on a chalk rock,

* The present Bishop of Llandaff.

about 100 feet from the river, which rises in high tides within a few inches of the level of the ground, its brackish water also soaking through to the wells that are dug near to the buildings.

The barrels of powder, when the magazines are full, lie piled on each other up to the spring of the arches; and there are 4 copper hoops on each barrel, which, with a number of perpendicular iron bars, (that came down through the arches, to support a long grooved piece of timber, wherein the crane was usually moved and guided to any part where it was wanted) formed broken conductors within the building, the more dangerous from their being incomplete, as the explosion from hoop to hoop, in the passage of lightning drawn down through the bars among the barrels, might easily happen to fire the powder contained in them. But the workmen were removing all those iron bars (by the advice of some members of this society, who had been previously consulted); a measure we very much approve of.

On an elevated ground, nearly equal in height with the tops of the magazines, and 150 yards from them, is the house where the board usually meet. It is a lofty building, with a pointed hip roof, the copings of lead down to the gutters, from which leaden pipes descend at each end of the building into the water of wells of 40 feet deep, for the purpose of conveying water forced up by engines to a cistern in the roof. There is also a proof-house, adjoining to the end of one of the magazines, and a clock-house, at the distance of feet from them, which has a weathercock on an iron spindle, and probably some incomplete conductors within, such as the wire usually extending up from a clock to its hammer, the clock, pendulum rod, &c.

The blowing up of a magazine of gunpowder by lightning, within a few years past, at Brescia in Italy, which demolished a considerable part of the town, with the loss of many lives, does, in our opinion, strongly urge the propriety of guarding such magazines from that kind of danger; and since it is now well known, from many observations, that metals have the property of conducting lightning, and a method has been discovered of using that property for the security of buildings, by so disposing and fixing iron rods, as to receive, and convey away, such lightning as might otherwise have damaged them; which method has been practised near 20 years in many places, and attended with success, in all the instances that have come to our knowledge, we cannot therefore but think it advisable to provide conductors of that kind, for the magazines in question.

In common cases, it has been judged sufficient, if the lower part of the conductor were sunk 3 or 4 feet into the ground, till it came to moist earth; but this being a case of the greatest importance, we are of opinion that greater precaution should be taken. Therefore we should advise, that at each end of

each magazine, a well should be dug in or through the chalk, so deep as to have in it at least 4 feet of standing water. From the bottom of this water should arise a piece of leaden pipe, to or near the surface of the ground, where it should be strongly joined to the end of an upright iron bar, an inch and half diameter, fastened to the wall by leaden straps, and extending 10 feet above the ridge of the building, tapering from the ridge upwards to a sharp point, the upper 12 inches of copper, the iron to be painted. We mention lead for the underground part of the conductor, as less liable to rust in water and moist places; in the form of a pipe, as giving greater stiffness for the substance; and iron for the part above-ground, as stronger, and less likely to be cut away. The pieces, of which the bar may be composed, should be screwed strongly into each other, by a close joint, with a thin plate of lead between the shoulders, to make the joining or continuation of the metal more perfect. Each rod, in passing above the ridge, should be strongly and closely connected by iron or lead, or both, with the leaden coping of the roof, by which a communication of metal will be made between the 2 bars of each building, for a more free and easy conducting of the lightning into the earth.

We also advise, in consideration of the great length of the buildings, that 2 wells, of the same depth with the others, should be dug within 12 feet of the doors of the 2 outside magazines; that is to say, one of them on the north side of the north building, the other on the south side of the south building; from the bottom of which wells, similar conductors should be carried up to the eaves, there joining well with a plate of lead, extending on the roof up to the leaden coping of the ridge, the said plate of lead being of equal substance with that of the coping. We are further of opinion, that it will be right to form a communication of lead from the top of the chimney of the proof-house to the lead on its ridge, and thence to the lead on the ridge of the corridor, and thence to the iron conductor of the adjacent end of the magazine; and also to fix a conductor from the bottom of the weathercock spindle of the clock-house, down on the outside of that building, into the moist earth.

As to the board-house, we think it already well furnished with conductors, by the several leaden communications abovementioned, from the point of the roof down into the water, and that, by its height and proximity, it may be some security to the building below it; we therefore propose no other conductor for that building, and only advise erecting a pointed iron rod on the summit, similar to those before described, and communicating with those conductors.

To these directions we would add a caution, that in all future alterations or repairs of the buildings, special care be taken that the metalline communications be not cut off or removed. It remains that we express our acknowledgments to Sir Charles Frederick, Surveyor-general of the Ordnance, for the obliging

attention with which he entertained and accommodated us on the day of inquiry. Signed, H. Cavendish, William Watson, B. Franklin, J. Robertson.

Mr. Wilson's Dissent from Part of the preceding Report.—I dissent from the report above, in that part only which recommends that each conductor should terminate in a point. My reason for dissenting is, that such conductors are, in my opinion, less safe than those which are not pointed. Every point, as such, I consider as soliciting the lightning, and thus, not only contributing to the increase of every actual discharge, but also frequently occasioning a discharge where it might not otherwise have happened. If therefore we invite the lightning, while we are ignorant what the quantity or the effects of it may be, we may be promoting the very mischief we mean to prevent. Whereas if, instead of pointed, we make use of blunted conductors, those will as effectually answer the purpose of conveying away the lightning safely, without that tendency to increase or invite it.

My further reasons for disapproving of points, in all cases, where conductors are judged necessary, are contained in a letter addressed to the Marquis of Rockingham, and published in the Phil. Trans., vol. 54. There are other reasons also, which I have to offer, for rejecting points on this particular occasion; and which were mentioned at the committee. Those I shall lay before the R. S. at another opportunity, for the benefit of the public. Aug. 21, 1772.

IX. Observations on Lightning, and the Method of Securing Buildings from its Effects: In a Letter to Sir Charles Frederick, Surveyor-General of the Ordnance, and F. R. S., By Benj. Wilson, F. R. S., &c. Dated Dec. 8, 1772. p. 49.

Sir,—Your station, as Surveyor-General of his Majesty's Ordnance, being such as makes the subject of this paper particularly interesting to you, I presume an apology for this address will be wholly unnecessary. On an application of the Board of Ordnance to the R. S., in July last, a committee was appointed, to consider of the properest method for securing the magazine at Purfleet from mischief by lightning: which committee reported to the council of that learned body, what they thought necessary to be done on that occasion. The council afterwards transmitted to the board a copy of that report, together with another paper written by myself, in consequence thereof. For, during the consideration of that business, some doubts having arisen in my mind, with regard to the propriety of points, which were proposed to terminate the top of each conductor; and those doubts being founded on some experiments and observations, I could not consistently subscribe to that report, nor suppress my opinion, on a subject of such importance.

Whatever may be the sentiments of others respecting those doubts, yet, being the result of my mature consideration, I thought it my duty to propose them to the committee; and further to express my dissent, in writing, to that particular part of their report: giving, at the same time, some of the principal reasons for such dissent; and referring them, for further satisfaction on this subject, to a letter which is already published in the transactions of the R. S. Agreeable to the declaration at the end of the above dissent, I shall now proceed to offer my further reasons for objecting to pointed conductors.

Experience, which is our best guide in all physical inquiries, but particularly in electrical ones, every day convinces me, that we know but little of that subtile fluid, which operates so secretly, and at the same time so powerfully, on the earth, and its atmosphere I confess that I am even now less acquainted with the principle of its action, than I thought I was 20 years ago; the smallest differences in the circumstances of our experiments, frequently causing very material differences in their results. And perhaps no one, who has not applied his mind closely to inquiries of this kind, could conceive how the pointing a piece of metal, or not, should make any material difference in the experiment. The electrician has it always in his power to convince any one of the fact, who, through inexperience, may be inclined to entertain the least scruple about it: for even from those experiments to which it was thought proper to appeal at the committee, it appeared, that the difference in the effects on this fluid, between pointed and blunted metal, is as 12 to 1.

A thunder cloud therefore, according to that reasoning, (the circumstances of it being supposed to be nearly similar to what is called the prime conductor in those experiments), if it acted at 1200 yards distance on a point, would require a blunted end to be brought within the distance of 100 yards; and beyond those limits, would pass over it, without affecting it at all. On this occasion permit me to observe, that the longer the conductors are above any building, the more danger is to be apprehended from them; as they will in that case approximate nearer in their effects to those that are pointed. And that is one reason why I was not for advising the proposed conductors at Purfleet, to be so high as 10 feet above the magazines, and more particularly on that building called the board-house, which stands considerably higher than the magazines themselves.

But, before we advance further into this subject, it may be proper to show the reasons for introducing a pointed apparatus, when the experiment on lightning was first proposed: what good consequences were derived from that experiment: and why, on further experiments and observations, such points ought now to be laid aside, when our intention is not to make electrical experiments, but by the means of conductors, to preserve buildings from the dangerous effects of lightning.

Dr. Franklin, in his conjectures, that lightning and electricity were one and the same fluid, considered how he should invite, or bring down and collect the lightning, so as to make experiments on it. And he concluded, from observation, that the likeliest method would be, to make use of such an apparatus for the purpose, as was most susceptible of electric effects; or, in other words, such an apparatus as would receive the electric fluid with the greatest ease. Repeated experiments taught him, that metals had the property of receiving that fluid, with more ease than other substances. He also learnt, from the like experience,

that metals, by being pointed, were rendered still more susceptible of receiving it. And therefore he proposed an experiment to be tried, "Whether it was not in our power to invite, or bring down the lightning, by an apparatus, consisting of an electric stand, and an iron rod, 20 or 30 feet in length, rising upright from the middle of the stand, and at the top, terminating in a very sharp point." This apparatus was recommended to be put on some high building, with the expectation, that if a thunder cloud should happen to pass near this apparatus, some quantity of the lightning deposited therein would probably be collected in the rod, by means of the very sharp point, and the electrical stand at the foot of the rod.

That this contrivance answered the end he first proposed, we have had sufficient evidence. And it is no wonder if, after this great discovery, we find him, and other electricians, pursuing new experiments of this kind, and raising those points higher into the air, to collect still greater quantities of that fluid which occasions lightning. Nor need we be surprized, after knowing that lightning could be brought down from the heavens by so simple an apparatus, and after experiencing its subtile effects to be similar with the electric fluid, that the Americans, and others, on Dr. Franklin's recommendation, adopted the principle of securing their buildings from its dangerous effects, by raising above their houses rods of iron, very sharply pointed, and applying wires from the ends of those rods, down the outside of their houses to the ground. But though there appeared many arguments at that time in favour of such conductors, yet experiments and observations, at last, induced Dr. Franklin to alter his opinion in respect to those wires, and to substitute in their place rods of iron: still retaining the principle of having the rods at the top sharply pointed; and many of the Americans, as well as Europeans, approved of the alteration, as appeared afterwards, from constructing their conductors accordingly.

About that time great attention was given, and many new experiments were made, in consequence of the frequent dangerous effects, which lightning was observed to produce in some valuable buildings, by rending and dashing to pieces very large stones and timbers, which were connected together by cramps and bars of iron: and at other times breaking and melting part of those rods, and sometimes exploding wires, even of a considerable thickness, like so much gunpowder. From careful observations of these extraordinary appearances, produced by violent shocks of lightning; and on making other experiments relating to a certain resisting power in, or on, all bodies, which appears to act against the attacks of lightning, as well as against the electric fluid, philosophers were enabled to assign the reason, and, it is apprehended, on a solid foundation, why conductors should be made of metal, in preference to all other materials; as the power of resisting such attacks is less in metals than in wood, stone, or marble. And that this resistance might be the more simple and uniform, it

appeared the most eligible to have the conductors made of one continued piece of metal only, and of an equal diameter throughout. But what that diameter ought to be, depended on other circumstances, some of which are taken notice of in a former paper, referred to above, which I laid before the R. S.

By this historical sketch, we see the propriety of Dr. Franklin's introducing points, and the advantage philosophy has derived from them: by ascertaining that lightning and electricity are one and the same fluid: which appears to be diffused every where, at least on this earth and in the atmosphere. But when curiosity, which I apprehend was one of the first motives for introducing points to invite the lightning, was satisfied; and experience had taught us, that we had it in our power to collect that fluid which occasions it: and when the principle of its action was from experiments thus investigated and ascertained, this matter of invitation, viz. by using points, ought, in my opinion, to have ceased;* because a greater quantity of lightning, than we have yet experienced, may chance to attack us. For we are so far from knowing how great the magazine of lightning may be in the heavens, or in the earth, when it is ready to discharge itself, either by one or more explosions, that we are ignorant even of the quantity actually discharged, whenever any stroke of lightning visits us. Nor can the ablest philosopher fix the limits of the greatest discharge that may possibly happen.

Seeing then how vain it is to look for any thing like absolute security, in all cases, it surely behoves us to proceed with caution. And it is for that reason I have always considered pointed conductors as being unsafe, by their great readiness to collect the lightning in too powerful a manner. And lest the conductors, without such points, should be too slender for very violent attacks, in places of great consequence, I have always recommended the having them above 4 times larger in diameter, than what are commonly made use of, that our security may be the greater, by opening a larger passage for any extraordinary discharge, and so far lessening the danger to be apprehended from it.

I ought not, in this place, to omit taking notice of a paper, containing some further experiments and observations, which were produced at the committee, to show, among other things, that pointed metals were more disposed to receive the lightning, by virtue of a repelling principle, in the lightning as well as the electric fluid, which acted on the natural quantity of the fluid contained within the metal, at a considerable distance from the point, causing, if I may be allowed the expression, a kind of vacuum therein; but I suppose the author means to a certain distance only.

So far from disputing this philosophy, I readily admit the fact. But, I am afraid, every attempt to prove that pointed conductors may be so disposed to

* Unless where the electrician, like Professor Richmann (who was killed by it) at his own hazard, chuses to make further observations on lightning.—Orig.

recive this fluid more readily, will not mend the argument in the least; because, the more we lessen the power of resisting, even supposing the whole conductor to be in that state, the more we increase the power of invitation.

In regard to other experiments, with locks of cotton,* which are acted on in a particular manner by the apposition of points, and the conclusions drawn from thence, in favour of pointed conductors, as causing similar effects on the fragments or small clouds, which, hanging below the thunder clouds, have been supposed a kind of stepping stones, for the lightning to pass on, towards the earth: such pointed conductors being supposed to occasion those fragments to retire up into the cloud whence they were suspended; and on that account, to prevent a stroke from lightning, which might otherwise have happened, I shall, for the present, wave entering in this philosophy, as I could wish the conjecture to be reconsidered: because I apprehend it is liable to many objections, which to enumerate would carry me beyond the proper bounds of such a paper as this. However, if the same opinion should again be offered, and brought in argument, it may be worth while to enter more deeply into the inquiry.

If those gentlemen, who argued at the committee for the necessity of points, could have made it appear, that such points draw off, and conduct away, the lightning imperceptibly and by degrees, without causing any explosion, during a thunder storm (which seems to have been once the opinion of Dr. Franklin) I should readily have subscribed to their report. But experience shows us, that the fact is otherwise: there being many instances, where violent explosions of lightning have happened to conductors that were sharply pointed. And 3 in particular, the accounts of which are inserted in a publication of Dr. Franklin's,† where the points were dissipated, or destroyed; and a small part of an iron rod melted next the points of one of them; and also at the several crooked ends of the rods below, where they were hooked on to each other, and formed the conductor belonging to Mr. Maine in North America. But as those letters are long, and contain several other curious facts, I shall reserve them, together with some further observations on the nature and power of that resisting principle, which is found to act so sensibly against the attacks of the electric fluid, or lightning, to some future dissertation.

There is no building, that I know of, more exposed to this kind of danger, than the Eddystone lighthouse, as it stands upon a rock in the sea, several miles from land. The fixing of a conductor to that building, was thought highly proper; and the fixing of a point on it, as highly improper. It was therefore resolved on to put up a conductor without a point, that no more lightning might be unnecessarily solicited to the building, and that all the lightning, which acci-

* Dr. Franklin's experiments.—Orig.

† Dr. Franklin's Experiments, p. 394, 416, 417, &c.—Orig.

dentally fell on it, might be conveyed away without injuring it. This conductor was fixed 12 years ago, and the building has since received no injury from lightning.*

There is another edifice of great consequence, I mean St. Paul's church, which stands much exposed, from its height, to accidents by lightning. The dean and chapter of that cathedral thought it an object deserving the serious attention of the Royal Society. A committee was therefore appointed, in consequence of their application: and proper conductors were put up, in the several places where they were thought necessary, from the top of the lantern to the sewers underground. And notwithstanding particular care was taken, to have the additional metal either of a considerable diameter, or an equal quantity of it formed into other shapes, for the conveniency of the several places; yet part of those conductors, consisting of iron, in the stone gallery, showed marks of their having been made considerably hot, if not absolutely red, by a stroke of lightning which happened in March last (as appears by a letter which I communicated to the R. S. from one of the vergers of that church, Mr. Richard Gould) who had examined the conductors the morning following, along with Mr. Burton of the same cathedral,† and that the appearances were in general as the verger's

* A former building erected for the same purpose, upon this rock, was set on fire by lightning.—Orig.

† Mr. Gould acquaints us in his letter, that he examined the four conductors in the lantern and stone gallery of St. Paul's church, the morning after the lightning happened. That no marks whatever appeared on the conductor to the south, which was the first he attended to. That he next examined the conductor to the west, and observed a thick rust lying on the pavement in the stone gallery, as if it had been cleaned off, from the conductor, with a tool: that several parts of the iron appeared black, particularly the screws or nuts: something like the effects left by gunpowder on iron or steel, or a smoky fire. That the conductors to the north showed no marks, no more than that to the south. But that on examining the conductor to the east, he found stronger marks abundantly, than on the west conductor, it being much blacker; particularly on the nut and screws: the rust lying in great quantities on the pavement. And the extreme part of the conductor, that goes into the water trunk, seemed like a piece of iron newly taken out of a forge by a smith, without working it on the anvil.

Mr. Gould has since added to the account in his letter, some circumstances which I apprehend ought not to be omitted. He says, that where the end of the conductor, on the east side, points towards the water trunk, a stone surrounds part of it, leaving an interval, half an inch wide or more, between them, and about 4 or 5 inches long, which is a little more than the breadth of the conductor. That this interval was filled up with dirt, and had been so for some time, occasioned by frequent showers of rain washing the pavement in the stone gallery. That, after the lightning happened, he observed a hole was made through the dirt, one quarter of an inch in diameter, and about 2 inches in length. That the hole was close to the iron; and that, on stooping down his head, he perceived a very disagreeable smell of sulphur from the stone, dirt, and conductor, particularly the last.

On hearing this account, Mr. Delaval and myself, a few days ago, went and examined the conductors again; but more carefully than before. For, on causing the stone to be removed, which covered the top of the water trunk, we had an opportunity of examining near 2 feet more of the iron which points to the water trunk, than we could perceive before this stone was removed. When we

letter related them to me. Mr. Delaval and myself attended, about a week afterwards, to observe them, and their particular situations, with the circumstances attending them; when we were very well satisfied with his account, notwithstanding it had rained in the interim for 3 days together.

It is worthy of note, that those conductors did not terminate in a point, nor was any point put upon the cross at the top. And yet Dr. Franklin was of that committee. If points are so essential to our safety, why was not the reason enforced at the committee, for having them on that capital edifice? For my part, I think it was a happy circumstance that there was no point fixed on the top of the church, to solicit a greater quantity of lightning at that moment, than what fell on the conductors, circumstanced as they were: as that quantity was great enough to heat so considerably a bar of iron, near 4 inches broad, and about half an inch thick.

This powerful effect reminds me of another instance still more extraordinary, which happened in Martinico, and is related by Captain Dibden, where a bar of iron, one inch in diameter, was by a violent shock of lightning reduced in one part of it to the thickness of a slender wire only. See Phil. Trans., vol. liv. p. 251.* Since then we are at all times ignorant of the quantity of lightning in the earth and its atmosphere; and the difference in the effects, between blunted and pointed ends, in causing a discharge in our electrical experiments, appears to be as 1 to 12; it is easy to comprehend the very great danger this noble fabric has probably escaped, by having no pointed apparatus upon it.

From the above observations, I am naturally led to consider a part of the proceedings of the committee, respecting the magazines at Purfleet; when a certain number of conductors, with tapering points at the top, were resolved on, as necessary to protect the several buildings where the powder is deposited. For it was agreed on at the same meeting, that the board-house, which is a large building for the use of the board officers, and which stands considerably higher than the magazines, as was observed above, did not require any point at the top: because it was apprehended to be perfectly secure, by reason of the copings on the roof, the gutters and pipes to carry off the water being all of lead: and further, because those pipes communicated with two wells, which always contained water.

I was not a little surprized at this last resolution; which appeared to be so

observed, that the conducting iron did not touch the lead. We likewise observed, that there was a very thick coat of rust all over that part of the iron; particularly at the end next the lead, where the water entered the trunk. As the necessity of attending to these circumstances will be obvious to any one, who is but in the least degree acquainted with these researches, the danger of neglecting them will be seen in the strongest light, by the gentlemen of the committee who recommended the conductors for the security of that cathedral.—Orig.

* Abridgment, vol. xii. p. 149.

inconsistent with the former. Because, if points were necessary in one place, they ought to be so in another. And on the other hand, if the board-house is secure by the leaden accidental conductors, which have no points, why ought not the magazines to be equally secure, when put into the same circumstances? I therefore enforced the inconsistency of such a resolution in the strongest terms. Notwithstanding which, the gentlemen at that time thought proper to confirm their resolution. However, at the next meeting of the committee, I observed that they had been pleased, in the mean time, to make an amendment in favour of points for the board-house; which amendment was no sooner proposed than approved of.

Why my observation was rejected at the preceding meeting, I must leave to the judgment of others. But it certainly carries an appearance, as if manifest contradiction, on further reflection, must have been the cause of that alteration.

And I am inclined to believe, from some gentlemen of the committee expressing their opinion, 'of its being a matter of mere indifference whether blunted or pointed conductors were made use of,' that they have not considered this subject with all the due attention, which so important an object deserves. For if our experiments show, that points, from the nature of their shape, and other circumstances attending them, resist the attacks of this fluid less than blunted ones; and that blunted conductors, of proper dimensions, are sufficient to convey away the lightning safely, whenever it attacks them; why should we have recourse to a method, which is at best uncertain; and which some time or other may be productive of the most fatal effects?

But perhaps no argument can be brought with more force against the principle of points, than Dr. Franklin's own words, which are published in his experiments, p. 481, where he declares positively, 'buildings, that have their roofs covered with lead, or other metal, and spouts of metal continued from the roof into the ground to carry off the water, are never hurt by lightning; as whenever it falls on such a building, it passes in the metals, and not in the walls.'

This is the case with the British Museum, a building also of considerable consequence, where there are no other conductors, than what are formed by the copings, gutters and pipes, which are all of lead, and communicate with the ground. Now it is from the great quantity of metal contained in the several pipes, together with the other circumstances attending them, that I considered that building, in a former paper laid before the R. S., as being sufficiently secured from those dangerous accidents. But if any gentleman should be disposed to entertain a doubt about it, or indeed of any other part of my reasoning on this subject, a declaration of those doubts may be attended with good consequences, as they will necessarily open the door to a more minute investigation.

I have now, sir, gone through the reasons which I proposed to lay before the

R. S. for the rejecting of points. And I am very sorry, in the course of this letter, to have been under the necessity of mentioning any differences in opinion, which passed between the members of the committee, to whom this important matter was referred. I think however I shall stand excused to the society, and the public, when it appears, as I hope it now sufficiently does, what my motive has been; namely, to state clearly, and impartially, the objections which I conceived to lie against pointed conductors: and to diclose without any reserve, the principles on which such objections are grounded.

P. S. Mr. Delaval, who was one of the committee, has given me leave to insert his opinion on this subject; which is this: That he concurs with me in thinking that such conductors as are elevated higher than the buildings to which they are applied, or are pointed at the top, are improper and dangerous. He was desirous of delivering his opinion at the committee: but as the meetings of it were held in the summer only, his absence from London prevented his attendance.

X. A Letter to Sir John Pringle, Bart., Pr. R. S., on pointed Conductors. Dated Dec. 17, 1772. p. 66.

SIR,

Having heard and considered the objections to our report, concerning the fixing pointed conductors to the magazines at Purfleet, contained in a letter from Mr. Wilson to Sir Charles Frederick, and read to the R. S., we do hereby acquaint you, that we find no reason to change our opinion, or vary from that Report. We have the honour to be, &c.

H. Cavendish, W. Watson, B. Franklin, J. Robertson.

XI. Astronomical Observations made at Chislehurst in Kent. By the Rev. F. Wollaston, F.R.S. p. 67.

Mr. W. the last year sent to the Society an account of the going of an astronomical clock with a wooden pendulum, for the year preceding; with such observations as he had made in this place; the latitude of which is 51° 24′ 33″ N., and its longitude 4′ 39″ = 18ˢ.6 in time, east of the Royal Observatory at Greenwich. The rate of the clock deduced from the observations of this last year, will not be found so uniform as the foregoing. To what cause to ascribe it, he is not certain. He thinks not to heat: perhaps to the great drought of the summer. However, its acceleration or retardation was not desultory, but sufficient to be depended on for any intermediate time. The clock was cleaned in November; and when set up again, lost, between the 18th and 28th, at the rate of 7ˢ.8 per day. The regulator was then altered, and clock set, and from that time never meddled with. Then follow several observations on the going of

the clock, for the year 1771. Next, observations on the barometer and thermometer, not necessary to be reprinted. The next observation is a solar eclipse, Oct. 25, 1772, the end observed at 20^h 36^m 34^s apparent time. Then several occultations of stars by the moon. Then follow observations of the eclipses and occultations of Jupiter's satellites, also of his belts.

XII. On the Early Cultivation of Botany in England; and some Particulars of John Tradescant, a Great Promoter of that Science, as well as of Natural History, in the last Century, and Gardener to King Charles I. By Dr. Ducarel, F.R.S., F.S.A. p. 79.

The sciences we know are subject to revolutions. But is it not a very extraordinary one that botany, so useful to mankind, and so well known to the ancients, should for some ages abandon Europe, and remain almost unknown there till the 16th century; when it is supposed to have suddenly revived; and has since, by the industry of the moderns, been brought to the highest perfection? The truth however is, that botany returned into England long before this æra. It was brought back here by the Saxons; since whose time, it has always flourished, more or less, in this kingdom. Dr. D. founds his opinion on the authority of the 4 following Saxon manuscripts.

Two in the Bodleian Library, viz.

N° 4125. Herbarium Saxonicum.

5169. Liber medicinalis MS. continens virtutes herbarum Saxonicè.

And two others in the Harleian Library, viz.

N° 5066. entitled, Herbarium Saxonicè.

585. Tractatus qui ab Anglo-Saxonibus dicebatur LIBER MEDICINALIS: scil. L. Apuleii Madaurencis Libri de Virtutibus Herbarum, Versio Anglo-Saxonica.

This Lucius Apuleius of Medaura was a famous Platonic philosopher, who flourished about A. D. 200. From this time he has met with no MS. concerning botany, till the 13th century, when Bishop Tanner mentions three MSS. on this subject, written by Gilebertus Legleus, sive Anglicus, a physician, who flourished in the year 1210, entitled,

1. De Virtutibus Herbarum, MS. Bodl. Digb. 75. 2. Gilberti Liber de Viribus et Medicinis Herbarum Arborum, et Specierum, MS. olim Monast. Sion. 3. De Re Herbaria, lib. i.

The bishop likewise mentions one John Arden, a famous surgeon, who lived at Newark in Nottinghamshire from 1349 to 1370, as the author of a MS. (now extant in Sir Hans Sloane's library), entitled, Volumen Miscellaneorum de Re Herbaria, Physica, et Chirurgica.

In the Ashmolean library are the following MSS. viz.

(N° 704.) entitled, A Treatise of Chirurgery, with an Herbal, &c. in Old English, 4to, 1438. And another, N° (7709.) called, an Herbary, &c. written alphabetically, according to the Latin names, in 1443. And (N° 7537.) entitled, A Book of Plants and Animals, delineated in their natural colours on vellum, Old English, A. D. 1504.

Mr. Ames, in his Typographical Antiquities, p. 470, informs us, that in the year 1516, a folio, entitled, 'The Greate Herball,' was printed in Southwark by Peter Treveris; and this Dr. D. believes, is the oldest English herbal now extant in print.

To come to later times: Mr. Gough (in his British Topography, p. 61) informs us, 'That, before the year 1597, John Gerrard, citizen and surgeon of London, seems to be the first who cultivated a large physic garden, which he had near his house in Holborn, where he raised 1100 different plants and trees.' He might have added, that Gerrard had another physic garden in Old-street, containing a great variety of plants; a printed catalogue of which is to be found in the libraries of the curious. But Gerrard had a famous contemporary, who greatly advanced that valuable science, and of whom but little has hitherto been said by the modern biographers.

John Tradescant is the person meant. And an attempt to revive the memory of this once eminent botanist and virtuoso may not be displeasing. John Tradescant was, according to Anthony Wood, a Fleming, or a Dutchman. We are informed by Parkinson, that he had travelled into most parts of Europe, and into Barbary; and, from some emblems remaining on his monument in Lambeth church-yard, it plainly appears that he had visited Greece, Egypt, and other eastern countries. In his travels, it is supposed he collected not only plants and seeds, but most of those curiosities of every sort, which, after his death, were sold by his son to the famous Elias Ashmole, and deposited in his Museum at Oxford.

When he first settled in this kingdom, cannot at this distance of time be ascertained; perhaps it was towards the latter end of the reign of Queen Elizabeth, or the beginning of that of King James the First. His print, engraven by Hollar before the year 1656, which represents him as a person very far advanced in years, seems to countenance this opinion. He lived in a great house at South Lambeth, where there is reason to think his museum was frequently visited by persons of rank, who became benefactors to it: among these were King Charles the 1st, to whom he was gardener. Henrietta Maria his queen, Archbishop Laud, George Duke of Buckingham, Robert and William Cecil, Earls of Salisbury, and many other persons of distinction. John Tradescant may therefore be justly considered as the earliest collector in this kingdom, of every thing that was curious in natural history, viz. minerals, birds, fishes, insects, &c. He had also a good collection of coins and medals of all sorts, besides a great variety of uncommon rarities. A catalogue of these, published by his son, contains an enumeration of the many plants, shrubs, trees, &c. growing in his garden, which was pretty extensive. Some of these plants are, if not totally extinct, at least become very uncommon, even at this time. A list of some remarkable ones

introduced by him, is inserted below.* And this able man, by his great industry, made it manifest, in the very infancy of botany, that there is scarcely any plant in the known world, that will not, with proper care, thrive in this kingdom. When his house at South Lambeth, then called Tradescant's Ark, came into Ashmole's possession, he added a noble room to it, and adorned the chimney with his arms, impaling those of Sir William Dugdale, whose daughter was his 3d wife, where they remain to this day.

It were much to be wished, that the lovers of botany had visited this once famous garden, before, or at least in the beginning of the present century. But this seems to have been totally neglected till the year 1749, when Dr Watson and Dr. Mitchel favoured the R. S. with the only account now extant, of the remains of Tradescant's garden. In it, Dr. Watson seems to confine the extent of it to that now belonging to Mr. Small's house. Dr. D. believes it was otherwise; and, on account of the great number of plants, trees, &c. is inclined to think that Tradescant's garden extended much farther. Bounded on the west by the road, on the east by a deep ditch, still extant, it certainly extended a good way towards the north, and took in not only Dr. D.'s orchard and garden, but

* From Parkinson's Garden of Pleasant Flowers, printed in 1656.

1. 'Pseudonarcissus aureus maximus flore pleno, sive roseus Tradescanti. The greatest double yellow bastard daffodil, or John Tradescant's great rose daffodil. This daffodil was primarily introduced by John Tradescant, and for its extreme beauty, may well be entitled the glory of daffodils.' page 102.

2. 'Moly Homericum, vel potius Theophrasti. The greatest moly of Homer, 141.

3. 'Moly Indicum, sive Caucason. Indian moly, ibid. Both the above molys are natives of Spain, Italy and Greece, and were procured from thence by John Tradescant, and flourished with him, in his garden at Canterbury.' (Should be South Lambeth).

4. 'Ephemerum Virginianum Tradescanti. John Tradescant's spider-wort of Virginia. This spider-wort is of late knowledge, and for it the Christian world is indebted unto that painful industrious searcher and lover of all nature's varieties John Tradescant.' 152.

5. 'Gladiolus Byzantinus. Corn-flag of Constantinople. With this species John Tradescant observed many acres of ground in Barbary overspread, 190.

6. 'Elleborus albus vulgaris. White hellebore. This groweth in many places in Germany, and also in some parts of Russia, and in such plenty, that John Tradescant observed quantity sufficient to load a good ship with the roots, 346.

7. 'Nardis montana tuberosa. Knobbed mountain valerian. Discovered in a botanic excursion by J. Tradescant, 388.

8. 'John Tradescant introduced a new strawberry, with very large leaves, from Brussels; but in the course of 7 years, could never see one berry completely ripe. 528.

9. John Tradescant procured a new and great variety of plums from Turkey, and other parts of the world. 575.'

10. 'The Argier, or Algier apricot. This, with many other sorts, John Tradescant brought with him, returning from the Argier voyage, whither he went with the fleet that was sent against pirates, Anno 1620.' 579.

Thus far Parkinson; but whether or no these plants bear his name at this period, I can no more pretend to assert than that all the species therein mentioned are even now existing in our gardens.—Orig.

also those of two or three of his next neighbours; and some ancient mulberry trees, planted in a line towards the north, seem to confirm this conjecture.

When the death of John Tradescant happened, Dr. D. has not been able to discover, no mention being made of it in the register book of Lambeth church. A singular monument was erected in the south-east part of Lambeth church-yard, in 1662, by Hester, the relict of John Tradescant the son, for himself and the rest of this family, which is long since extinct.* This once beautiful monument has suffered so much by the weather, that no just idea can now, on inspection, be formed of the north and south sides. But this defect is happily supplied from 2 fine drawings, preserved in Mr. Pepys's library at Cambridge. We see on the east side, Tradescant's arms; on the west, a hydra, and under it a skull; on the south, broken columns, Corinthian capitals, &c. supposed to be ruins in Greece, or some other eastern countries; on the north, a crocodile, shells, &c. and a view of some Egyptian buildings. Various figures of trees, &c. in relievo adorn the four corners of this monument.

XIII. Of the Intense Cold in the Months of Jan. 1767, *and* 1768, *and Nov.* 1770, *observed at Franeker. By J. H. Van Swinden, Prof. Philos. in the Acad. of that place, and Fellow of the Harlem Soc. From the Latin.* p. 89.

In Jan. 1767, Fahrenheit's thermometer showed degrees as follow: Jan. 6^d 8^h a. m. 16°; at 10^h p. m. 2°.—Jan. 7^d $7\frac{1}{2}^h$ a. m. $-$ 2°; at noon $+$ 12°; at 7^h p. m. 5°; at 9^h p. m. 7°.—Jan. 8^d $7\frac{1}{2}^h$ a. m. 12°.—Here the lowest was $-$ 2° or 2 below 0, the mercury being all sunk down into the bulb. But M. Vander Bild observed it as low as $-$ 5.

In 1768, Jan. 3 it was as low as $-$ $3\frac{1}{2}$.

In 1770, the lowest in Nov. was $+$ 9, viz. on the 20th, at $7\frac{1}{2}^h$.

XIV. An Inquiry into the Quantity and Direction of the Proper Motion of Arcturus; with some Remarks on the Diminution of the Obliquity of the Ecliptic. By Tho. Hornsby, M.A., Savilian Prof. of Astron. Oxford, and F.R.S. p. 93.

By comparing ancient with the best modern observations, it appears that some of the fixed stars have a proper motion, independent of any motion hitherto known in our own system; or that, in other words, the angular distances of the fixed stars have not always continued the same, and in some of them the alteration is so very considerable, as to be easily perceived in the course of a few years, with instruments accurately made, and nicely adjusted. Of all the stars visible

* John the grandson, buried 15th September 1652.
John the son, buried 25th April 1662.
Hester, widow of John Tradescant, buried 6th April 1678. From the register of Lambeth Church. —Orig.

in our hemisphere, the variation in the place of Arcturus is the most remarkable, and such as cannot possibly be attributed to the uncertainty of observation. It has accordingly been noticed by many astronomers: in particular, Dr. Halley mentions it in N° 355 of the Phil. Trans: M. Cassini, in the Memoirs of the Academy of Sciences for 1738, p. 231, has shown, that there is a variation of 5′ in the latitude of that star, between his own time and that of Tycho, in an interval of a century and a half; and M. le Monnier, in the Memoirs of the Academy of Sciences for 1767, p. 417, proves, that the latitude of Arcturus varies at the rate of 2″ every year; and that the longitude decreases at the rate of 60″ in 100 years.* But as an inquiry both into the true quantity and into the direction of this motion, has not hitherto been made public, Mr. H. proposes to give some account of his own observations, made expressly with this view in the years 1767 and 1768, with a transit instrument of 44 inches, and a moveable mural quadrant of 33 inches, both constructed by Mr. Bird, and of the conclusions resulting from a comparison between them and some observations made by Mr. Flamsteed in 1690.

It may perhaps be objected, that the differences of right ascension, as determined by Mr. Flamsteed's mural instrument, are not to be depended on, from the very nature of his instrument. Mr. Flamsteed was himself too good an observer not to be aware of this, and accordingly, in the Prolegomena to the 3d volume of the Historia Cœlestis, p. 132, he informs us in what manner he determined the error of the plane at different distances from the zenith. By distributing these errors in the best manner, Mr. H. is of opinion, that the error of the plane of his instrument may be supposed to decrease uniformly at the rate of half a second in time for every degree of zenith distance from 28° to 60°, the error being 39″ at the former, and 23″ at the latter, by which quantity stars passed the horary wire, in his instrument, before they came to the true meridian. It should seem also, that the error continued nearly the same from 60° to 75°, being at the latter only 22″; but that it decreased irregularly from 75° to 85°, viz. 1″ in time for each degree from 75° to 80°, and 0″.4 for each degree from 80° to 85°. The mural arc was fixed on a stone pier, the southern part of which was found to settle yearly, whence the error of the line of collimation to the south necessarily became every successive year greater and greater. As Mr. Flamsteed seems not to have had any method of adjusting his instrument by a plumb-line, these errors must have been irregular at different seasons of the same year, and were perhaps never truly determined. But as the observations here referred to were made on the same day, and within the compass of an hour,

* See also the Memoirs of the Academy of Sciences for 1769, p. 21. See also Astronomiæ Fundamenta, by the Abbé de la Caille; who, in reducing his observations of Arcturus, supposes the annual motion of declination in that star = 19″, p. 169, and 187.—Orig.

they are probably not affected with this latter error. We are at present concerned with the difference of 2 zenith distances, and not with the absolute quantity of them. The conclusions may indeed be affected with an error in the divisions; and from the examination which Mr. H. has been able to make, he is of opinion that the arc of Mr. Flamsteed's instrument was not of the proper quantity; and that, though the observations generally erred in defect, yet in some parts they erred in excess.

On Feb. 14, 1690, Mr. Flamsteed observed, that a small star, of the 7th or 8th magnitude, whose place is not determined in the British catalogue, and which star was named by him Infra Arcturum, preceded Arcturus 3^s in time, or $3^s.3$, when an allowance is made for the error of the plane of the instrument $= 0' \, 42''.6$, and was $26' \, 30''$ to the south of Arcturus.* By a mean of 8 observations made at Oxford, on or near June the 10th, 1767, with the transit instrument, and with a refracting telescope of 8 feet, furnished with a micrometer; the difference of right ascension was $1' \, 8''.75$ of a degree, the star following Arcturus; and by a mean of 3 observations, the extremes differing only $3''$, the small star was $23' \, 55''.0$ to the south of Arcturus.

The right ascension of Arcturus and the small star being nearly the same, the change in declination ought to be so likewise. But, from the observed difference in declination, the right ascension of the two stars must vary unequally, though with a very small difference. Accordingly it appears from computation (in which the annual precession is supposed $= 50''.35$, the obliquity of the ecliptic at the middle of the interval of the time $= 23^\circ \, 28' \, 30''$, and the right ascensions and declinations of the two stars taken at a mean between the times of observation) that the variation of Arcturus in right ascension was $3270''.6$, and of the small star $3277''.6$, in 77.287 years. Therefore the right ascension of Arcturus alters less than that of the star; and consequently Arcturus should in 1767 have followed the star by $42''.6$. But the star was observed to follow Arcturus by $1' \, 8''.75$. The right ascension therefore of Arcturus has increased less than that of the star, or Arcturus has moved westward $1' \, 51''.35$ in 77.287 years; and has gone southward $2' \, 35''$ in the same time, supposing the small star not to have moved, which is highly probable.

On the same day the difference of right ascension in time between the star η Bootis and Arcturus was $21^m \, 32^s$ of mean solar time, $= 5^\circ \, 24' \, 2''.2$, when a proper allowance is made for the going of the clock, and for the error of the plane of the instrument, and the difference of declination was $50' \, 45''.6$, when an allowance is made for refraction. On the 24th, 26th and 29th, of May, and the 9th of June of the year 1768, Mr. H. determined the difference in

* This is the only observation of that star made by Mr. Flamsteed.—Orig.

right ascension to be 21^m 27^s of sidereal time by the two former observations, and 21^m $26\frac{3}{4}^s$ by the two latter, the difference in declination being 49′ 48″.7, by a mean of the observations in May, the extremes differing only 4 seconds. It appears from computation, that between the times of observation the variation of η Bootis in right ascension was 3371″.7, and 1417″.3, in declination; of Arcturus 3311″.7 in right ascension, and 1347″.9 in declination; the difference of variation in right ascension is 1′ 0″, and of declination 1′ 9″.4; by the former the difference in right ascension was diminished, and in declination increased by the latter, agreeably to the places of the two stars. The difference in right ascension therefore in 1768, if neither of the stars had moved, should have been = 5° 23′ 2″.2, and 51′ 55″ in declination; but they were observed to be 5° 21′ 43″.4, and 49′ 48″.7. Arcturus therefore, by this observation, has in 78.257 years gone 1′ 18″.8 to the west, and 2′ 6″.3 to the south, supposing η Bootis not to have any proper motion.

On the 5th of April, 1691, the difference in right ascension between η Bootis and Arcturus was 21^m 33^s of mean solar time, = 5° 24′ 14″.0; and the difference of declination 50′ 45″.6, as in the preceding example. The difference of variation in right ascension is 59″.1, and in declination 1′ 8″.4. The difference of right ascension therefore at the latter end of May, 1768, should have been 5° 23′ 14″.9, and 51′ 54″.0 in declination; but, according to observation, they were 5° 21′ 43″.4, and 49′ 48″.7. Arcturus therefore, according to this observation, has moved 1′ 31″.5 to the west, and 2′ 5″.3 to the south in 77.120 years.

On the 4th of May, 1691, the difference of right ascension between η Bootis and Arcturus was 21^m 33^s of mean solar time, = 5° 24′ 14″.3, when allowance is made for the going of the clock and the error of the plane of the instrument, and the difference of declination on the 3d of May = 50′ 50″.6. According to computation, those differences should have been 5° 23′ 15″.2 and 51′ 59″.0 respectively; but they were observed to be 5° 21′ 43″.4 and 49′ 48″.7. Arcturus therefore, in 77.071 years, has moved 1′ 31″.8 westward, and 2′ 10″.3 southward. N. B. The zenith distance of Arcturus, as determined by Mr. Flamsteed, on the 4th of May, is manifestly erroneous.

On the 27th of May, 1692, η Bootis preceded Arcturus in right ascension by 21^m $32^s.5$ of mean solar time, = 5° 24′ 10″.1, the difference of declination being 50′ 50″.6. In 75.978 years the difference of right ascension should have been 5° 23′ 11″.8, and 51′ 58″.0 in declination; but those differences were observed to be 5° 21′ 43″.4 and 49′ 48″.7. Arcturus therefore has moved 1′ 28″.4 westward, and 2′ 9″.3 southward.

On the 27th of May, 1692, Arcturus preceded π Bootis in right ascension by 24^m $35^s.5$ of mean solar time, = 6° 9′ 32″.2, when an allowance is made for the going of the clock and the error of the plane of the instrument, the differ-

ence of declination being 3° 2′ 28″.9. On the 24th and 26th of May, and 5th of June, 1768, the difference of right ascension between the same stars observed at Oxford was 24^m $44^s.58$ of sidereal time, = 6° 11′ 9″.1, the difference of declination being 2° 58′ 24″.2. In 1768, the difference of right ascension should have been $2^s.7$ greater, = 6° 9′ 34″.9; and the difference of declination 1′ 31″.7 less, = 3° 0′ 57″.2. But they were observed to be 6° 11′ 9″.1, and 2° 58′ 24″.2. Arcturus therefore in 75.978 years has, by a comparison with this star, moved 1′ 34″.2 westward, and 2′ 33″.0 southward.

Again, the difference of declination between Arcturus and π Bootis was observed to be 3° 2′ 33″.9 on the 14th of February, 1690, when the difference of right ascension between these two stars was not observed by Mr. Flamsteed. It appears by computation, that the difference of variation in declination between the times of observation was 1′ 34″.5, by which quantity the difference of declination was diminished, and should therefore in 1768 have been 3° 0′ 59″.4. But it was 2° 58′ 24″.2 by actual observation. Arcturus therefore by this observation has moved southward 2′ 35″.2 in 78.255 years.

By the foregoing comparisons Arcturus appears to have moved as in the following table.

	Years.	Westward.	Southward.
By the small star Feb. 14, 1690,	in 77.237	1′ 51″.35	2′ 35″.0
η Bootis Feb. 14, 1690,	in 78.257	1 18 .8	2 6 .3
η Bootis April 5, 1691,	in 77.120	1 31 .5	2 5 .3
η Bootis May 4, 1691,	in 77.071	1 31 .8	2 10 .3
η Bootis May 27, 1692,	in 75.978	1 28 .4	2 9 .3
By π Bootis May 27, 1692,	in 75.978	1 34 .2	2 33 .0
π Bootis Feb. 14, 1690,	in 78.257	not obs.	2 35 .2

As the quantity of the motion of Arcturus southward in declination, as deduced from a comparison with η Bootis, differs considerably from the quantities given by the small star and π Bootis, which agree very nearly together, Mr. H. compared η Bootis with some of the neighbouring stars, as that star, though of the 3d magnitude only, may have a small motion of its own. On the 14th of Feb. 1690, the difference of declination between η and π Bootis was observed by Mr. Flamsteed to be = 2° 11′ 47″.8. By computation, that difference in 1768 should have been 2^m $43^s.9$ less, = 2° 9′ 3″.9: but it was actually observed to be 2° 8′ 34″.3 only. The star η Bootis therefore appears by this comparison to have moved southward 29″.6 in 78.257 years.

On the 27th of May, 1692, η Bootis was observed by Mr. Flamsteed to be 2° 11′ 37″.8 to the north of π Bootis, which quantity should by computation be 2′ 39″.1 less in 1768, or 2° 8′ 58″.7. But it was found to be 2° 8′ 34″.3. The star η therefore appears to have moved southward 24″.4 in 75.978 years.

On the 25th of April, 1693, η Bootis was observed to be 40′ 20″.8 to the south of f Bootis, a star of the 6th magnitude; and by Mr. H. that difference

was observed to be 42′ 37″.5, by taking the mean of two observations on the 24th and 26th of May, 1768, differing only 4″.7. According to computation, the variation of η Bootis in declination during the interval of the two observations was 1359″.3, and of Bootis 1256″.0; therefore the difference of variation in declination was 1′ 43″.3, by which the distance of the stars was increased. The difference in declination therefore in 1768, if neither of the stars moved, should have been 42′ 4″.1; but it was observed to be 33″.4 greater, by which quantity therefore η Bootis must have moved southward in 75.052 years.

By reducing all the foregoing deductions to 78 years, Arcturus appears to have moved,

	Westward.	Southward.
By the small star, Feb. 14, 1690	1′ 52″.380	2′ 36″.43
η Bootis Feb. 14, 1690	1 18 .541	2 5 .88
η Bootis April 5, 1691	1 32 .557	2 6 .75
η Bootis May 4, 1691	1 32 .906	2 11 .87
η Bootis May 27, 1692	1 30 .752	2 12 .74
By π Bootis May 27, 1692	1 36 .707	2 37 .07
π Bootis Feb. 14, 1690	not observed	2 34 .69

But the star η Bootis appears also to have moved southward.

By π Bootis Feb. 14, 1690	0′ 29″.503
π Bootis May 27, 1692	0 25 .049
By f Bootis April 5, 1693	0 34 .712
By a mean	0 29 .755

As Arcturus appears to have moved southward of η Bootis 2′ 9″.31, by taking a mean of the 4 quantities resulting from the comparisons with that star; and as η Bootis has also moved southward of some of the neighbouring small stars by 29″.755 in the same time, Arcturus on the whole has moved 2′ 39″.06 to the south, by the comparisons with η Bootis only; and therefore, by taking a mean of all the results, Arcturus has altered its right ascension less than the neighbouring stars by 1′ 33″.97 in 78 years, in which time it has also moved 2′ 36″.81 to the south of the same stars.

In order to see how far the motion of right ascension is to be depended on, which is deduced from the above comparisons, Mr. H. selected and computed the following observations, made at Shirburn castle with a transit instrument of 5¼ feet, placed exactly in the plane of the meridian, and consequently more to be relied on than those made with a mural instrument. By a mean of 5 observations, made on the 7th, 12th, 23d, 24th, and 31st of May, 1741, o. s., the difference in right ascension between η Bootis and Arcturus was 5° 22′ 38″.9, the extremes differing only 4″.4 of a degree. The difference in the variation of right ascension to the end of May, 1768, is 20″.5, by which the ascensional difference is diminished. It should therefore have been 5° 22′ 18″.4; but it was observed to be 5° 21′ 43″.4. Therefore in 27 years Arcturus has moved westward 35″.0.

On the 16th and 20th of May, 1744, the difference in right ascension be-

tween η Bootis and Arcturus was 5° 22′ 30″.0 by each of the observations, which difference should have been, supposing neither of the stars to have any proper motion, 5° 22′ 11″.7 in May 1768. But it was found to be 28″.3 less; by so much therefore had Arcturus moved westward in 24 years.

On the 24th of May, and 8th of June, 1746, the difference in right ascension between the same stars was 5° 22′ 26″.2, by taking a mean of the two observations; that difference should have been 5° 22′ 9″.5 in 1768. But it was observed = 5° 21′ 43″.4. Arcturus therefore in 22 years has moved 26″.1 to the west.

Lastly, on the 16th of April, and 27th and 28th of May, 1747, the difference in right ascension between η Bootis and Arcturus, by taking a mean of the 3 observations, was 5° 22′ 25″.0. By computation the variation in the difference of right ascension was 16″.0, by which the ascensional difference should have been diminished, and = 5° 22′ 9″.0. But by observation it was found = 5° 21′ 43″.4; Arcturus therefore by this last observation appears to have gone 25″.6, westward.

By the observations therefore at Shirburn castle, Arcturus appears to have gone westward, as in the annexed table; in the last column of which are contained the quantities resulting from the observations of each year, reduced to 78 years.

1741	0′ 35″.0	1′ 41″.11
1744	0 28 .3	1 31 .97
1746	0 26 .1	1 32 .59
1747	0 25 .6	1 34 .90
	Mean	1 35 .14

The mean of all the observations, when reduced to an interval of 78 years, is 1′ 35″.14, which differs only 1″.17 from the mean of the other comparisons.

As then the proper motions of Arcturus westward in right ascension = 1′ 33″ 974, and 2′ 36″.81 in declination southward, seem well established, the real motion of Arcturus is inclined in an angle of 30° 56′ to the west of the meridian or horary circle, and to be in that direction 3′ 2″.81 in 78 years, or at the rate of 2″.343 in a year. As this direction of its motion is nearly perpendicular to the plane of the ecliptic, the latitude of Arcturus must diminish yearly almost in the same proportion; and its longitude will alter less than that of other stars, though not so considerably as its right ascension. The proper motion of Arcturus then, in right ascension westward, being 1″.205, and in declination 2″.005, its annual precession in right ascension is 41″.108, and in declination 19″.133; and the true right ascension of Arcturus, on Jan. 1, 1773, is 211° 19′ 47″.4, and declination north 20° 22′ 23″.3.

As none of the other principal stars have been found to have a motion so considerable as this, though many of the stars of the first magnitude, as for instance, Sirius, Procyon, α Aquilæ, α Orionis, as also β Aquilæ of inferior magnitude, do really vary their positions, and perhaps all of the first order will hereafter be found to have a proper motion, we may fairly conclude, that Arcturus is the nearest star to our system, visible in this hemisphere. If therefore the

annual parallax of the fixed stars can ever be discovered, that is, if the diameter of the annual orbit bear a sensible proportion to the distance of the nearest fixed star, it is most likely to be discovered from the observations of Arcturus. The system of the world, considered in an enlarged sense, and agreeable to the idea we may entertain of an all-powerful benevolent Creator, may be taken to occupy the whole abyss of space, and to consist of an assemblage of bodies, having different magnitudes, and emitting various degrees and modifications of light. The apparent change of situation visible from the planet which we inhabit, and which revolves round one of the great bodies constituting a part of the general system, as a centre, may be owing either to the motion of our own system in absolute space, or, if our system should be at rest, to a real motion in the stars themselves: whence the angular distances of the stars must vary in proportion to the velocity of those motions, or to the direction of those motions with respect to ourselves. I have reason, at present, says Mr. H., to believe that a small motion may be discovered in the star *o* ceti, and perhaps in other stars that vary in degrees of brightness, which the diligence of future astronomers will discover, and perhaps in less time than at first sight might seem necessary, when we consider the several improvements which have of late been made in the methods of observing the heavenly bodies.

As the motion of Arcturus in declination, the quantity of which we have thus endeavoured to ascertain, has been often acknowledged, it is matter of wonder that some astronomers, by comparing either the altitude or zenith distance of the sun's limb with Arcturus, without previously settling the quantity of that star's motion in declination, or at least doing it indirectly, should endeavour to determine whether the obliquity of the ecliptic has remained constant, or still continues to diminish, as it should seem to have done for many centuries past, from the observations of successive astronomers. M. Cassini, and M. le Monnier, have both practised this method, and are of opinion, that the obliquity of the ecliptic has not altered; or, if it has altered, that the quantity of its alteration is not near so considerable as has been imagined by some celebrated astronomers. By observing for several days, before and after the solstice, the altitude or zenith distance of the sun's limb, and that of a star situated near the same parallel, the differences to be remarked in process of time, in the distances of the sun from that star (the motion of the star in declination being allowed for during that interval of time), will be the quantity by which the sun will have approached to or have receded from the star. If the star were absolutely a fixed point, and the observations sufficiently numerous, that, by taking a mean, the necessary and unavoidable errors in observation might either be considerably diminished, or almost annihilated, the method might be practised to great advantage. But as the star Arcturus had a proper motion, and its apparent place was continually

varying from the effect of the nutation of the earth's axis; as the limb of the sun was sometimes approaching to, and sometimes receding from, the star, by a kind of libratory motion, from the effect of the nutation; and also as the obliquity of the ecliptic itself was, in all probability, continually diminishing; from a combination, and as it were involution of these motions, no certain conclusion could be drawn, since, in the space of a few years, the apparent obliquity may be the same, and yet the mean obliquity may have diminished, or perhaps, in the space of a few years, the obliquity may appear to have increased, when it may really have become less. Whereas, by reducing the observations to their mean position, and by assigning to each known cause its proper and allowed effect, a regularity and uniformity must necessarily take place, as far at least as is consistent with the unavoidable errors in observing.

M. Cassini, in the Memoirs of the Academy of Sciences for 1767, acquaints us, that, in 1748, the apparent distance of Arcturus from the upper limb of the sun, at the time of the solstice, was the same as in 1766.

In 1748, distance of Arcturus from the sun's solstitial limb	3°	13′	36″	40‴
Altitude of Arcturus	61	41	17	0
Therefore the apparent solstitial altitude	64	55	13	40
In 1766, distance of Arcturus from the sun's solstitial limb	3	19	32	0
Altitude of Arcturus	61	35	42	0
Therefore the apparent solstitial altitude	64	55	14	0

The same astronomer has, in the Memoirs for 1759, p. 325, communicated the following conclusions.

	Dist. of the star from the sun's limb.			Reduction.		Solstitial distance.		
1763. June 14	3°	7′	29″	+ 11′	1″	3°	18′	30″
15	3	10	16	+ 8	13	3	18	29
25	3	15	40	+ 2	48	3	18	28
July 1	2	59	1	+ 19	22	3	18	23
2	2	54	55	+ 23	33	3	18	28
3	2	50	18	+ 28	8	3	18	26
					Mean.	3	18	27

Mr. Le Monnier, in the Memoirs for 1762, p. 269, has published the annexed distances of Arcturus from the limb of the sun, reduced to the solstitial point, with a view to obtain differences in the apparent obliquity of the ecliptic: and, from the observations made with the gnomon of St. Sulpice, and communicated by Mr. Le Monnier, in the same volume, it should seem that that astronomer is of opinion, that the obliquity of the ecliptic has no other variation than what the nutation of the earth's axis will occasion; and that therefore we must either abandon the absolute diminution of the ecliptic, or at least suppose it extremely small, since, in the space of 18 years, it has not produced a sensible alteration.

1738	3°	10′	15″	
1740	3	11	5	
1742	3	11	48	
1763	3	18	40	with the mural 5-feet quad.
	3	18	35	with the large mural instru.

As the results of the observations only, and not the observations themselves, are communicated, Mr. H. observes, that there is a very considerable difference between the conclusions of the two astronomers for the same year 1763, and declares his suspicion, that if the apparent (for such he apprehends them to be) were reduced to the mean distances, they would probably afford a confirmation of the diminution of the ecliptic. For the following observations of the sun's zenith distance, made at Shirburn castle, near the summer solstices of the years 1743, 1746, 1748, and 1766, and of Arcturus in the years 1743, 1746, and 1766, when reduced to their mean state at the solstice, do not confirm the assertion of Mr. Cassini, but are an evident and absolute proof that the obliquity of the ecliptic has sensibly diminished during an interval of 23, and even of 18 years.

The observations of 1743 were made with a mural quadrant of 5 French feet, constructed by the late Mr. Sisson: but as the linear divisions were found to be somewhat less accurate than was expected, and as the body of the quadrant was not framed with proper strength and solidity, Mr. Bird was employed in the summer of the year 1745, by the Earl of Macclesfield, (the body of the instrument having been strengthened by screwing a large and broad plate of brass on the cross bars), to put a set of points on the limb between the 90 and 96 arches of linear divisions. By these operations the line of collimation was found to have varied, and to be = 6″.3, by which the zenith distances were given too small, by the positive divisions, from the end of 1746 to the end of June 1751, when Mr. Bird bisected the spaces between the points which he had formerly added in 1745. But after the year 1751, the error of the line of collimation was = 2″.6, as appears from observations of γ Persei, β and γ Draconis, by which the zenith distances are also given too small; and in that state the instrument continued to the year 1767, when a new set of wires was put into the telescope, and the line of collimation thereby altered. The error of the line of collimation from 1743 to 1745 cannot directly be ascertained, for want of zenith observations; but, from some indirect methods, it should seem that the error was as nearly as possible = 2″, to be added to the observed zenith distances.

Thus by a series of observations of the sun's zenith distances, from the 7th to the 27th of June, 1743, when corrected for his semidiameter and refraction, the medium of all the 12 days, when reduced all to the solstice, is as follows:

The Mean	28°	10′	58.2″
Sun's parallax			—4.1
	28	10	54.1
Nutation			+6.7
	28	11	0.8
Line of collimation			+2.
Mean solstitial zenith dist., 1743	28	11	2.8

A similar set of 14 days observations, from May 31, to June 30, 1746, give

For the Mean	28°	10	52.5″
Sun's parallax			—4.1
	28	10	48.4
Nutation			+9.4
	28	10	57.8
Error of the line of collination			+6.3
Mean solstitial zenith dist., 1746	28	11	4.1

A like set of 8 days observations, from the 15th to the 29th of June, 1748, give,

For the mean	28°	10′	55.8″
Sun's parallax			—4.1
	28	10	51.7
Nutation			+6.1
	28	10	57.8
Error of the line of collimation			+6.3
Mean solstitial zenith dist., 1748	28	11	4.1

And again another set of 10 days observation, from the 11th to the 29th of June, 1766, give,

For the mean	28°	11′	10.5″
Sun's parallax			—4.1
	28	11	6.4
Nutation			+7.6
	28	11	14.0
Error of the line of collimation			+2.6
Mean solstitial zenith dist., 1766	28	11	16.6

Then follow a series of 10 days observations of the zenith distances of Arcturus, from May 12 to July 1, 1743; when these are corrected for refraction, aberration, nutation, precession, the medium of all is as follows: viz.

The mean	31°	7′	33.6″
Error of the line of collimation			+2
Mean zenith distance of Arcturus, June 21, 1743	31	7	35.6
Mean zenith distance of the sun's centre, June 21, 1743	28	11	2.8
Mean distance of Arcturus from the sun's centre, 1743	2	56	32.8

In like manner, 5 days observations, from June 4 till Oct. 9, 1746, give for

The mean	31°	8′	26.4″
Error of the line of collimation			+6.3
Mean zenith distance of Arcturus, June 21, 1746	31	8	32.7
Mean zenith distance of the sun's centre, June 21, 1746	28	11	4.1
Mean distance of Arcturus from the sun's centre, 1746	2	57	28.6

Again, a like set of 4 days observations, from May 13, to June 23, when corrected, give for

The mean	31°	14′	50.3″
Error of the line of collimation			+2.6
Mean zenith distance of Arcturus, June 21, 1766	31	14	52.9
Mean distance of the sun's centre, June 21, 1766	28	11	16.6
Mean distance of Arcturus from the sun's centre, 1766	3	3	36.3

From the foregoing observations, it appears that the mean solstitial zenith distance in summer was as here annexed: and by comparing the 3 former with the latter, the variation of the obliquity of the ecliptic in 100 years is as is expressed in the last column of the table.

				Variat. in 100 years.
1743..	28°	11′	2.8″	60″
1746..	28	11	4.1	62.5
1748..	28	11	4.1	69.4
1766..	28	11	16.6	

By comparing the distance of Arcturus from the sun's centre in 1743, with the same distance as observed in 1766 (an allowance being made for the proper motion of the star during the interval, as also for its variation in declination arising from the precession of the equinoxes), it appears that its distance is 17″.3 less than it would have been, if the distance of the sun's centre from the equator had remained unvaried. By that quantity therefore the obliquity of the ecliptic has altered in 23 years; which is at the rate of 75″.2 in 100 years. By comparing, in like manner, the distance in 1746, the obliquity of the ecliptic has diminished 15″.6 in 20 years, or 78″ in 100 years.

Distance in 1743	2°	56′	32.8″	In 1746,	2°	57′	28.6″
Motion of the star in decl. southward		7	20.8			6	23.3

Computed distance in 1766	3°	3′	53.6″	In 1746,	3°	3′	51.9″
Observed distance in 1766	3	3	36.3		3	3	36.3
Variation of obliquity			17.3				15.6

The foregoing deductions prove, Mr. H. thinks, beyond all doubt, that the obliquity has become less; but as the interval of time between the two terms of comparison is so short, that the errors committed in observing, may bear a sensible proportion to the small quantities just now found, and which perhaps are somewhat too large; Mr. H. has recourse to Mr. Flamsteed's observations, and compares them with observations made by himself, in the course of the last and present years. For this purpose he reduced all the observations of the sun, made in 1690, from May 26 to June 24, o. s. and also all the observations of Arcturus, made in the same year, to their mean position at the summer solstice of that year. The observations, together with his own made at Oxford, are as follow: These observations of the zenith distances, both of the sun and Arcturus, when corrected as the preceding, and the medium of all taken, give first, for the sun's zenith distance at the solstice,

The mean	28°	0′	54.2″
Error of the line of collimation	—	1	30
	27	59	24.2
Sun's parallax			—4.1
	27	59	20.1
Nutation			+9.5
Mean solstitial zen. dist. of the sun's centre, June 11, 1690, o. s.	27	59	29.6
And for the zenith distance of Arcturus,			
The mean, January 1, 1690, o. s.	30°	39′	34″
Precession to June 11, 1690			+8.4
Mean zenith distance of Arcturus, June 11, 1690	30	39	42.4
Mean solst. zen. dist. of the sun's centre, June 11, 1690	27	59	29.6
Mean distance of Arcturus in declin. from the sun's centre, 1690	2	40	12.8

And for that of Arcturus the same year,			
Mean zen. dist. of Arcturus, Jan. 1, 1772	31°	22′	29.8″
Precession to June 21, 1772			+9
Mean zen. dist. of Arcturus, June 21, 1772	31	22	38.8
Error of the line of collimation			+4.2
True mean zenith dist. of Arcturus, June 21, 1772	31	22	43
Mean zen. dist. of ☉'s centre, June 21, 1772	28	17	5.4
Mean dist. of Arcturus in declination from ☉'s centre, June 21, 1772	3	5	37.6

Again, for the sun's zenith distance in 1771,			
The mean is	28°	17′	8.7″
Sun's parallax			—4.1
	28	17	4.6
Nutation			—6.8
	28	16	57.8
Error of the line of collimation		+	4.8
Mean solstitial zenith distance of the sun's centre, 1771	28	17	2.6
And the same for 1772, gives, for			
The mean	28°	17′	13.4″
Sun's parallax			—4.1
	28	17	9.3
Nutation			—8.7
	28	17	0.6
Error of the line of collimation			+4.8
Mean solstitial zenith distance of the sun's centre, 1772	28	17	5.4

Mean dist. in June 1690	2°	40′	12.8″
Precession, &c. to June 1772		+26	16.4
Computed dist. in June 1772	3	6	29.2
Observed dist. in June 1772	3	5	37.6
Diminution of obliquity in 82 years			51.6

From the foregoing observations it appears that, at the summer solstice of the year 1690, Arcturus was 2° 40′ 12″.8 to the south of the sun's centre in declination: the motion of the star in declination, from that time to the summer solstice of the year 1772, including its proper otion, is 26′ 16″.4. Arcturus therefore, in 1772, should have been 3° 6′ 29″.2 to the south of the sun's centre,

if the angle of the ecliptic and equator had not varied: but that distance was found by actual observation to be 51″.6 less. By so much therefore must the obliquity of the ecliptic have become less in an interval of 82 years; and consequently the variation in 100 years will be 62″.92. If the observations of Arcturus be reduced to the solstice of 1771, and the zenith distance of the sun's centre, as observed in that year, be made use of in the same manner, the variation of the obliquity in 81 years will be found = 48″.8, and in 100 years = 60″.

If the quantity of the arc of Mr. Flamsteed's instrument were accurately known, the observations which he made at the winter solstice in 1690 might be compared with later observations, in order to determine both the quantity of the obliquity in 1690, and also the variation since his time. Accordingly, Mr. H. endeavoured to determine the error of the arc of the instrument between 28° and 75° of zenith distance, and proceeded in the following manner. He computed several observations of the stars ζ Tauri, η Pleïadum, η and μ Geminorum, and φ, σ, and o Sagittarii, as observed by Mr. Flamsteed in the years 1690, 1691, and 1692, and reducing them to the years 1760 and 1766; he compared the differences of declination between those stars, resulting from Mr. Flamsteed's observations, with the differences given by the places of the same stars, as settled by Dr. Bradley in 1760, and also by actual observations of the same stars made at Shirburn castle in 1766; and by combining these differences together, he found that the whole arc of 90° was too short by 43″. Supposing the error to be uniform, the proportional part of this quantity, thus found for the solstitial zenith distance of the sun in June = 13″.4, is nearly confirmed on the authority of Mr. Flamsteed himself, who, in the prolegomena to the 3d volume of the Historia Cœlestis, where he is deducing the latitude of the Royal Observatory at Greenwich, and the quantity of the obliquity in 1690, from his own observations, allows the zenith distances at 28°, 36°, and 40°, on his instrument, to be too small by 15″ and by 20″, at 75°. Mr. H. therefore computed the observations of the sun, made from November 30 to December 20 of 1690, which, reduced to the solstice, are as in the following table; to which are subjoined the observations made by himself at Oxford, at the winter solstice of 1771.

	°	′	″
Of the former, the mean is	74	58	25.9
Error of the line of collimation ..		−1	10
	74	57	15.9
Sun's parallax			−8.5
	74	57	7.4
Nutation			−9.6
Mean solstitial zenith distance of the Sun's centre, Dec. 1690 ..	74	56	57 8

	°	′	″
Of the latter, the mean is	75	13	17.3
Sun's parallax			−8.5
	75	13	8.8
Nutation....................			+7.9
	75	13	16.7
Error of the line of collimation ..			+4.8
Mean solstitial zenith dist. of the sun's centre, December 1771	75	13	21.5

The mean obliquity of the ecliptic resulting from the zenith distances, as observed at the two solstices in 1690, by applying the known latitude of the

place, will be found to be widely different, if no correction be applied for the error of the instrument.

June, zenith distance	−27°	59′	29.6″	Dec. zenith distance	74°	56′	57.8″
Latitude of Greenwich	51	28	38		−51	28	38
	23	29	8.4		23	28	19.8

But if the observations be corrected by the error of the instrument, the two results will be found to agree together as nearly as can be expected.

Thus, 27°	59′	29.6″	74°	56′	57.8″
		+13.4			+35.8
−27	59	43	74	57	33.6
51	28	38	−51	28	38
Obliquity 23	28	55	23	28	55.6

Or, if the obliquity be required independent of a knowledge of the latitude of the place, it will be found to be = 23° 28′ 55″.3. Thus,

December	74°	57′	33.6″
June	−27	59	43
Difference	46	57	50.6
Mean obliquity 1690, ½ difference	23	28	55.3

By comparing the observations at the summer solstices of 1771 and 1772, with those at the winter solstice of 1771, it appears that the mean obliquity was, about the beginning of the year 1772, = 23° 28′ 9″.4 and 23° 28′ 8″. Mr. H. supposes therefore the mean obliquity to be 23° 28′ 8″ at the beginning of the present year; and consequently the obliquity has diminished, by his observations, 47″ in 81 years, since Mr. Flamsteed's time, or at the rate of 58″ in 100 years, a quantity which will be found nearly at a mean of the computations framed by Mr. Euler and Mr. de la Lande, on the principles of attraction.

XV. New Observations on Vegetation. By Mr. Mustel of the Acad. of Sciences at Rouen. Translated from the French. p. 126.

Many celebrated writers, induced by the analogy which they observed between the vegetable and animal kingdoms, have admitted the circulation of the sap in the one, in a similar manner to the circulation of the blood in the other. This important point of vegetable economy produced a diversity of opinions, and has not yet been sufficiently cleared up. Dr. Hales, in his Vegetable Statics, does not seem to embrace the system of the circulation of the sap; nor does he prove the contrary.*

Mr. Du-Hamel, in his Physiology of Trees, contents himself with relating what has been said for or against this opinion; but though he sufficiently hints

* Il ne prouve pas contre. This certainly is a mistake. Dr. Hales, in the 4th chapter of his Physical Statics, not only declares openly against the doctrine of the circulation of the sap, and overturns the arguments alleged in favour of this opinion; but he produces several new experiments, which prove directly the impossibility of such a circulation. (See p. 144, &c.) His reasons have been thought so convincing, that the system of the circulation in plants has been ever since exploded in England; and that they have had a similar effect abroad, appears from the following quotation from a book of the ingenious Mr. Bonnet, F. R. S. of Geneva, entitled Recherches sur l'Usage des Feuilles, printed in 1754, p. 269. 'Pour moi, persuadé de la fausseté de cette opinion (que la seve circuloit comme le sang) par les expériences de M. Hales (ch. 4) &c.' M. M.—Orig.

that he does not believe it true, he determines nothing about it. The friends of the circulation in plants have never been able to find in them any thing analogous to that powerful organ, which is the promoter of it in animals, for want of such an organ, they were forced to imagine valves and paps in the lymphatic vessels of plants, by means of which the liquors once introduced into the sap vessels were supposed to be hindered from going back; but unfortunately no body has ever been able to discover these valves and paps, so different from the simple contrivances, by which nature is used to arrive at her ends.

An experiment, which Mr. M. made, and of which he proposes giving an account in this paper, throws a great light on this question, as well as on several others; and the conclusions deducible from it appear to him decisive. On the 12th of January, he placed several shrubs in pots against the windows of his hot-house, some within the house, and others without it. Through holes made for this purpose in the panes of glass, he passed a branch of each of the shrubs, so that those on the inside had a branch without, and those on the outside one within; after this, he took care that the holes should be exactly closed and luted. This inverse experiment, he thought, if followed closely, could not fail affording sufficient points of comparison, to trace out the differences, by the observation of the effects.

The 20th of January, a week after this disposition, all the branches that were in the hot-house began to disclose their buds. In the beginning of February, there appeared leaves, and towards the end of it shoots of a considerable length, which presented the young flowers. A dwarf apple-tree and several rose-trees, being submitted to the same experiment, showed the same appearance then, as they commonly put on in May; in short, all the branches which were within the hot-house, and consequently kept in the warm air, were green at the end of February, and had their shoots in great forwardness. Very different were those parts of the same tree, which were without, and exposed to the cold. None of these gave the least sign of vegetation; and the frost, which was intense at that time, broke a rose-pot placed on the outside, and killed some of the branches of that very tree, which on the inside was every day putting forth more and more shoots, leaves, and buds, so that it was in full vegetation on one side, while frozen on the other.

The continuance of the frost occasioned no change in any of the internal branches. They all continued in a very brisk and verdant state, as if they did not belong to the tree, which, on the outside, appeared in a state of the greatest suffering. On the 15th of March, notwithstanding the severity of the season, all was in full bloom. The apple tree had its root, its stem, and part of its branches, in the hot-house. These branches were covered with leaves and flowers; but the branches of the same tree, which were carried to the outside,

and exposed to the cold air, did not in the least partake of the activity of the rest, but were absolutely in the same state which all trees are in during winter. A rose-tree, in the same position, showed long shoots with leaves and buds; it had even shot a vigorous branch on its stalk, while a branch which passed through to the outside had not begun to produce any thing, but was in the same state with other rose-trees left in the ground. This branch is 4 lines in diameter, and 18 inches high.

The rose-tree on the outside was in the same state; but one of its branches drawn through to the inside of the hot-house, was covered with leaves and rose-buds. It was not without astonishment that Mr. M. saw this branch shoot as briskly as the rose-tree which was in the hot-house, whose roots and stalk, exposed as they were to the warm air, ought, it should seem, to have made it get forwarder than a branch belonging to a tree, whose roots, trunk, and all its other branches were at the very time frost-nipt. Notwithstanding this, the branch did not seem affected by the state of its trunk; but the action of the heat on it produced the same effect, as if the whole tree had been in the hot-house.

It would be useless to give an account of the diary he kept throughout the course of this interesting experiment. It may be sufficient to observe, that the walk of nature was uniformly the same. The interior branches continued their productions in a regular manner, and the external ones began theirs at the same time, and in the same manner as they would have done, had they been left in the ground. The fruits of the interior branches of the apple-tree were, in the beginning of May, of the size of nutmegs; while the blossoms but just began to show themselves on the branches without. Mr. M. observed that 3 of the flower-buds of the apple-tree had been gnawed off by a snail in such a manner, that all the petals and stamens had disappeared, being eaten up close to the calyx. This not having been entered by the snail, the basis of the pistillum and the embryo were preserved. He took it for granted that these flowers would bear nothing; but he was soon convinced of his mistake. Almost all of them bore fruit; the apples were perfectly formed, and 6 or 7 pretty large ones too were seen on each bunch. On the other hand, the snail had spared some other bunches, doubtless because more difficult to be got at; but out of 10 or 12 flowers in each bunch, not above 1 or 2 showed any signs of fruit. This suggested the idea, that when the flowers of trees are full blown, the prevention of the natural fall of the petals and stamens gives a greater assurance of the fructification: and on several times repeating the following experiment, he convinced himself that it did so. In imitation of the snail, he cut with his scissars the petals of apple, pear, plum, and cherry blossoms, close to the calyx. Almost

every one of those, which were thus cut, succeeded, while several of the neighbouring flowers miscarried.

Thus did a snail teach him how to render a tree fruitful; nor is it the first time that animals have been the instructors of mankind. However, this process is not very practicable in a large orchard; but it might be adopted in an espalier; in which one would choose to procure a great deal of fruit from trees of the best sort. It may indeed be questioned, whether the suppression of the stamens would not render the fruit barren; and in fact he found, that though the flowers of the dwarf apple-tree, whose petals and stamens were eaten up by the snail, gave apples equally large and beautiful, and that when he came to open them, he found the capsules formed as usual at the centre of them; yet they were entirely empty, without the least appearance of a pip. Absolute fructification therefore did not take place; since botanists, with reason, call nothing fruit but the seed, which contains the germen, which is to perpetuate the species. All the other parts, being only intended to co-operate in the formation and preservation of the seeds, perish of course, when once the seeds are come to maturity and perfection, and the work of nature fulfilled. Another remarkable thing in these apples is, that in the upper part there was found a much deeper cavity than usual. It was 8 or 9 lines deep. The orifice of this cavity was bordered by 5 tubercles, indented and somewhat elevated; but there was no vestige of the calyx, which it is well known remains always to the upper part of apples and pears, and is commonly called the eye.

But to return to the first experiment; the consequences of which, as before described, seem to prove, 1. First that the circulation of the sap does not take place in plants, as the circulation of the blood in animals. This may be deduced from the following observations. The tree in the hot-house went through all its changes during the winter, and the branch exposed to the open air underwent none; consequently the sap, which was in action in the root, stock, and head, of the tree, did not circulate through the branch without; which had no share in the vegetation of the roots and trunk. It might indeed be argued, that the cold air, to which this branch was exposed, stopped the circulation, and therefore that the first experiment would not be decisive: but the inverse of it seems fully so. The tree placed on the outside of the hot-house continued, during the whole winter, in the state of numbness, natural to all trees, which are exposed at that season; but one of its branches, which was in the hot-house, put forth successively its buds, leaves, blossoms, and fruits. While therefore the root of the tree, to which this branch belonged, was in the ground so frozen, that the pot itself in which it stood was broken by it, while the stock and top of the tree were so covered over with ice, that many of the branches were killed; this branch

alone did not in the least partake of the common state of numbness and suffering, but was on the contrary in full vegetation. The sap in it must have been extremely rarefied, and in very quick motion, while that of the tree was greatly condensed, and in total inaction. How is it possible to conceive a circulation of the sap from such a frozen root and stock, to a branch full of vigour, and loaded with leaves and flowers? Surely this experiment must appear conclusive against the system of circulation; since in this case it could at best only be admitted to have taken place in the vegetating branch; and that would very improperly be termed circulation, which should be confined to one limb.

2. This experiment proves, that each part of a tree is furnished with a sufficient quantity of sap to effect the first production of buds, flowers, and fruits. There is little probability that the branch drawn into the hot-house should have derived its sap from the roots of the tree: as they, at that time, lay in a very small quantity of earth, rendered extremely hard and dry by the frost, they could have but little fluid to spare; and even this, considering the congealed state of the lymphatic vessels of the stock, could have found no passage to the branch. This branch must of course have been enabled to continue its vegetation by the quantity of sap with which it was provided, the consumption of which must have been supplied at the first breaking of the frost. This truth, now demonstrable by experience, had been pointed out before by a multiplicity of other facts. Every body may have observed that a tree, which has been blown down in autumn, though separated from its trunk, begins the same vegetation, that it would have done if it had remained standing. Its buds open, it bears leaves, and even shoots, which sometimes are very long, and must be the effects of the sap it contained. It is true indeed that this appearance does not continue long, because the provision of sap once exhausted, without being renewed, every thing must of necessity perish. An effect of the like kind often deceives us in trees that have been newly planted, and in scions, which produce flowers and even fruits, without ever having taken root. But in this case, the symptoms which would seem to promise life, are on the contrary the forerunners of death; because the leaves, being from their nature the most powerful organs of transpiration and dissipation, the graft is the more readily exhausted, when there is no root to furnish it with a fresh supply of nutritive juices.

3. This experiment proves that it is heat which unfolds the leaves, and produces the other parts of fructification, in the branch exposed to its action. Autumn is the time in which nature employs itself, as it were clandestinely, under the cover of the leaves, in forming the buds, which contain the rudiments of the leaves, blossoms, and fruits, that are to be produced in the course of the succeeding summer. These buds prepare and work themselves out, during the winter, under the rough coats that are destined to preserve them

from the injuries of the weather. As soon as the warm weather in the spring begins to be felt, the buds open, and their coats, which then become useless, drop off, and give place to the productions which they contained and preserved. Immediately after this, the blossoms, flowers, and fruits make their appearance. This is the usual operation: but in the case before us, nature was, as it were, surprized by art; what she should not have done till spring, she did in the winter, because the heat of the hot-house produced that expansion, which, according to the natural course, ought to have been effected by the rays of the sun darting less obliquely than before on the horizon. There is no doubt but it is to heat, either natural or artificial, that this expansion is owing; and the experiment proves that it is only in that part of the tree, which is exposed to the effect of heat, that the sap, which in every other part remains torpid and inactive, is put into motion, and produces vegetation. From this it appears, that the vegetable economy is different from the animal, and that those who endeavoured to establish the circulation in both, carried their analogy too far.

This fact, now established, furnishes a good reason why in the tapping of the maple and sugar birch trees, so much liquor runs out on one side, and none at all on the other. It is well known that, if during the time of a frost, or a summer's day, towards noon, you bore a hole on the side of the maple tree exposed to the south, you will get a great quantity of liquor from it; and that if you bore the north side at the same time, you will not get a drop. The cause of this evidently appears from what has been said. We likewise see why trees exposed to the south lose a great many of their branches, and sometimes die altogether, in the course of a severe winter; while trees of the same sort, but placed to the north, or in some other exposition, will stand the hardest frosts. This is particularly remarkable in the evergreens, whose resinous and oily sap being liquefied by the heat of the sun, the tree cannot escape suffering a great deal, whenever it is surprized in that state by the night frosts. Those observers who attend to this, and know how well pines, firs, and bays succeed, when planted on the back of mountains exposed to the north, will take care not to place such kind of trees in a southern aspect, in hopes of their succeeding better by it.

XVI. Actual Fire and Detonation produced by the Contact of Tin-foil, with the Salt composed of Copper and the Nitrous Acid. By B. Higgins, M. D., p. 137.

Several pieces of thin sheet copper, placed vertically, and at a small distance from each other, in the strong nitrous acid diluted with half its quantity, or more, of water, and suffered to remain in a close vessel, till the acid is saturated, afford a crystalline bluish green salt, which is to be separated from the undissolved copper and the superfluent green liquor, and kept in a well corked

bottle; because, on exposure to the air, it deliquesces. This salt, taken moist, but not very wet, and beaten to the fineness of basket sea salt, in a mortar, is to be strewed to the thickness of a shilling, on a piece of tin foil, 12 inches in length, and 3 in breadth. Then the foil is to be instantly rolled up, so as to include the salt, as it lay, between the coils. The ends are to be shut by pinching them together, and the whole is to be pressed flat and close.

All this being done as quickly as possible, the first phenomenon is—A part of the salt deliquesces. 2d. This part impregnated with tin, changed in colour, and of a thicker consistence, begins to froth from the ends of the coil. 3d. A strong frothing, accompanied with moderate warmth. 4th. The emission of copious nitrous fumes. 5th. Heat intolerable to the fingers. 6th. Explosion and fire, which burst and fuse the tin foil in several places, if it be very thin.

After many conjectures and experiments, Dr. H. discovered a property in the cupreous salt, from which, and the known affinities of the bodies concerned, these appearances, however new and singular, may be accounted for. The cupreous salt, properly dried, and placed where it may receive a heat, not much greater than what the hand can bear, takes fire. The circumstances which favour this ignition, and contribute to produce it in the smallest degree of heat, concur in the following convenient method of trying the experiment. A piece of soft bibulous paper is to be dipped in the nitrous solution of copper, and dried before the fire 2 or 3 times alternately. Then it is to be approached towards the heat, as near as can be borne, by the hand which holds it, without pain: there, if it has been sufficiently dried, it will presently catch fire, and burn to a brown calx.

The easy ignition of the salt in a slight heat being thus ascertained, there is no room to doubt that the foregoing phenomena are produced in the following manner. The acid of the liquor, which moistened the salt, quits the copper, to unite with the tin, leaving the water to be imbibed by the contiguous salt of copper, which then dissolves, and acts briskly on the tin foil.

It is well known that the action of the nitrous acid on tin is always accompanied with considerable heat and effervescence, and that the solution of metallic salts in watry liquors is hastened by heat. In this experiment, the warmth generated by the first action of the cupreous solution, promotes the deliquescence of the crystallized salt. The union of the acid with the tin is rapid, not only as being assisted by heat, but on account of the great surface exposed; whence the strong frothing, and the extraordinary heat, by which the redundant moisture is carried away, and the undecomposed part of the cupreous salt, together with that lately formed with the tin, perfectly dried.

The heat generated on both surfaces of a large expanse of tin, is concentrated by closely coiling it into a small compass, and being retained by the various sur-

rounding laminæ of metal, it is necessarily accumulated to a quantity which, if we may judge from the touch, is more than sufficient to fire the dry cupreous salt. The salt formed with tin, and the nitrous acid, burns and sparkles in a red heat. Catching fire therefore, from the ignited cupreous salt, it burns with it, and assists in the detonation, which is common to all nitrous compositions in similar circumstances.

If the salt be very wet, there will not be much fire or explosion, because the heat will be dissipated before the salt can be sufficiently dried in every part. If the salt be not moist, it cannot commence the action which is necessary; and there will be no fire, because there can be no hasty solution of the tin to give the requiste heat. If the tin and salt be not coiled up in due time, there will be very little heat, and no fire; because the dissipation of the heat from a broad expanse, keeps pace with the generation of it; and as the moisture exhales quickly in this manner, there is none left to renew the action on the tin and consequent heat, when the proper time of coiling has elapsed.

A piece of tin foil, larger than that above described, cannot easily be managed; smaller pieces give less fire in the direct proportion of their surfaces, and the quantity of salt which they can, at the same instant, reduce to the required state of dryness. The sudden dissipation of the moisture appears the most curious of these phenomena. To render it the more observable, he made the following experiments: he placed a piece of tin foil, 12 inches long by 2 broad, loosely coiled, and standing vertically on the flattest end, in half a table-spoonful of the saturated solution of copper in the diluted nitrous acid; and found that scarcely 5 seconds elapsed, from the time when a brisk effervescence, accompanied with weak nitrous fumes, arose, till the liquor became a consistent mass, and sparks of fire issued from the coils of tin; which having attracted part of the solution above the common level, brought it into the condition in which it is readily dried, heated, and fired.

A like quantity of the same solution, kept in a strong boiling heat, does not acquire such consistence in a ten fold space of time. The hasty exhalation therefore, is not caused by the heat alone; neither does it seem to require any great surface. What else it is owing to, he commits a while to the examination of the curious.

XVII. Extracts of some Letters, from Sir Wm. Johnson, Bart., to Arthur Lee, M. D., F. R. S., on the Customs, Manners, and Languages of the Northern Indians of America. p. 142.

In all inquiries of this sort, we should distinguish between the more remote tribes, and those Indians, who, from their having been next to our settlements for several years, and relying solely on oral tradition for the support of their

ancient usages, have lost great part of them, and have blended some with our customs, so as to render it extremely difficult, if not impossible, to trace their customs to their origin.

The Indians did certainly live under more order and government formerly, than at present. This may seem odd, but it is true; for, their intercourse being with the lower class of our traders, they learn little from us but our vices; and their long wars, together with the immoderate use of spirituous liquors, have so reduced them, as to render that order, which was first instituted among them, unnecessary and impracticable. They do not at present use hieroglyphics; their figures being drawn, to the utmost of their skill, to represent the thing intended. For instance, when they go to war, they paint some trees with the figures of warriors, often the exact number of the party; and if they go by water, they delineate a canoe. When they gain a victory, they mark the handle of their tomahawk with human figures, to signify prisoners; and draw the bodies without heads, to express the scalps they have taken. The figures which they affix to deeds have led some to imagine, that they had alphabetical characters or cyphers. The fact is this: every nation is divided into tribes, of which some have 3, as the turtle, bear, and wolf; to which some add the snake, deer, &c. Each tribe forms a little community within the nation; and as the nation has its peculiar symbol, so has each tribe the particular badge from which it is denominated: and a sachem of each tribe being a necessary party to a fair conveyance, such sachem affixes the mark of his tribe to it, like the public seal of a corporation. With respect to the deed of 1726, of which you sent me the signatures, the transaction was in some measure of a partial nature. All the nations of the confederacy did not subscribe it; and those chiefs who did, neglected to pay due regard to their proper symbols; but signed agreeably to fancy, of which I have seen other instances. The manner I have mentioned is the most authentic, and conformable to their original practice.

As to the information which, you observe, I formerly transmitted to the governor of New-York, concerning the belt and 15 bloody sticks sent by the Missisagees, the like is very common; and they use these sticks, as well to express the alliance of castles, as the number of individuals in a party. The sticks are generally about 6 inches in length, very slender, and painted red if the subject be war. Their belts are mostly black wampum, painted red when they denote war. They describe castles sometimes on them, by square figures of white wampum; and in alliances, human figures holding a chain, which is their emblem of friendship, and each figure represents a nation. An axe is also sometimes described, and always imports war: the taking it up, being a declaration of war; and the burying it, a token of peace.

With respect to your questions concerning the chief magistrate, or sachem,

and how he acquires his authority, &c. I am to acquaint you, that there is, in every nation, a sachem, or chief; who appears to have some authority over the rest, and it is greatest among the most distant nations. But in most of those bordering on our settlements, his authority is scarcely discernible, he seldom assuming any power before his people. And indeed this humility is judged the best policy; for, wanting coercive power, their commands would perhaps occasion assassination, which sometimes happens. The sachems of each tribe are usually chosen in a public assembly of the chiefs and warriors, whenever a vacancy happens by death or otherwise; they are generally chosen for their sense and bravery, from among the oldest warriors, and approved of by all the tribe; on which they are saluted sachems. There are however several exceptions; for some families have a kind of inheritance in the office, and are called to this station in their infancy.

The chief sachem, by some called the king, is so, either by inheritance, or by a kind of tacit consent, the consequence of his superior abilities and influence. The duration of his authority depends much on his own wisdom, the number and consequence of his relations, and the strength of his particular tribe. But even in those cases where it descends, should the successor appear unequal to the task, some other sachem is sure to possess himself of the power and the duties of the office. I should have observed, that military services are the chief recommendations to this rank. And it appears pretty clearly, that heretofore the chief of a nation had, in some small degree, the authority of a sovereign. This is now the fact among the most remote Indians. But as, since the introduction of fire arms, they no longer fight in close bodies, but every man is his own general, I am inclined to think this has contributed to lessen the power of a chief. This chief of a whole nation has the custody of the belts of wampum, &c. which are as records of public transactions: he prompts the speakers at all treaties, and proposes affairs of consequence. The chief sachems form the grand council; and those of each tribe often deliberate on the affairs of their particular tribes. All their deliberations are conducted with extraordinary regularity and decorum. They never interrupt him who is speaking; nor use harsh language, whatever may be their thoughts. The chiefs assume most authority in the field; but this must be done, even there, with great caution; as a head warrior thinks himself of most consequence in that place.

The Indians believe in, and are much afraid of witchcraft: those suspected of it are therefore often punished with death. Several nations are equally severe on those guilty of theft, a crime indeed uncommon among them: but in cases of murder, the relations are left to take what revenge they please. In general, they are unwilling to inflict capital punishments, as these defeat their grand political object, which is, to increase their numbers by all possible means.

On their haunts, as on all other occasions, they are strict observers of meum and tuum; and this from principle, holding theft in contempt; so that they are rarely guilty of it, though tempted by articles of much value. Neither do the strong attempt to seize the prey of the weak; and I must do them the justice to say that, unless heated by liquor, or inflamed by revenge, their ideas of right and wrong, and their practices in consequence of them, would, if more known, do them much honour. It is true that, having been often deceived by us in the purchase of lands, in trade, and other transactions, many of them begin now to act the same part. But this reflects most on those who set them the example.

As to your remark on their apparent repugnance to civilization, I must observe, that this is not owing to any viciousness of their nature, or want of capacity; as they have a strong genius for arts, and uncommon patience. I believe they are put to the English schools too late, and sent back too soon to their people, whose political maxim, Spartan like, is to discountenance all pursuits but war, holding all other knowledge as unworthy the dignity of man, and tending to enervate and divert them from that warfare on which they conceive their liberty and happiness depend. These sentiments constantly instilled into the minds of youth, and illustrated by examples drawn from the contemptible state of the domesticated tribes, leave lasting impressions; and can hardly be defeated by an ordinary school education.

I wish my present leisure would allow me to give you as many specimens of their language as would show that, though not very wordy, it is extremely emphatical; and their style adorned with noble images, strong metaphors, and equal in allegory to many of the eastern nations. The article is contained in the noun, by varying the termination; and the adjective is combined into one word. Thus of echin, a man, and gowana, great, is formed echingowana, a great man. Caghyunghaw is a creek, caghyungha a river, caghyunghaowana a great river; caghyungheeo a fine river. Haga the inhabitants of any place, and tierham the morning; so, if they speak of eastern people, they say tierhans-aga, or people of the morning. Eso is expressive of a great quantity, and esogee is the superlative. The words goronta and golota, which you mention, are not of the six nations, but a southern language. It is curious to observe, that they have various modes of speech and phrases peculiar to each age and sex, which they strictly observe. For instance, a man says, when he is hungry, cadagcariax, which is expressive both of his want and of the animal food he requires to supply it; whilst a child says, in the same circumstances, cautsore, that is, I require spoon-meat.

There is so remarkable a difference in the language of the six nations from all others, as affords ground for inquiring into their distinct origin. The nations

north of the St. Lawrence, those west of the great lakes, with the few who inhabit the sea coasts of New England, and those again who live about the Ohio, notwithstanding the respective distances between them, speak a language radically the same, and can in general communicate their wants to one another; while the six nations, who live in the midst of them, are incapable of conveying a single idea to their neighbours, nor can they pronounce a word of their language with correctness. The letters M and P, which occur frequently in the other languages, are not in theirs; nor can they pronounce them but with the utmost difficulty. There is indeed some difference of dialect among the six nations themselves; but this is little more than what is found in all the European states.

XVIII. Of some curious Fishes, sent from Hudson's Bay. By Mr. J. Reinhold Forster, F. R. S. p. 149.

The governor and committee of the Hudson's Bay Committee presented the R. S. with a choice collection of skins of quadrupeds, many fine birds, and some fish, collected by their servants at the several ports in Hudson's Bay; the committee of the R. S., for examining and describing these curiosities, referred them to Mr. F. for examination. And he here adds the following observations on the fish from that place.

The 4 kinds of Hudson's Bay fish are, the sturgeon, the burbot, the gwiniad, and a new fish called the sucker at Hudson's Bay. The sturgeon was about 14 inches long, and therefore seems to be a young fish: as it is likewise observed in the list written by the gentleman who sent this fish from York fort. Its nose is very long and slender, terminating in a point; the eyes are small; under the projecting snout, before the mouth, are 4 beards or cirrhi, placed nearly in the same line, and not by pairs as in some other species of sturgeon. The mouth is beneath, nearly opposite the eyes, toothless, cartilaginous, semilunar when in its natural position, but round when open; on each side are 2 nostrils. The whole head is depressed, and very nearly quadrangular; the whole body pentagonal, and tapering towards the tail; the whole skin tough, covered with 5 rows of uncinated scales; the dorsal series consists of 14 large roundish scales, and a single one behind the dorsal fin; each of the lateral rows has 35 oblique scales; in the 2 ventral rows are 9 roundish strong scales between the pectoral and ventral fins; one scale is behind the vent, and another behind the anal fin.

The fish, according to this description, seems to come the nearest to that species of sturgeon which he described in the Phil. Trans., vol. 57, [abridged vol. xii.] in his Specimen Historiæ Naturalis Volgensis, N° 10, under the name of acipenser ruthenus major, rostro elongato acuminato, paululum supino, and which the Russians call Sevruga. Kramer, in his Elenchus Vegetabilium et

Animalium Austriæ, p. 383, is the only writer who takes notice of this species; he calls it acipenser rostro acuto, corpore tuberculis spinosis aspero: the inhabitants of Austria call it shirk, a name they have no doubt borrowed from the Slavonian name sevruga. The famous painter and traveller Cornelys de Bruyn mentions this kind of fish, but in so superficial a manner, that one plainly sees he was little, if at all, used to discussions in points of natural history. Had de Bruyn examined the sevruga, he would certainly have found it materially different from the stoer or assetrina, i. e. the common blunt nosed sturgeon of Germany and the Baltic. Mr. F. supposes the English sturgeon, from Pennant's description, and the drawing in the British Zoology, illustrated by plates, tab. 89, to be the same with this kind from Hudson's Bay, and with the sevruga of the Russians, and the shirk of the Austrians. The true sturgeon, which gave the name to the whole genus,* he thinks an unknown fish in England. The species of sturgeons are more numerous than one is at first aware of; and it would therefore be of some utility, that persons, who have an opportunity of examining all the various kinds at Vienna, and in Russia, might do it with more care than has hitherto been done. Mr. Klein, a very ingenious naturalist, has enumerated 10 sturgeons, in his 4th Missus Piscium, p. 11—16; and Count Marsigli, in his splendid work about the Danube, tom. 4, gives the names of at least 6 sturgeons, but the characters are not sufficiently settled in both these works. Klein saw only 2 kinds of sturgeons, and a 3d in spirits; and Count Marsigli was not enough of a naturalist to give adequate descriptions of these fish.

The 2d of the Hudson's Bay fish, is called, by the wild natives of that country, marthy, and is no other than our common burbot, gadus lota, Linn. only vastly superior in size. The descriptions given of this fish, in the British Zoology, is entirely corresponding with this specimen, so that it would be superfluous to presume to make any additions to it. However, after a most minute examination, Mr. F. could find no more than 6 branchiostegous rays in the two specimens from Hudson's Bay, of which Mr. Pennant mentions 7 in the English burbot, and Artedi as many in his specimens. This great naturalist seems likewise to be right, when he observes that the cirrhi, or beards on the end of the nose, are the valves to one of the nostrils; for Mr. F. found that these beards, on their under side, opened into a hole corresponding with the lower nostril. Mr. A. Graham, the collector of the natural history specimens at Severn river in Hudson's Bay, observes, that these fish constantly swim close to the ground, and are extremely voracious; for he represents them as not content with

* The Germans call this fish stoer, from the old Teutonic word stor or stuhr, which signifies great, as this fish grows to a very large size Thus likewise the Scotch call the tunny, mackrel sture. Vide Mr. Pennant's Tour in Scotland, p. 192.—Orig.

devouring every fish* they can overcome, but likewise feeding on putrefying deer, or other carrion that comes in their way; even stones are sometimes swallowed to satisfy their insatiable appetite, of which Mr. Graham was himself a witness, having taken a stone of a pound weight out of the stomach of this fish. The pike is often obliged to fall a victim, together with the trout, tickomeg, and others, to this rapacious fish. After sunset, it is caught by a night hook. It does not masticate its food before deglutition. Its roe and liver are reckoned a delicacy, when fresh caught; but they turn rancid and oily in a few days, though kept frozen solid all the time. At Hudson's Bay this fish is thought to be dry and insipid; its weight is from 1 to 8 pounds.

The 3d species of fish, from this cold climate, is by the natives called tickomeg, and is our gwiniad or salmo lavaretus, Linn.; only the size is somewhat larger, for the greatest specimen sent over measures 18 inches from the head to the tip of the tail, is 4½ inches deep, and not above an inch and ¼ thick. This fish differs in no circumstance from our gwiniad, but the length. Mr. Pennant mentions in the British Zoology, vol. 3, p. 269, a ferra or gwiniad from Switzerland 15 inches long, as an uncommon size;† the Hudson's Bay fish, as I have before observed, is 18 inches long, and 4½ inches its greatest depth. The great abundance of food, and the small number of inhabitants, who let the fish grow up undisturbed, are perhaps the causes of their uncommon size. They weigh from 1½ pound to 3 pounds, says Mr. Graham; but the fish Mr. F. examined must, when fresh, have weighed more. These fish abound in the river Severn in Hudson's Bay, from its origin in the great lakes to its mouth, where it empties itself into the bay. The natives catch 5 or 6 hundred a day, by means of wears which they contrive in the river: they will not take bait, and are poor at the breaking of the ice in the river. In the middle of the summer, after a gale of wind, they are often found thrown up into the marshes, and on the shoals, where they remain at the recess of the water and abating of the wind, and serve as food to numbers of crows. The inhabitants of Hudson's Bay think this fish very sweet, and good to eat, contrary to the opinion of many Europeans.

The 4th and last fish brought from Hudson's Bay, is there called a sucker, because it lives by suction, according to Mr. Graham's account, who also says there are 2 varieties of this fish, both of a whitish colour, but one distinguished by a mixture of beautiful red. In the smallest of 2 specimens brought over, a broad stripe of red could be observed all along the linea lateralis. They are very numerous in the creeks and rivers, and troublesome in overburdening the nets.

* This too is the fish that makes such havock in the lake of Geneva. P.—Orig.

† However, the gwiniads of Lapland, a similar climate to that of the Hudson's Bay, are vastly large. Brit. Zool. 3, 297, note.—Orig.

They are not deemed a palatable food, being very soft, and full of small bones. They weigh from one-half to $2\frac{1}{2}$ pounds.

The above is literally what Mr. Graham says of this fish, and all that is known of its natural history. Examining it carefully, Mr. F. found it was a new species of the genus of cyprinus, or carp. The head is broader than the body, gradually decreasing towards the nose, full of elevations and tubercles, nearly quadrangular, and not scaly. The mouth is quite under the head, as in the loricariæ, when shut, semilunar; when open, round; not far from the extremity of the snout, and included in small round lips. To the under lip is fixed a bilobated, beard-like, papillose caruncula; it has no teeth. The eyes are large, but the colour of the iris could not be determined. The number of the branchiostegous rays is 3. The body is flat, tapering towards the tail, and scaly. The greater specimen measures very near 15 inches from the nose to the extremity of the tail; next to the head it is nearly 2 inches thick, about the dorsal fin $1\frac{1}{4}$ inch; its greatest depth before the ventral fins is $2\frac{4}{5}$ inches. On the snout are about 5 round prominent tubercles; 2 nostrils are found on each side, the largest next before the eye is kidney-shaped. The covers of the gills are double, and divided; the head has several sutures; over each eye, in a cavity, are 2 longitudinal ones, joined opposite the nostrils by a still shorter transverse one; on the covers of the gills are 2, on each side 1, beginning near the lobes of the caruncula of the under lip, and going up arched towards the eye. Near the extremity of the snout begins on each side a longitudinal one; it passes under the eye, and mounts in a curvature behind it; then it goes on straight to the end of the head, where it again gets downwards, and joins the lateral line. Where the head joins to the body, these two sutures are connected by a transversal one, which, as it were, separates the head from the body. The lateral line at first descends from the head, but then runs on straight, rather nearer the back than the body, to the beginning of the tail. The scales are small near the head and back, increasing in size towards the middle and tail, close to which they are again smaller. The dorsal fin is placed somewhat behind the equilibrium of the fish, rhomboidal, and consisting of 12 strong branched rays. The pectoral fins are lanceolated, fixed under the covers of the gills, and have 17 rays. The ventral fins have 10 or 11 rays, and are placed in the middle of the belly, and under the dorsal fin. The anal fin consists of 8 branched strong rays. The tail is somewhat forked or concave, and consists of 17 rays.*

* Dr. Forster's English description of this fish (*Cyprinus catostomus*,) being very complete, of course renders the Latin one unnecessary, which is therefore omitted.

XIX. *Experiments on the Different Kinds of Marl found in Staffordshire. By Wm. Withering, M.D.* p. 161.

Number.	Description.	Quantity of calcarious earth in half a dram, as separated by the nitrous acid, and precipitated by mild fixed alkali.	What was left after the foregoing separation, was no longer acted on by the nitrous acid; but being		One dram of each of the marls being calcined, weighed			The calcined marls put into water, produced
		Grains.	Mixed with water, became	When burnt	Grs.	Lost grs.	Burnt to	
1	Red and blue intermixed, in small friable lumps	1	Uniform and plastic.	A hard red brick.	52	8	Red brick.	No effect.
2	Red, in small friable lumps	$0\frac{3}{4}$	Uniform and plastic.	A hard red brick.	53	7	Red brick.	No effect.
3	Grey, in large hard lumps	5	Plastic, but a little gritty.	A soft yellowish brick.	49	11	Soft yellow brick.	Weak lime wat.
4	Red, hard, compact	3	Uniform and plastic.	A hard red brick.	50	10	Red brick.	No effect.
5	Red, with grey spots, in large hard lumps	$8\frac{1}{2}$	Plastic.	A soft pale red brick.	48	12	Hard grey stone.	Lime water.
6	Light grey, like a grit stone	8	Gritty, no union.	No union.	51	9	Soft and stony.	Lime water.
7	Brown, friable, in large lumps	18	No union.	A little cohesion.	46	14	Soft stone.	Lime water.
8	Red, in large friable lumps	14	Plastic, but a little gritty.	A soft red brick.	48	12	Soft stone.	Strong lime wat.
9	Brownish white, very hard, like calca. incrustations	16	No union, gritty.	No union.	43	17	Soft stone.	Strong lime wat.
10	Lead colour, friable, flaky	$14\frac{1}{2}$	No union, gritty.	No union.	48	12	Soft stone.	Strong lime wat.
11	Brown grey, very hard, in irregular lumps	16	No union, gritty.	No union.	40	20	Soft stone.	Strong lime wat.
12	Lead colour, in powder and in small hard lumps	$20\frac{1}{2}$	Uniform and plastic.	A soft whitish brick.	29	31	Powdery.	Strong lime wat.

Half a dram of the marls being put into similar glass cups, 2 drams of nitrous acid being added to each glass, they all effervesced; N° 1 and 2 the least, N° 12 the most. The effervescence having ceased, and 6 drams of rain water being added to each glass, the liquors were all filtered, and after filtration, changed violet paper to a red colour. To the filtered colours was gradually added mild fixed alkali, sufficient to saturate the acid, and precipitate all the earth it had dissolved. The precipitated earth being washed in rain water, till free from all saline matter, weighed, when dry, as in the 3d column. Column the 4th shows that, after the separation of the calcarious earth, there remained in N° 1, 2, 4, a red clay, in N° 12 a white clay; in N° 8 a red clay, and a portion of sand; in N° 3 a whitish clay, with a portion of sand; in N° 6, 9, 10, 11, pure sand; and in N° 7 sand, with a small portion of clay. These residuums were all washed with rain water before they were burnt. The precipitated powders being mixed together, 82 grains of it put into a crucible, and calcined with a strong heat, lost 35 grains in weight. Rain water was poured on the calx; the next morning there was a pellicle on the surface of the water; it tasted strongly of lime, and let fall a calcarious earth, on the addition of mild fixed alkali. The marls were kept for some weeks in a dry place before they were used. They were all got out of marl pits in the neighbourhood of Stafford, except N° 12, which is found near the Duke of Bridge-

water's

water's canal, in a powdery form, and when mixed with $\frac{1}{4}$ part of clay is burnt to quick lime. All the above marls crack and fall to pieces when exposed to the weather.

The foregoing experiments were undertaken with a view to ascertain how far it would be advisable to attempt burning the marls of this country into quick-lime, for the purposes of agriculture; they may likewise furnish us with some useful hints relative to the kind of marls proper to be used on different kinds of lands. Perhaps the calcarious earth united with clay, as in N° 1, 2, 4, &c. may be the best for light sandy soil; and N° 6, 9, 10, 11, where the calcarious earth is united with sand, the most eligible where the land is already stiff, and abounding with clay. How far the different quantities of fixable air, or other volatile parts, contained in each of the marls, as shown by the 5th column, will influence their preference in agriculture, must be left to the experience of the farmer to determine.

XX. Of a Fiery Meteor, seen Feb. 10*th*, 1772; *and also on some New Electrical Experiments. Dated Eccles, Berwickshire. By Patrick Brydone, Esq.*, p. 163.

On Monday the 10th of Feb. 1772, exactly at 7 in the evening, as Mr. B. was riding through Tweedmouth, a village at the south end of Berwick-bridge, he observed that the atmosphere was suddenly illuminated in a very extraordinary manner. The light of the moon, which was about half full, seemed to be extinguished by the blaze; and he saw his shadow projected on the ground, and almost as distinct, and well defined, as in sun-shine. He turned round to see whence the light proceeded, when he beheld a long, bright flame, moving almost horizontally along the heavens. It was of a conical form, and from the base to the apex could not be less than 6 or 7 degrees; its height, when he first observed it, seemed to be about 50 degrees; but it descended gently, and appeared to burst about 5 or 6 degrees lower. Its course was from N. W. to S. E., and seemed to have an inclination to the horizon; but this might be only a deception. The base of the cone was rounded like a sphere; and apparently of about $\frac{1}{3}$ of the diameter of the moon at her greatest height; but its light was brighter than that of the planet Venus, and in colour resembled the flame of burning camphor. Near the end of the tail there was a kind of waving motion, which, with the whole appearance, is endeavoured to be represented by the annexed figure. In about 10 or 12 seconds it seemed to burst, dividing into a number of small luminous bodies, like the stars in a sky-rocket, which immediately disappeared.

As Mr. B. had formerly observed explosions from meteors of this kind, he had presence of mind to pull out his watch, which has a 2d hand, to measure the

exact time the report should take in reaching him. He waited upwards of 4 minutes, which in his state of expectation appeared a much longer time; when, despairing of any report, he rode on, but had not got to the middle of the bridge, when he was stunned by a loud and heavy explosion, resembling the discharge of a large mortar, at no great distance, and followed by a kind of rumbling noise, like that of thunder. He examined his watch, and found that the sound had taken 5 minutes, and about 7 seconds, to reach him; which, according to the common computation of 1142 feet in a second, amounts to the distance of at least 66 miles. It did not occur to him to measure the duration of the light, which probably did not exceed 10 or 12 seconds; and during this short period, the length of the path, the meteor seemed to describe, could not be less than 30 degrees. He expected to have seen some account of this phenomenon from Newcastle, as, by its direction and distance, he imagined it had burst pretty near the zenith of that town; but no notice was taken of it in the newspapers there. About a week after, he mentioned what he had seen to Sir John Paterson of Eccles, who told him he was at that time on the road, between Greenlaw and his own house; and as he was riding to the south, he observed the meteor from its first appearance, which was about 3 or 4 seconds sooner than he had time to turn about and view it; and this perhaps is the reason that it appeared so much higher to him than it did to Mr. B. That gentleman observed, that when it first became luminous, it was almost vertical, but went off descending to the S. E., and had in other respects the appearance above described. He added, that some considerable time after the light disappeared, he heard a great report, which he took for a clap of thunder; for the interval was so long, that he did not imagine this sound had any connection with what he had seen.

Now, as this gentleman was at least 20 miles to the west of the spot where Mr. B. made his observation, and as the appearance and height of this meteor seems to have been nearly the same to them both, it is probable that it was at a very great distance from the earth, and much beyond the limits that have been assigned to our atmosphere. The smaller meteors, called falling stars, Mr. B. frequently observed from the mountain of St. Bernard, one of the high Alps; and last year he had the good fortune to see several of them from the highest region of mount Etna; an elevation still more considerable, and probably the greatest accessible one in Europe, and they always appeared as high, as when seen from the lowest grounds; so that probably the height of 2 or 3 miles, bears but a small proportion to the common altitude of these bodies.

From their frequent appearance during the last frost, Mr. B. was inclined to believe, that the air was then in a very favourable state for electrical purposes; but not being provided with a common machine, he bethought him of a whim-

sical one to supply the want of it. The back of a cat, it is well known, often exhibits strong marks of electricity; being therefore desirous to try what effect this might produce, when made use of instead of the glass globe, he cut a quantity of harpsichord wire into short pieces, of 5 or 6 inches, and tying them together at one end, made the other diverge like the hair of a brush. He took a large metal pestle of a mortar for a conductor, to the end of which he fixed the brush of wire; and insulated the whole, by placing it on a couple of wine-glasses. He then took a cat on his knee, and bringing her back under the wires, he began to stroke it gently. The animal continued in good humour for a few minutes, and he had the satisfaction to see that the conductor was so much charged, that it emitted sparks of a considerable force, and attracted strongly such light bodies as were brought near it; but the cat at last becoming uneasy, threatened to put an end to the experiment. The passage of the electrical fire, from the hair of her back to the small wires, occasioned, it seems, a disagreeable sensation, which she could not bear; so that turning about her head to defend her back, the tip of her ear happening to touch the conductor, and a large spark coming from it, she sprung away in a fright, and would not allow him to come near her more. However, after a long interval, the animal seeming to have forgotten her adventure, a young lady in company, less obnoxious to her than he was, undertook to manage her. Having first covered the back of this lady's hand with a piece of dry silk, that none of the electric fire communicated to the wires might be lost, she then began to stroke the cat as he had done, and the conductor soon after appeared fully charged: they drew large sparks from it; and if the animal would have continued quiet, he had no doubt that they should have showed many of the common experiments in electricity; but she soon became so outrageous, that they were glad to put an end to the operations, without any hopes of being able to repeat them, at least with the same instrument. In this dilemma he recollected, that a lady had told him, that on combing her hair, in frosty weather, she had often been sensible of a little crackling noise; and in the dark had sometimes observed small sparks of fire to issue from it. He proposed, therefore, that one of the young ladies would suffer the experiment to be made on her head, which she agreed to. The conductor was then insulated as before, and the lady having placed herself so, that the back part of her head almost touched the brush of wire, he desired her sister to stand behind her, on a cake of bee's wax; who, as soon as she began to comb the hair of the former, the conductor emitted sparks still of a larger size than those they had hitherto seen. The hair was extremely electric, and when the room was darkened, they could perceive the fire pass from it along the small wires to the conductor. The young lady who was on the wax, was not a little surprised to find, that the moment she began to comb her sister's hair, her own body became electric, dart-

ing out sparks of fire against every substance that approached her. They found however that these sparks were not strong enough to fire spirits. Mr. B. then coated a small phial, and soon charged it from the conductor; but afterwards he did it more completely from the hair itself in the following manner. He fixed a brush of small wires to the large one that went through the cork of the phial; and taking the phial in his hand, he followed every motion of the comb with the brush of wires; and, in the dark, could observe the fire pass by these wires into the bottle. In a few minutes he found it was highly charged; when taking a spoonful of warm spirits in his left hand, and with his right, which grasped the phial, bringing the hook of the great wire near the surface of the spirits, a large spark darted from it, gave him a smart shock, and at the same time set the spirits on fire.

The day following, he wanted to repeat the experiments; but as the weather was hazy, and the frost had greatly abated, they did not so well answer. However, from making them on several heads, he found that the stronger the hair, the greater was the effect; whereas soft flaxen hair produced little or no fire at all. These experiments were made in a warm, dry room, before a good fire, and at a time when the thermometer, in the open air, was at 6 or 7 degrees below the point of congelation. The hair, which succeeded best, was perfectly dry, and no powder or pomatum had been used on it for some months before.

XXI. *Of a Fossil lately found near Christ-Church, Hants. By the Hon. Daines Barrington, V. P. R. S.* p. 171.

The shining divisions on the surface of this stone, seem to be the scales of a fish, which Mr. B. conceives to be the acus maxima squamosa, engraved in Willoughby's History of Fish, tab. p. 8, and described by Ray* in his Synopsis Piscium, p. 109. It appears by the catalogue of English fossils, in the collection of Dr. Woodward, that a still larger specimen of the same sort was found in Stansfield quarry, near Woodstock, though Dr. Woodward could only procure a single scale, v. 2, p. 53, c. 24. Single scales from the same quarry are also to be seen in the noble collection of fossils, given by Mr. Brander, F.R.S., to the British Museum. Though this fish therefore is a stranger to our seas, yet its exuviæ are by no means so to our cliffs and quarries.

P. S. Mr. Hunter, F.R.S., having seen the fossil at Crane-court, happened to dissect a beaver's tail very soon afterwards, which he showed, as bearing a strong resemblance to the scaly divisions in this specimen; Mr. B. however still thinks that the form of the scales in the acus maxima squamosa of Willoughby is still nearer to it, than those in a beaver's tail.

XXII. Description of a Rare American Plant of the Brownæa kind; with some Remarks on this Genus. By Mr. Peter Jonas Bergius, F. R. S. p. 173.

As the Leucandendra, Bruniæ, Diosmæ, Phylicæ, Hermanniæ, &c. are peculiar to Africa, so are likewise the Varroniæ, Ehretiæ, Samydæ, Malpighiæ, Cacti, Brownæa, &c. peculiar to America, not having been found in any other country: at present Mr. B. confines himself to the last mentioned kind. Mr. Jacquin, during his botanical travels in America, founded this genus, in memory of Dr. Patrick Browne, the celebrated English botanist; but Jacquin found only one species of this genus; neither was Sir Ch. Linné hitherto acquainted with any more. Mr. B. has now specimens of a new species of this kind, which he received from Mr. Pihl, who gathered it in Portobello in America, which will afford an opportunity of exhibiting the whole genus of the Brownæa, and the specifical differences of it. If we compare Mr. Jacquin's description of his species with this, we see how carefully nature has observed the same order and position of the essential parts in both; a circumstance common to all natural genera. Mr. B. does not know whether this plant will vegetate and thrive in our stoves or green-houses; if it does, he is convinced it will make a beautiful appearance with its assemblage of purple or blood-red flowers.

Genus Brownæa.—1. Brownæa *(coccinea)* B. with separate umbellated flowers. Brownæa coccinea. Linn. Spec. Plant. 958. Jacquin, Hist. Stirp. Amer. 194, t. 121. Native of rocky and woody places.

2. Brownæa *(Rosa de monte)* B. with aggregate headed sessile flowers, with very long stamens. Hermesias. Loefling. Itin. p. 278. Native of mountainous places.

Descr. Trunk arboreous; *branches* torulose with a cinereous bark; *branchlets* (or common petioles) subalternate, cylindric, smooth, with a cork-like wrinkled joint at the base, spreading; *leaves* coriaceous, a span's length, opposite, perfectly entire, ovate oblong, lengthened sharp, smooth on both sides, with obsolete alternate nerves, shortly footstalked, the lower ones gradually smaller, the lowest ovate, subcordate at the base; *petiolets* short, thick, wrinkled; *flowers* within a common calyx, aggregated into a roundish head or fascicle, very beautiful, of the size of a fist; *fascicles* solitary, alternate, distant, sessile, subaxillary; *calyx common* imbricate, leaflets or bractes ovate, rather sharp, submembranaceous concave, rather lax, smooth, about two thumbs breadth long, red: each including single, or even two or three flowers; deciduous; the exterior rounded; the interior smaller, gradually linear; *perianth. proper* cylindric, tubulate, above rather enlarged, red, villose, bifid; with the divisions ovate, sharpish, subequal, erect; *corol. universal* uniform, blood-red; *proper* double; *exterior* infundibuliform, longer than calyx: tube cylindric, subangulate, narrowed downwards, sub-

coriacecus, permanent; border five-cleft, (often four-cleft): divisions lanceolate, obtuse, erect, unequal, one twice the breadth of the others, deciduous; *interior* pentapetalous, petals ovate lanceolate, obtuse, broadish, erect, nearly twice the length of the outer corol: claws subulate, inserted into the margin of the tube of the outer corol; *stamens. filaments* constantly eleven, filiform, very long, i. e. twice the length of the corol, erect, subcurvate, equal, beneath coalescing into an entire tube, opening in front, surrounding the germ, growing to the margin of the tube of the outer corol, then split into filaments equal at the base; *anthers* ovate, incumbent; *pistil. germ* superior, footstalked from the tube of the outer corol: the footstalk growing to the tube, cylindric, downy; *style* filiform, length of stamens, inflected; *stigma* simple; *pericarp. legume* oblong, compressed, narrowed about the dissepiment, common bilocular; *dissepiment* membranaceous; *seeds* solitary, ovate, compressed, somewhat wrinkled, covered with fungous fibres.

XXIII. *Fatal Effect of Lightning, in a Letter from the Rev. S. Kirkshaw, D. D., of Leeds.* p. 177.

On the 29th of Sept. last, (1772), about 2 o'clock in the morning, were 3 remarkably loud claps of thunder, attended with proportionable lightning. Mr. Thomas Heartly, formerly wine-merchant of Leeds, but lately retired from business to Harrowgate, lived there in a hired house, the 2d northward from the queen's head. While he was in bed with his wife, she was awaked from sleep by the thunder, and went to the window; but not being afraid, she went to bed again, and fell asleep. About 5 she awaked; and, not perceiving her husband to breathe, though warm, endeavoured to awake him—in vain! She quickly sent for Mr. Hutchinson, a considerable apothecary at Knaresborough, who, on sight of Mr. Heartly, and after some experiments, declared him dead, though still very warm. At her request however, he opened a vein; and Mr. Heartly bled freely, insomuch that the blood did not cease to ooze out of the orifice till the body was put in the coffin, which was on Thursday evening, the 1st instant, viz. October, and it was not even then cold. His hair, which he wore, was considerably burnt, or singed on the right side of his head, which was uppermost, as he lay then on his left side, and the inside of his night-cap, on the same side, was singed or browned, though no where on the outside marked at all. Within the cap was found a splinter from the bed post next to his head, which post was torn and split into many splinters or shivers, from the top to the bottom, though a strong oaken post, and almost new. No wound, or mark of any sort, was discovered on any part of his body; but the lower part of his right cheek was swelled, and much hardened.

In the chamber where this happened, there was a small chimney to the north,

made up, but not quite close, by a chimney board, on which could not be discovered any mark or hole, or other indication of the lightning passing that way. Between that chimney and the west end of the room, stands the bed, in the N. W. corner of the room, close to the west and north walls; the deceased lay next the west wall, with his head near the head bed post, in the N. W. corner abovesaid. There is only one window in the room, full east, consisting of 3 pretty large lights, separated by two stone mullions, each light supported by 6 strong iron bars across it, parallel to the floor, and the intermediate one, rather more than one half of it, made into a casement, the frame of which is of iron, and the surrounding frame of the same. In the southermost light, which had 3 squares of glass in breadth, 2 of the lowest squares were perforated in or near the middle, about an inch square; but as some small parts of the glass were gone, he could only guess at the size of the holes, nor could distinctly estimate the shape of them, nor form the slightest conjecture, whether the lightning had made its ingress or egress through both, or either of them. The intermediate square of glass left perfectly sound. There was no other iron about the window, except the abovementioned: but the curtain rods of the bed, which stood about 10 feet from the window, were iron, stronger (larger) somewhat than usual.

Mrs. Heartly lay on Mr. Heartly's left hand, when the thunder was, and felt not the least stroke from the lightning, or perceived any effects from it, except that her right arm, she found, when she awoke, was stunned and benumbed, and a little painful, which continued for a few days, but is now quite well. Dr. K. took notice of a pump, which stood about 10 or 11 feet from the house, in nearly a right line from the window abovementioned, the handle of which is all of iron, very thick and long, and a strong iron ball for a head to it.

XXIV. On the Increase of Population in Anglesey. By Paul Panton, Esq., of Plaswgyn, in Anglesey. p. 180.

I wished to have sent you a fuller account of the state of the population in this island; but so little care has been taken to preserve the parish registers, that scarcely any that are ancient are to be met with. There is great reason to make the pleasing conclusion, that we become more healthy, and increase in population. Heretofore the inhabitants of this island lived chiefly on fish, with which, especially herrings, these coasts were abundantly furnished. Salted herrings were their principal food. This rambling fish, the herring, having left us, our islanders have neglected pursuing other branches of the fishery, and have betaken themselves more to agriculture. The potatoe plant was not cultivated in any great quantities here until of late years; but, since the failure of our herring fishery, it has made great part of the food of the inhabitants. Perhaps the want of the one, and the increased consumption of the other, may be among

the causes that have contributed to the better health of our people. The increase in population in Llanduvnan and Pentraeth parishes, has not been owing to mines, or any new advantage introduced. The inhabitants are wholly employed in husbandry.

The numbers baptized and buried, in 5 parishes, for several periods, are as below:

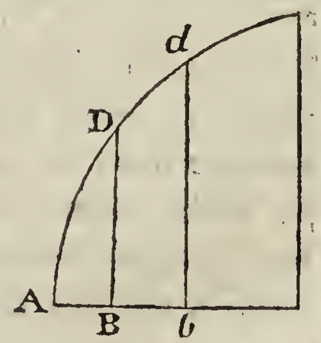
		Baptized.	Buried.
Llansadurn Parish	from 1590 to 1597	34	30
	from 1620 to 1627	34	33
	from 1750 to 1757	68	50
	from 1764 to 1771	69	68
Pentraeth Parish	from 1664 to 1671	69	188
	from 1672 to 1779	102	106
	from 1740 to 1747	100	85
	from 1764 to 1771	149	80
Llanwair yn Geornwy Parish	from 1732 to 1739	68	67
	from 1764 to 1771	101	77
Beaumaris Parish	from 1676 to 1683	135	174
	from 1710 to 1717	236	212
	from 1764 to 1771	328	249
Llanddyfnan Parish	from 1547 to 1554	36	39
	from 1620 to 1627	44	67
	from 1750 to 1757	111	46
	from 1764 to 1771	154	108

XXV. A Letter to the Rev. N. Maskelyne, F. R. S., from Mr. Bailly, of the Royal Acad. of Sciences at Paris: Containing a Proposal of some new Methods of improving the Theory of Jupiter's Satellites. Translated from the French. To which are subjoined Notes on the same by the Rev. Samuel Horsley.* p. 185.

Sir, Though I have not the honour of being personally known to you, I flatter myself, you will excuse the liberty I have taken, of communicating to you

* This very respectable astronomer, John Sylvain Bailly, was born at Paris in 1736, and was cut off in 1793, at 57 years of age. Early in life he showed a strong attachment to the sciences, and while very young communicated some valuable memoirs to the Royal Acad., of which he was elected a member in 1764. In 1768 he published the Eloge of Leibnitz, for which he had the honour of a gold medal from the academy of Berlin. This was followed by the Eloges of Corneille and Lacaille, which, with the former, were collected together. In 1775 appeared the first volume of his great and celebrated work, the History of Astronomy, the 4th and last volume of which came out in 1779. Besides these principal works, he published several astronomical observations and historical disquisitions. Mr. B. entered warmly into the convulsions of his native country, at the commencement of the revolution, and filled the critical office of president of the first national assembly. On July 14, 1789, he was also chosen mayor of Paris; but soon lost his popularity, owing to his enforcing obedience to the laws, and to the liberal sentiments he expressed for the royal family. In consequence he resigned his office in 1791, and retired again to the calm pursuit of the celestial sciences. But, in the sanguinary period that followed, he was apprehended, and after a summary process, condemned to be guillotined; which he suffered with firmness, the 12th of November, 1793.

two methods, of my invention, for perfecting the theory of Jupiter's satellites. The former of these methods serves to measure their diameters, and the latter is intended to make the observations comparable with each other, though made in different places, and with different instruments. You know, that the observations of the eclipses of the 3d and 4th satellites, made by different observers, vary from each other 3, 4, and 5 minutes, and sometimes more; and that there is even a pretty sensible difference in those of the 2d. In the 38th page of the preface to my Essay on the Theory of the Satellites, which has been presented to the R. S., I mentioned the inequality discovered by Mr. de Fouchy, and I suggested, that the perfecting of this theory might perhaps depend on the quantity of this inequality, which Mr. de Fouchy has not determined, not having been at leisure to resume the subject, since the year 1732. The segment of the disc which is not eclipsed, when the satellite disappears, must vary in the proportion of the squares of the distances of Jupiter from the sun, and from the earth. This is what a little reflection will make evident to every one, and this is the first cause of the inequality. Since Mr. de Fouchy's observation, it has been discovered, that the light of the satellite also decreases, in proportion to the proximity of Jupiter's disc; the brightness of the planet weakens that of the satellite, and, for this reason, the eclipses, which happen too near the opposition [of Jupiter to the sun], are considered as defective. Besides, the light of Jupiter, as well as that of his satellites, is different, in his different elevations above the horizon: when the planet is low, more rays of light are lost, in their passage through a thicker atmosphere; and whenever the light is less, the segment, which is not eclipsed when the satellite disappears, and which I call the insensible segment, increases, and occasions another inequality in the moment of the eclipses; lastly, the power of the telescopes, or their aperture, which, according as it is greater or less, give more or less light, contributes to the variation of this segment. Here then are 4 causes of inequality, which I reduce to one principle, and the following is the scope of my researches. When the satellite disappears, there is certainly a segment of its disc which remains uneclipsed; the magnitude of this varies, on account of the 4 causes just mentioned; thence it follows, that if in one eclipse the segment is ABD, and in another A*bd*, when the satellite disappears in the 2d eclipse, it will have got less into the shade, by a part of its diameter B*b*: which part B*b*, therefore, must be the value of the equation between the two eclipses. Now, if we call A*b*, a; AB, b, the radius of the disc of the satellite r, the semidiameter of the shadow, taken from the tables, R, and the total duration of the eclipse d, the time taken up in going over B*b*, or the equation ([a]), will be $\frac{2Rr(a-b)}{d}$, which

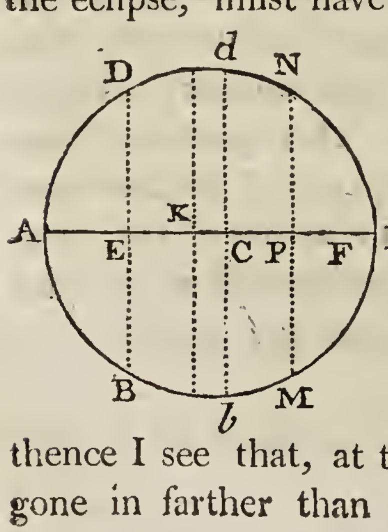


contains 3 unknown quantities, viz. the versed sines a and b, of the two invisible segments, and the semidiameter of the satellite's disc: for you know, sir, that there is nothing to be depended on, in all that has been done on the diameters of the satellites by Cassini, Whiston, and Maraldi. The following is the way which I have taken, to determine these unknown quantities. I observe, first of all, that 2 of them, a and b, are reducible to one; because, as you will see presently, the 2 segments are always in a known proportion [to the whole disc of the satellite, as well as to each other]; and consequently the proportion of their versed sines Ab, AB, may be obtained, either by calculation, or by a table made for the purpose. In order to discover it, considering that when the satellite disappears, it is from the diminution of its light, I conceived, that one might contrive to imitate, at any time, what happens in the eclipses, by diminishing the light. I have an acroamatic telescope of 5 feet length, and 24 lines aperture. I made some diaphragms of pasteboard, which I could apply on the outside of my object glass, the openings of which lessened, by half lines successively, from 24 lines down to 3. In fine weather, I applied these successively to my object glass, and endeavoured to find out, whether, by trying from the greatest to the less, some one of them could not be found, that would make the satellite disappear. My success in this gave me great satisfaction. One day, for instance, the 3d satellite disappeared, when the opening was reduced to 3 lines, and the 1st, when it was reduced to 6 only; and as, in the telescopes, the quantity of light is in the proportion of the squares of the apertures, I concluded, that the 64th part of the light of the 3d satellite, and the 16th part of the 1st, were insensible; whence it follows, that if, at the instant of an eclipse of the 1st satellite, the 16th part of its light is insensible, the invisible segment ABD will be likewise a 16th part of the disc; and thence it will be easy to compute the versed sine AB. In these first observations, I took care to chuse the time when the satellite was at its greatest elongation; for the insensible part increases prodigiously, and sometimes amounts to a 3d of the disc, when the satellite is very near the edge of Jupiter. This variation is much larger than that which takes place in consequence of the distance of Jupiter from the opposition to the sun, and contrary to it. As it is scarcely possible to estimate the law of the variations of this segment, occasioned by the proximity of Jupiter's disc, I judged that they ought to be determined by observation. Accordingly, I followed the satellite from the edge of Jupiter's disc, to the furthest limit of its eclipses, that is, with respect to the 1st, to the distance of 2 semidiameters of Jupiter. Having thus several points by observation, I got the rest by interpolation, and made a table of the variations of the invisible segment, which depend on the distance from the edge of Jupiter; a similar table I likewise made for each of the first 3 satellites; but have not yet been able to make sufficient observations on

the 4th. These tables are contained in a long paper of mine, which will be published in the volume of our academy for 1771; but, if you please, I will send them to you. These segments being known, it is clear that, besides their variations occasioned by the distance of the satellite from the edge of Jupiter, they will be liable to others. First, in consequence of the change of Jupiter's distances, both from the sun and from the earth. On this account, the magnitudes of these segments being known, for a particular epoch,* those known magnitudes must be multiplied by $\frac{m^2n^2}{p^2q^2}$, to determine the magnitude of the segment at any other time. In which expression, p and q denote the distances of Jupiter from the sun and from the earth respectively, at the given epoch, and m and n the distances, at the other time, for which the value of the invisible segment is required. 2dly. There will be other variations, depending on Jupiter's height above the horizon. The segments which I have observed, have all been reduced to the constant height of 15°. Mr. Bouguer, in his optics, has given a table of the degrees of light of the planets, at their different elevations above the horizon, which, from my own observations, I have found to be very exact, and useful for the present purpose. Now, as the segments are in the inverse ratio of the numbers of this table, putting g for the number corresponding to the elevation of 15°, and h for the number corresponding to any other elevation, the segments must be multiplied by $\frac{g}{h}$. 3dly. These segments will yet be subject to another variation, depending on the aperture of the telescope. It is certain that a larger aperture giving more light, the insensible part of the disc must be smaller; and it seems demonstrable by theory, that this insensible part, or the invisible segments, must be inversely as the squares of the apertures. I resolved however to assure myself of this by experiment. For this purpose, I carried my telescope to Mr. Messier's observatory, who has one of Dollond's telescopes, of $3\frac{1}{2}$ feet length, and 40 lines aperture. On the 20th of August 1771, he saw the 2d satellite disappear in his telescope, through an aperture of 3 lines. The same satellite disappeared in mine, when the aperture was reduced to the same quantity of 3 lines, and not before. We changed instruments, and, repeating the experiment, found the same effect. Now, the insensible part was $\frac{9}{1600}$ of the disc, in Mr. Messier's instrument, and $\frac{9}{576}$ in mine. These portions therefore, in these telescopes, were in the inverse ratio of the squares of the apertures. Consequently, in order to determine the segments for an aperture of any number of lines k, the segments of my table, which are all calculated for an aperture of 24 lines, must be multiplied by $\frac{576}{k^2}$. Hence, to compute the invisible seg-

* Known, by the author's tables, for any distance of the satellite from the edge of Jupiter, at the particular epoch to which the tables are adapted.—Orig.

ment for any particular eclipse, the actual distance of the satellite from the edge of Jupiter being known, look for the quantity of the invisible segment which answers to that distance, in my table, and multiply this quantity by $\frac{m^2n^2}{p^2q^2} \times \frac{g}{h} \times \frac{576}{k^2}$. If two different observations, made in the same place, or rather two observations made in different places, by different observers, are to be compared, the invisible segment must be determined, such as it was for each observer; AB and Ab, the versed sines of these segments must be computed, and in the expression $\frac{2\text{R}r\,(a-b)}{d}$, the only remaining unknown quantity will be r. The following is the method I have hit upon for determining it. I considered, that by trying different diaphragms successively, some few minutes before an immersion, it would be easy to find out the particular size which would make the satellite disappear; and that the proportion of the invisible segment to the whole disc of the satellite, for that instant, would by that means be determined. Suppose then that I have found this diaphragm: my next step is, to cover the object-glass of my telescope with a diaphragm somewhat larger, which suffers me just to perceive the satellite, but so weak and small, that the least further diminution of its light must render it invisible. I wait till it actually disappears; I write down this time, then take away the diaphragm, and the number of seconds which pass between this first disappearance and the true immersion, giving me a great part of the diameter, I easily compute the whole. The following is an example of my method. On the 26th of June 1771, there was an immersion of the 3d satellite, at 56^m after 9 in the evening. I found the diaphragm which made the satellite disappear, to be of 12 lines. I then fitted my glass with a diaphragm of 17 lines; I might have taken one much smaller: presently the satellite disappears. But removing the diaphragm, I see the satellite again, very distinctly, for 2^m 18^s; after which the true immersion followed. Now this is my calculation. The aperture of the diaphragm, which made the satellite disappear, being 12 lines by observation, the invisible segment at the instant of the eclipse, must have been a quarter of the disc. Let ABD be this quarter. I



I know that, at the instant of the immersion, the satellite had entered the shade, by the whole part EF of its diameter. I say then, if on an aperture of 24 lines, the part ABD is insensible, the insensible part, on an aperture of 17 lines, will be larger than ABD, in the ratio of the square of 24 to the square of 17. Saying, then as $17^2 : 24^2 :: 0.25000 :: x$, x comes out $=$ 0.49827, or near half the disc, represented by unity; thence I see that, at the instant of the first disappearance, the satellite had not gone in farther than K. Putting the radius AC $=$ 1, the versed sines AE, AK

will be = 0.59602 and 0.99884; consequently EK = 0.40282. Substitute this value of EK instead of $a - b$, in the expression $\frac{2R \times r\,(a-b)}{d}$, and you will have $\frac{2Rr \times 0.40282}{d} = 2^m\ 18^s$, and $r = 5^m\ 23^s$ of time, will be the semidiameter of the satellite, or the whole diameter will be 22′ 34″ (of the satellite's orbit, considered as a circle, or would be seen under an angle of this quantity, from Jupiter's centre). In this observation, as I have already said, I used a diaphragm with too great an opening (*c*), for the first disappearance. Take then a 2d observation. On the first of August 1771, there was an emersion of the 3d satellite, about 15^m after 9. I marked the instant of this emersion; then I furnished my telescope with a diaphragm of 8 lines. The satellite disappeared, and did not begin to appear again till at the end of 6^m 24^s. Some minutes after, when it was quite come out of the shadow, I measured the diaphragm, which would make it disappear, and found it of 7 lines. These 7 lines give a segment ABD of 0.08507. Then saying $8^2 : 24^2 :: 0.08507 : x$; x, or the segment ANM, comes out = 0.76562, AE = 0.27994, AP = 1.43098, and EP = 1.15104. Therefore $\frac{2Rr \times 1.15104}{d} = 6^m\ 24^s$. From this equation r comes out 5^m 20^s, and the whole diameter 22′ 22″, (in parts of the circular orbit). These two conclusions agree so perfectly well with each other, that, if I am not too fond of my own work, I may venture to say, the method I have invented may be carried to great exactness. I hope you will have the goodness to give me your opinion of it, which I have the greatest respect for, and will be very useful to me, especially if you have leisure to repeat the observations. I shall take the liberty to subjoin a few hints, on the manner of making them, at the end of this letter. The diameter of the 1st satellite I have determined by 3 observations as follows:

An Immersion	1771, June 30	gave	7^m	17^s	in time or	1°	1′	45″	as seen from ♃.
Emersion	Aug. 1		7	3		0	59	46	
Emersion	Sept. 2		7	1		0	59	29	

You see, sir, that this agreement is likewise very satisfactory. Mr. Messier took part in these observations, and found the application of them very easy. He himself observed the diameter of the 2d satellite, by the emersion of the 30th of August; but it emerged at so small a distance from the 1st, that this circumstance may have vitiated the observation; the diameter of this satellite must therefore be verified by fresh observations. However, the result of Mr. Messier's makes it 7^m 2^s in time, or 29′ 42″ seen from Jupiter; and a former observation of mine, of the 11th of July, gave the same quantity precisely. Thus, by means of the tables given in my paper, which I had the honour of mentioning to you, it will be possible to compute the invisible segment, for all the observations which have been hitherto made, and the diameter of the satellite being

likewise ascertained, to reduce the instant of the observed eclipse to that of the passage of the centre over the edge of the shadow, which will be a fixed term for all the observations, and all the observers, who but seldom agree in the observation of the same eclipse. I confess that the transparency of the air is not always the same, and that a greater or less degree of transparency will make the segments smaller or larger, and consequently affect the observation. The inequality of sight may likewise occasion some error; for though it might be possible to settle the general effect of the difference of sight of different observers, the sight of the same person is not constantly the same, and even independently of the change produced by age, may not have the same strength at all times. But by the method I propose, all these inconveniencies will be remedied, in future observations, with little trouble. Every observer is to furnish himself with several diaphragms of pasteboard, gradually diminishing by half-lines, to be applied to the object-glass externally, and some minutes before an immersion, or after an emersion, he is to determine which of them intercepts from him the sight of the satellite. Having found this, and knowing also the diameter of the satellite, he will reduce, by the process of calculation already explained, the observed instant of the eclipse, to that of the passage of the centre; which is the same, as I said before, for all the observers in the world. You see, sir, what advantages would arise, from this agreement, for the theory of the satellites, and the precision of the terrestrial longitudes. This method takes in every thing; the difference of glasses, that of sights, the greater or less transparency of the atmosphere, &c. Observation gives the segment greater or less, in proportion to the combined influence of all these causes. The principal advantage of this method, which requires only a very simple calculation, is, that it depends on no hypothesis. It enables us to measure immediately the light of the satellite, whether increased or diminished by all the causes above mentioned; to measure, I say, the real impression of that light upon the eye, whatever be the actual state of the organ. I must add, that I am sensible, the determination of the invisible segment, by means of the diaphragm, might be inconvenient to those, who make use of large telescopes for the eclipses of the satellites, were it not, that this observation may be equally well made with a smaller telescope, provided only, that it be sufficient to see and distinguish the 4 satellites; and after the diaphragm is determined by this smaller telescope, the larger one may be used for the observation of the eclipse. For these measures are easily transferred from one instrument to another, the invisible segments in different telescopes, being inversely as the squares of the apertures. For reflectors, I have a method of the same kind with the former, grounded at least on the same principles, by which I can determine their power, and compare them, both with each other, and with the refracting telescopes. I shall conclude with some hints concerning the observa-

tion of the diaphragm, for determining the invisible segment. To repeat these observations with judgment, it will be necessary to recollect the intention of them; which is, to measure what portion of the disc remains illumined, that is, what portion of the satellite's light continues, though unperceived, to be transmitted to the observer's eye, at the instant when the satellite disappears, on the brink of an eclipse. In lessening gradually the aperture of the glass, the observer should not begin with too small an opening; because the eye, not accustomed to the great obscurity which follows, might not see the satellite at all. As the opening is gradually contracted, the satellite seems to grow less. The observer sometimes loses sight of it for a moment; but if he continues to look attentively, he sees it again. The real disappearance is only to be concluded, when, on fixing with steady eyes, for about half a minute, on the place it occupied, it is seen no more; for if one persisted to observe it much longer, it might happen, that it might be seen to glimmer at times, and immediately disappear. I have always made it a rule, to consider the debilitation of the light, in this degree, as actual disparition, and it is necessary that observers should agree on this point, in order that their different estimations may be consistent. These fits of momentary glimmering and extinction are undoubtedly owing to the motion of the particles of the atmosphere. In the clearest weather, there are always particles of vapour floating in it, in vast abundance; according as these particles place themselves in the direction of the ray of light, or out of it, the light of the satellite is diminished or restored, and the satellite, in consequence, is either hid or rendered visible. This does not happen in eclipses, wherein a great part of the light is in reality extinguished. But in the case I am now speaking of, though the diminution of the aperture of the glass does indeed take away a great quantity of it, yet this quantity is always relative to the actual state of the atmosphere: ([d]): if that state changes, this quantity becomes alternately sensible or insensible, according as the light meets with more or less obstructions in its passage from the vapours. Another thing, which it will be necessary to point out, is, that the operation with the diaphragms, for determining the invisible segment, must be made and concluded before the satellite has touched the shadow. The proper time therefore for beginning this observation, will be determined by the time the diameter takes in entering, which, in the perpendicular ingress, or when Jupiter is in the nodes, is 7^m for the first 2, and 11^m for the 3d. The time of the oblique ingress is $(7^m)\frac{2R}{d}$ for the first two, and $(11^m)\frac{2R}{d}$ for the 3d; which, for this last, may in extreme cases amount to about 27^m or 28^m. It is proper to take 5^m more; for the observations of the diaphragm will take up 2^m, even when use has rendered them familiar, and the tables may be 2^m or 3^m behind. At present, it is sufficient to begin the observation 16^m before an eclipse of the 3d satellite; but there are times, in which it would be neces-

sary to begin 29^m or 30^m before. It is essential not to begin too late, for fear of missing the observation; it is also essential not to begin too soon, because then the segment measured would be too small, as the satellite is continually either approaching to Jupiter, or receding from him. All this is hastily explained; but these matters are so familiar to you, that you cannot but understand me, and this letter is already too long. I am afraid it will tire you; but I am extremely desirous of having the exactness and utility of these two methods, the one for the measure of the diameters, the other, for making all the observations capable of mutual comparison, ascertained, by repeating the observation of the diaphragm in every eclipse. I cannot take a better way than to consult the several astronomers, who, like you, besides being deeply skilled in the theory, are the most celebrated observers. If they will adopt this method, it will be the best way of making it general, as others will follow it of course. I have communicated it to Mr. Messier, who proposes making use of it. Mr. Maraldi, who is gone to his house near Nice, has tried the observation of the diaphragm with success, with an achromatic telescope $3\frac{1}{2}$ feet long; and he would already have made use of it, but that it is impracticable with the telescope of 15 feet, which he uses for the eclipses of the satellites. I have written to him that he may observe the eclipse with his usual telescope, and the diaphragm with the achromatic; so that I make no doubt he will use this method as soon as Jupiter shall have come out of the sun's rays. These, Sir, are the things on which I wish to consult you, and have your advice. I shall be much flattered by your communicating this letter to the R. S., if you think it deserves attention.

Notes on the Foregoing Paper. By the Rev. Samuel Horsley. p. 213.

(a) $\frac{2\text{R}r\,(a-b)}{d}$. This formula is deduced from the following principles. 1st. That the motion of the satellite, in its orbit, is uniform, or at least may be considered as such, without sensible error, in the present investigation. 2. That the time which the semidiameter takes to enter the shadow, in any eclipse, is inversely as the whole time of the duration of the eclipse. 3. That the time which any given part of the semidiameter takes to enter the shadow, is to the time which the whole semidiameter takes to enter, as that part to the whole.

Now, let a and b denote the versed sines of the arcs Ad, AD (in the first figure) respectively, the radius being unity. Let R denote the half-time of the duration of an eclipse, when Jupiter is in the node of the satellite's orbit, r, the time which the semidiameter takes to enter the shadow in such eclipses; d, the whole duration of an eclipse, happening when Jupiter is at any given distance from the node. Then will $\frac{2\text{R}r}{d}$ express the time which the semidiameter of the satellite will take to enter the shadow, in the eclipse whose duration is d (by 2d, because $d : 2\text{R} = r : \frac{2\text{R}r}{d}$). And, $\frac{2\text{R}r}{d}$ being the time that the semidiameter takes to enter the shadow, $\frac{2\text{R}r\,(a-b)}{d}$ will be the time that the part Bb takes to enter, by 3d.

It is to be observed, that, to compare two eclipses by this formula, it is necessary, that the planet should have been at the same distance from the node of the satellite's orbit, at the commencement of

both. For comparing eclipses otherwise circumstanced, a more general formula may easily be deduced from the same principles. If Ad be the insensible segment in one eclipse, A D in another (vide the first figure), a the versed sine of the arc Ad, b, of A D, the radius being unity, d the whole duration of the first eclipse, δ of the 2d, then $\frac{2\text{R}r}{d\delta} \times (\delta a - db)$ is what the author would call the equation, between the two, arising from the different magnitudes of the insensible segment, or the time by which the interval between the observed eclipses, differs from the interval between the real passage of the centre in each eclipse. This is a general formula, for all eclipses of the same satellite. If the planet's distance from the node has been the same in both, then $\delta = d$, and this formula changes into the author's. The more general one is here given, rather for the fuller explication of the theory, than for any necessity that there is to have recourse to it in practice. For, though the use of it may sometimes be convenient, eclipses of the same satellite may always be compared without it, when once the diameter of the satellite is known, and the magnitude of the insensible segment in each eclipse determined, by reducing the observed immersion or emersion to the true ingress or egress of the centre.

(^{b}) The words printed in the Italic characters are designedly omitted in the translation, it being apprehended, that it is owing to some inadvertency, that they appear in the author's text. For unless they are expunged, the general description, here intended, of the author's method of determining the diameters of the satellites, will by no means agree with the examples of that method immediately subjoined. These words imply, that the author takes the instant of the disparition of the satellite, in the contracted aperture of the diaphragm, for the moment of the contact of the satellite's limb with the edge of the shadow, and makes that moment so determined, the basis of his calculations: reasoning as it should seem thus. 'When any part of the diameter of the satellite, however small, has entered the shadow, some part of the light, which the observer receives, through the aperture of the diaphragm, from the whole unshaded disc, will be intercepted. But that aperture is so small, that the light transmitted through it, from the whole unshaded disc, is but just sufficient to be sensible; and must therefore cease to be so, when it is in the smallest degree diminished, i. e. when the very smallest part imaginable of the disc is shaded. Therefore the moment of the disparition, in the aperture of the diaphragm, is the true commencement of the eclipse, or differs from it by less than any assignable difference.'

But it appears, from the examples given afterwards, that the author's calculations proceed on much safer principles. Having determined the portion of the disc, that is insensible on the whole given aperture of his telescope, he computes what larger portion will be insensible, on the smaller given aperture of his diaphragm. And then, by observing the two disparitions, the earlier one in the diaphragm, the other in the telescope with the object-glass uncovered, the last of which he calls the true immersion, he knows the time in which a given portion of the diameter enters the shadow, and consequently the time in which the whole enters; which determines the magnitude of the whole, in parts of the satellite's orbit, or its apparent magnitude to an observer of Jupiter's centre.

(^{c}) The disadvantage of using too great an aperture is, that the part of the diameter obtained by observation, from which the whole is to be concluded, will be less than the same method of observation would give, with a more contracted aperture. For the larger the aperture of the diaphragm is which is applied to the object-glass, the less is the difference between that aperture and the whole aperture of the telescope; consequently the less is the difference between the segments, which are insensible in these apertures severally, and the less the portion of the diameter, which passes over the shadow's edge, between the two disparitions.

(^{d}) In eclipses, when once the satellite has disappeared, or is become visible, the author says, we are not to expect those fits of glimmering and extinction, which he has described as taking place, when we observe the uneclipsed satellite with very contracted apertures. The reason is plainly this: in immersions, a part of the disc is still indeed enlightened, when the satellite disappears; and the quantity of light, transmitted from this part to the observer's eye, must be very different, in different states of

the air's transparency; and consequently the satellite, after having disappeared, might become visible again, by a sudden increase of the air's transparency in the tract of the satellite's light, provided the magnitude of the unshaded part remained, at the instant of the increased transparency, what it was when the satellite first disappeared. But as this is not the case, as the unshaded part is continually becoming less, the satellite cannot reappear, unless the increase of transparency be such, as to overbalance the diminution of light made by the progress of the eclipse. And the motion into the shade is so quick, that this can rarely, if ever, happen. By the like reasoning, fits of extinction are not to be expected, when once the satellite has shown itself in an emersion.

The author of these remarks does not imagine, that any apology is necessary for the liberty he has taken. He has the highest opinion of the merit of Mr. Bailly's invention; and this has excited him to contribute what he could to obviate objections, and to prevent mistakes.

XXVI. A short Account of an Explosion of Air, in a Coal-pit, at Middleton, near Leeds in Yorkshire. In a Letter from Mr. W. Barnard, of Deptford. p. 217.

I have at length procured from my father, a memorandum made by him on the spot, of the effects of foul air set on fire, which I have copied exactly as below:

"Being engaged in Middleton wood, the estate of Cha. Brandling, Esq. near Leeds in Yorkshire, in directing the falling and barking of a large quantity of timber bought of him in May 1758, I was witness of the following accident. Some miners, being to renew their operations on the shaft of a coal-pit, which, in a former year, had been sunk to the depth of 60 yards, in order to get through a stratum of very hard stone, thought proper to drill holes, and fill them with gunpowder. They afterwards from the top, threw down fire to blast the stone, which made a report little louder than that of a pistol; but the blaze setting the foul air on fire, produced an effect truly shocking. The whole wood was shaken, the works at the mouth of the pit were all blown to pieces, and the explosion was such as cannot be described. The vacuum in the air was so considerable, that oak trees of a load or more each, at a great distance from the pit's mouth, that before stood upright, stooped towards the pit very much, and must have fallen wholly down, had not the air been instantly replaced. The bark-pullers, at a quarter of a mile from the pit, were so alarmed by the shaking and explosion, that not one of them would have remained in the wood, had they attempted to blast it again. N. B. The trees in the whole circuit stooped towards the pit."

XXVII. Extract of a Register of the Barometer, Thermometer, and Rain, at Lyndon, Rutland, 1772. By T. Barker, Esq. p. 221.

This is a register of the highest, lowest, and mean state, of the barometer and thermometer, as also the quantity of rain, for each month of the year 1772. The whole depth of rain for the year was $28\frac{1}{3}$ inches.

XXVIII. Observations on the Lagopus, or Ptarmigan. By the Hon. Daines Barrington, V. P. R. S. p. 224.

The many different specimens of lagopi, both in their winter and summer plumage, which have lately been presented to the R. S. from Hudson's Bay, enable us to correct many mistakes that have hitherto been made in the description of this bird; as well as the unnecessarily multiplying of the species of the tetrao genus. As M. de Buffon is the last ornithologist who has made any observations on this bird, it may not be improper to take notice of some of his supposed inaccuracies. The lagopus, of which he gives an engraving, is in its winter plumage; and the feet of the bird are consequently covered very thickly with feathers. M. de Buffon however, from not having examined the specimens of the lagopus with proper attention, says, that Aristotle could not have been acquainted with this bird, because the under parts of the claws are entirely covered with feathers; which circumstance is so very striking and peculiar, that it could not have escaped this father of natural history. If a winter specimen however of the lagopus, or ptarmigan, be accurately examined, it will be found, that no feathers grow precisely under the claws; though, by wrapping very thickly round them, they have very strongly that appearance: and, in a summer specimen, not only the feet, but even the legs, are rather bare of plumage. If Aristotle therefore had procured the bird in its summer dress, he could not have observed this very striking circumstance, which M. de Buffon relies on as so strongly characteristic.

The same difference between the plumage in summer and winter is experienced in each of the three species of tetrao, which have (according to one of Linnæus's subdivisions) feathered feet; and it is usually said with us, that they have in winter their snow boots. M. de Buffon therefore unjustly charges the author of the British Zoology, for supposing that this is a wise provision of Nature against the inclemency of the season, when he says, that the vrogallus minor, or our black cock, has not the same protection for its feet, though it buries itself under the snow, and, becoming torpid, equally wants such additional warmth. With regard to the torpidity of this bird, M. de Buffon relies on Linnæus's asserting, that sæpe sepelitur in nive; which by no means signifies that the bird is torpid, but only that it buries itself sometimes under the snow; as sheep do with us in the more rigorous seasons, when it lies very deep in the mountains. The black cock however is so far from being torpid in the winter, that it even approaches the habitation of man when distressed for food; and Mr. B. concludes, till he shall see a specimen which proves the contrary, that, like the other tetraos, whose feet are covered low with feathers, this part of the plumage becomes thicker in winter. M. de Buffon also seems to be mistaken in supposing, that the thick plumage round the feet is peculiar to the lagopus; as it is believed that

Linnæus's first division of this genus have, all of them, the same additional cloathing for the winter; nor is this extraordinary warmth confined merely to this genus, as the noble specimen of the large white owl, which has lately been presented to the R. S. from Hudson's Bay, is covered about the claws with a plumage of perhaps an equal thickness.

The next remarkable circumstance in this bird is, that the shafts of many of the wing-feathers are black; which M. de Buffon supposes to be only 6; whereas they are 8 in the specimens from Hudson's Bay; the last 2 are indeed of a fainter colour. M. de Buffon next says, that Brisson counts 18 feathers in the tail; and Willoughby 16; which he himself reduces to 14. It seems however that Willoughby's number is the more accurate; and, by examining the difference between the summer and winter specimens, the black feathers of the tail are found covered by 2 upper ones, which in summer are brown, and in winter white. Neither can Mr. B. discover, in any of the specimens, the 2 white feathers in the tail, according to Linnæus's description, rectricibus nigris apice albis, intermediis albis, as the 2 covering feathers before-mentioned cannot, with propriety, be termed intermedii, nor are they white in the summer but brown; so that Linnæus makes a circumstance, which varies with the season, to be a permanent characteristic of the bird.

M. de Buffon next supposes, that Willoughby and Frisch speak of different birds under the name of lagopus; because the first says that the feet are covered with soft, and the latter, with harsh and bristly feathers. The remarks however of these ornithologists are easily reconciled, for, if the finger is drawn according to the course of the feathers, they feel soft; and if in the contrary direction, harsh and bristly. The difference also between Belon, Gesner, and Linnæus, with regard to the call of this bird, is as easily accounted for; because most male birds differ from the female in this respect, and sometimes the young birds from those which are full-grown.

This naturally brings Mr. B. to show, that M. de Buffon (who has great merit in other parts of his Natural History, by not unnecessarily multiplying the species of animals) has, in this kind of tetrao, considered as 2 species what, when properly examined, will turn out to be only the lagopus, or ptarmigan. His chief reason for considering the lagopus of Hudson's Bay as being distinct from the ptarmigan, arises from his asserting, that Mr. Edwards, in his description of that bird, says it is twice as large. Mr. Edwards however only considers the size of the Hudson's Bay lagopus as between that of a pheasant and a partridge, in which he is very accurate; the bird is not only evidently so to the eye, but weighs 3 ounces more than a common partridge.* M. de Buffon likewise

* The partridge, when full grown, weighs 13 ounces, and the ptarmigan 16.—Orig.

seems to make an unnecessary species of tetrao, under the name of le petit tetras, à plumage variable; as his principal argument for this opinion is, that they are not found on the mountains, as the lagopi are. Now it is very clear, from the name given in the catalogue from Hudson's Bay to this bird, of the willow partridge, that it lives entirely in that part of the world on the plains; nor are there, it is believed, any very high mountains in the neighbourhood of our forts.

When M. de Buffon therefore conceives, that the lagopus is always endeavouring to find out snow and ice, and that it carefully avoids the glare of the sun; it should seem that the observation is by no means generally true; because, though the rigour of a Hudson's Bay winter is great, yet the summer is very pleasant, and the snow soon disappears, without which M. de Buffon imagines that the bird cannot exist; though his 9th plate represents the ptarmigan, in his winter dress, surrounded with trees and plants in most luxuriant foliage and vegetation. A new observation is, that the claws are scooped off at the end exactly like a writing pen, only wanting the slit; which circumstance may likewise be seen in the claws of our common grous, or heath-game, though the resemblance is not quite so strong as in the ptarmigan.

Mr. B. concludes with copying, from the catalogue transmitted with the specimens from Hudson's Bay, what further relates to the lagopus; which, as before observed, is there called a willow-partridge.* "The willow-partridges gather together in large flocks in the beginning of October, harbouring amongst the willows, the tops of which are their principal food; they then change to their winter dress. They change again in March, and have their complete summer dress by the latter end of June. They make their nest in the ground in dry ridges; and are so plentiful, that 10000 have been killed in the three forts in one winter."

XXIX. On the Effects of Lightning at Steeple Ashton and Holt, Wilts, June 20, 1772, *extracted from several Letters, communicated by Edw. King, Esq., F. R. S.* p. 231.

L. Eliot, vicar of Steeple Ashton, in Wiltshire, writes, that on the 20th of June, 1772, between 12 and 1 o'clock in the afternoon, a violent storm of thunder and lightning happened at Steeple Ashton, in Wiltshire. During the storm, a woman in the village saw a large quantity of lightning come out of a cloud, part of which is supposed to have fallen on the top of the north chimney

* It is not at all extraordinary, that it should there be considered as a partridge, because the white partridge is the name given to this bird by the old ornithologists, who have very naturally considered edible birds nearly of the same size as partridges, when they have short tails, and as pheasants, when they have long ones.—Orig.

of the vicarage house, attracted probably by an iron hoop that went round the chimney, and by some iron bars placed within it, that formerly made part of an apparatus to prevent its smoking. That the lightning fell on these iron bars is very probable, because the colour of two of them that were contiguous was changed, 9 or 10 inches in length, to a dark blue, like that of a watch spring, no uncommon effect of electrical fire.

In the north parlour, to which this chimney belonged, were the Rev. Mr. Wainhouse, of Steeple Ashton, and the Rev. Mr. Pitcairn, of Trowbridge, the former standing, and the latter sitting in a great chair, with his back to the fire-place, near the wire of a bell. In the south parlour, separated from the other by a hall, were a maid servant and a painter; in the kitchen another maid servant; in the coal-house, 4 or 5 yards from the house, a man servant; near the barn, about 50 or 60 yards from the house, another man servant. When the lightning fell on the house, the man servant near the barn heard a very loud noise, equal, he supposes, to the sound of 20 cannons fired at once, and would have fallen to the ground, if he had not caught hold of something to support himself. The other man servant in the coal-house was struck backward, and felt something, as he describes it, like a stream of warm water poured on the middle of his body, which, if it was not the electric fluid itself, was the heated air expanding itself with violence after the explosion. The maid in the kitchen heard a great noise, but received no shock. The other maid servant, who was standing near the middle of the south parlour, suffered likewise no shock, being only terrified exceedingly with the explosion, and the sparks of fire, which she saw on all sides of her; but the painter, who was in the same room, painting near the chimney and the bell wire, was struck on the left side of his body that was next the wire, from his head to his waist; he felt in particular a severe shock, like the electrical one, in his left wrist, which was marked all round with blue and yellow intermixed; a splinter from the wooden case, that covered the bell wire, struck through his glove, and wounded his hand, and he was stunned for some time. Immediately after the woman had seen the lightning come from the cloud, as above-mentioned, some persons in the village, besides those in or near the vicarage house, were thrown to the ground.

The following is the account, which Mr. Wainhouse and Mr. Pitcairn give, of what happened in the north parlour in which they were. As they were conversing about a loud clap of thunder that had just happened, they saw on a sudden a ball of fire between them, on a level with the face of the former, and about a foot from it. They describe it to have been of the size of a sixpenny loaf, and surrounded with a dark smoke; that it burst with an exceedingly loud noise, like the firing of many cannons at once; that the room was instantly filled with the thickest smoke; and that they perceived a most disagreeable smell,

resembling that of sulphur, vitriol, and other minerals in fusion; insomuch that Mr. Pitcairn thought himself in danger of suffocation. Mr. Wainhouse providentially received no hurt, except a slight scratch in his face from the broken glass that was flying about the room, a kind of stupefaction for some time, and a continued noise in his ears, which noise, the effect of the explosion, happened likewise to Mr. Pitcairn, and others in the house.

The lightning fell on Mr. Pitcairn's right shoulder, made a hole in his coat, about a quarter of an inch in diameter, went under his arm in one line to his breast, thence descended down the lower parts of his body in two irregular lines, about half an inch broad, attracted probably by his watch, the glass of which it shivered in small pieces, and meeting perhaps with a little resistance from it, spread itself round his body, and produced the sensation of a cord, tied close about his waist. A violent pain in his loins immediately followed; and thence to his extremities there seemed to be a total stoppage of circulation, all sensation being lost, and his legs and feet resembling in colour and appearance those of a person actually dead. Besides shivering the glass of his watch, the lightning melted a little of the silver of it, and a small part also of half a crown in his pocket. When it came to the middle of his thigh, it left an impression of a blackish colour, resembling the branch of a tree, which in a few days disappeared; but the lines on his body are still visible, and are of a dark blue, intermixed irregularly with a deep yellow. From the middle of his thigh the lightning changed its direction again, and went down the under side of it to the calf of his leg, and so to his shoe, which was split into several pieces in so remarkable a manner, as justly to claim the inspection of the curious. As soon as Mr. Pitcairn was struck, he sunk in his chair, but was not stunned; his face was blackened, and the features of it distorted. His body was burned in several places, small holes were made in different parts of his clothes, and he lost in some measure the use of his legs for 2 or 3 days; but by proper care he soon recovered, except a weakness and numbness in his right leg, which still remains. What is remarkable, Mr. Pitcairn remembers very well to have seen the ball of fire in the room for a short time, a second or two, after he found himself struck with the lightning. Extraordinary as this circumstance may appear, it may be proper to take notice, that it is entirely agreeable to an observation of the learned and ingenious Dr. Franklin, quoted below.*

The effects of the lightning on the building and furniture were as follow. The north chimney was thrown down, the roof and ceiling near it beat in; large

* In every stroke of lightning, I am of opinion that the stream of the electric fluid, &c. will go considerably out of a direct course for the sake of the assistance of good conductors; and that in this course it is actually moving, though silently and imperceptibly, before the explosion, in and among the conductors, &c. Franklin's Experiments and Observations on Electricity, edit. 4, p. 124.—Orig.

stones were forced out of the walls, some were driven to a considerable distance, one in particular to about 200 feet. The glass of the windows in the north parlour and the chamber over it was forced outwards, except in the casements, which were open, and in which not a pane of glass was broken. The case of a clock in the same parlour fell forwards, and was beaten to pieces; a looking glass over the chimney was thrown on the floor and broken, some of the quicksilver was melted, as was likewise some of the lead belonging to the windows. A bureau, that was locked, was opened; as was also the parlour door, inwards, probably by the external air rushing in to restore the equilibrium. Some bedding in one of the chambers was fired, but the fire was extinguished of itself, or by the rain that fell during the storm, before it was discovered. Several splinters were torn out of a hogshead full of beer, but the cask was not materially damaged, nor the beer spilt. The iron bell wire in both the parlours and the hall was reduced to smoke and entirely dissipated, excepting in those parts where it was twisted, and double, and also the wire springs contiguous to the bell, which the lightning left undamaged, as well as the brass handles and bell itself. The ceiling and wall on each side, where the wire went, was stained irregularly, a foot or more in breadth, with a dark blue intermixed with a deep yellow. It is worth observing, that this iron bell wire was very small, considerably less than a common knitting needle; but though it was itself destroyed, yet it seems to have served as a conductor to the lightning, and to have prevented worse effects than happened. For when the lightning had run along, and consumed all the single wire, and had reached that which was twisted and double in the south parlour, contiguous to the brass handle, which the bell used to be rung with, it made a hole in the wall of 5 or 6 inches in diameter, being attracted probably by an iron stove on the other side in the kitchen chimney, where meeting with several large conductors, handirons, poker, tongs, &c. it seems to have been conveyed into the ground. This appears probable, because the progress of it below stairs could not be traced beyond this hole, which it made in the wall. In the chamber over the kitchen, a small piece of wood was indeed struck out of a bed post, and the glass of half a window was driven outwards; but this does not seem to have been the immediate effect of the lightning, but of the shake from the explosion.

Whether Steeple Ashton is from its situation particularly exposed to thunder storms, is uncertain. It may however be proper to mention, that in the year 1670, July the 25th, a violent storm of thunder and lightning damaged the church steeple, which was 93 feet high; and on the 15th of October in the same year, another thunder storm threw it entirely down, and killed 2 of the workmen, who were repairing it.

P. S. Mr. Field, a painter of Trowbridge, during the storm, observed a ball

of fire vibrate forward and backward in the air over some part of Steeple Ashton, and at last dart down perpendicularly, which in all probability was the ball of fire that Mr. Wainhouse and Mr. Pitcairn saw in the north parlour of the vicarage house. Another circumstance is as follows: after the explosion of the ball of fire in the north parlour, Mr. Pitcairn observed a great quantity of fire of different colours vibrating in the room forwards and backwards with a most extraordinary swift motion.

To the Rev. Mr. Eliot, from Mr. William Paradise.

During the storm a person in this place (Holt) saw a body of fire moving towards a house that is next to mine, though at some distance from it; attracted probably by a large iron bar of 10 or 12 feet long, fixed horizontally to support a high chimney. This body of fire changed its direction, and fell on my house, forced a brick out of the chimney, near that part of it to which the iron bar was fastened, and went through the house to an outer door on the opposite side, which happened to be open; there it burst with a loud noise, like the firing of cannons, and filled the room where I was with smoke and the smell of sulphur. I was fortunately 3 or 4 feet out of the line in which it moved. I was however struck against the wall near which I stood; my body was covered with fire, and I thought for some time I should have been suffocated with smoke, and the smell of sulphur.

XXX. On a singular Sparry Incrustation found in Somersetshire. By Edw. King, Esq., F. R. S. p. 241.

In the parish of High Littleton, Somerset, midway between Bristol and Wells, are several coal-mines; and about the end of 1766, a new shaft, or pit, was opened, for conveying air into an adjoining work; but when this shaft was finished, the water that flowed in from the sides, and which at first was taken up by buckets, greatly incommoded the under-works; and therefore the miners at about the depth of 10 fathoms, and just below the place where the water broke in, affixed to the 4 sides of the pit some wooden shoots, about 4 or 5 inches wide, and as many deep; all of them a little inclined towards one corner, where was a hollow perpendicular pipe or trunk of elm, nearly a long square, being about 7½ inches one way, and 4½ inches the other; and through this the water, that fell into the lateral shoots, was conveyed down to the level, or passage out; which being about 7 fathoms lower than the shoots, the hollow perpendicular trunk was about 14 yards in length. This trunk having been thus fixed up, in the latter end of 1766, was in about 3 years, or rather less, found to be much stopped up; so that, in August 1769, the miners were obliged to take it up; and then, on taking it to pieces, they found the whole cavity, from one end to the other, nearly filled with a sort of sparry incrustation, somewhat

softer than marble, but harder than alabaster, and which therefore Mr. K. calls a species of marble.

The water, that flowed into the pit on all sides, issued from a stratum of hard brown and reddish sand-stone, replete with shining sparry micæ, and some ocherous matter; and had, in its passage through the trunk, regularly filled up the cavity by slow degrees, with solid incrustations; so that the increase of the marble is marked much in the same manner as the increase of the growth of a tree appears to be, when its trunk is cut horizontally: and at last the water had left only a cavity, which appears in the middle of the block, and which was uniform in its figure from one end of the pipe to the other, and nearly similar to the original cavity; but which, at last, not being large enough to let all the water pass, occasioned the discovery. Since that time, in order to prevent the inconvenience, if possible, a new trunk has been made, larger than the first; and yet, in June 1771, this new trunk also was so far filled up with the sparry incrustation, that there was but just room to thrust 4 fingers into the central cavity; and the lateral shoots, or troughs, also have filled so fast, that they have been obliged every now and then to be cleaned out.

Mr. K. adds the following observations. 1st. As the water flowed in from the shoots, on 2 sides of the square trunk or pipe, it is manifest that the streams must have stricken against each other, at the corner of the pipe where they first met, and also at the opposite corner. And, as it is a known principle of mechanics, that a body, which is acted on by 2 forces, moving in different directions, will describe the diagonal of a parallelogram, of which the directions of those forces is the sides; so here, the line in which the two streams met, and impeded each other's motions, has plainly, as the marble increased, gone on in the diagonal of such a parallelogram from both the corners, viz. from that where the pipe joined the shoots, or troughs, and from the opposite one; but it is also very remarkable, that there is such a diagonal line, not only at these corners, but in like manner at the other two; which can be accounted for no otherwise, than by supposing that each of the 2 streams, dashing against the opposite side of the pipe, formed continually, the whole way down, another stream, in a contrary direction; and so, both together, produced the same effect throughout the whole pipe, as if there had been 4 streams flowing over the 4 sides. On examining the block however, very strictly, it appears, that the lines in the diagonal one way, are stronger than those in the diagonal the other way; and indeed the specimen of the pipe, presented to the Society, has even broken in halves, exactly in one of the diagonals, though the block here described remains entire, and has the appearance of having had its sides joined accurately, in the manner in which a skilful workman would fit 4 boards to be glued together.

2dly. At one place there seems to have been, by some accident or other, the

point of a small nail projecting into the pipe; and here, it is very remarkable, that, either by the dashing of the water, or rather perhaps by an effect which iron has been observed to have of hastening and increasing petrification, the incrustation has gone on faster than in other parts of the same side; but so regularly, that, from the point of the nail to the inner cavity, there is a swelling, or protuberance, so uniform, that it makes throughout nearly the same segment of different circles, of which the point of the nail is the common centre; and that not merely directly opposite to the nail, but throughout this whole block, and even farther downwards.

3dly. The regular increase of these segments of circles is visible in each lamina of the block, and in each lamina the diameter of the circle increases in due proportion; so that it is still nearly the same segment; though, if there be any difference, it is rather a smaller portion of a larger circle; as, from the cause which occasioned it one would be led to expect. And with regard to these laminæ it is worth observing,* that as they mark the increase of the marble uniformly all round, as the growth of a tree is marked, only the marble increased inward, whereas a tree grows outward, so they seem to have become visible, and to have been thus distinctly marked, by means of the water bringing, at different times, more or less ochre along with the sparry matter: and this is the more probable, as the whole country all round abounds with beds of ochre, and the waters are sometimes much tinged with it.

4thly. The cavity left in the middle of the block is not perfectly similar to the original cavity of the trunk or pipe; because the water did not flow quite uniformly over the edges, at the ends of the shoots or troughs, in consequence probably of their not lying exactly horizontally; whence more water fell upon and against one part of the sides of the trunk, than against the other.

5thly. The outside of the block has taken off impressions of all the roughnesses, knots, and shivers of the elm boards, which composed the trunk or pipe, even more accurately than they could have been taken off by wax, plaster of Paris, or almost any composition whatever, and certainly much more durably. There is in the Philos. Trans., vol. 60, p. 47, (Abridg., p. 10 of this vol.) a very curious paper, from R. S. Raspe, concerning the production of white marble in a similar manner; in which he mentions the taking off impressions of medallions, by means of petrifying waters. And a paper was read at the R. S. some time ago, containing an account of several impressions, actually so taken off in a short time, in durable marble, by means of a petrifying water, near Bologna in Italy: when some of the impressions were also sent, both to the R. S. and to the British Museum. And, as this block here described, and the whole contents of the pipe, of above 40 feet in length, were formed in less than 3 years, there is reason to conclude, that the water of this mine in Somersetshire is as capable

of being improved to the purposes of a new manufactory, as either that near Bologna, or those of Germany and Bohemia. And it is perhaps worth mentioning, that something of this sort has actually been attempted, with good success, in Peru; for we are told by P. Feuillée (who made several curious observations in South America, both phisiological and astronomical, in 1709), that he saw many statues and beautiful vases, or holy water pots, in the churches at Lima, which were simply cast in moulds, by means alone of a petrifying water near Guankabalika, or Guankavelika. And this circumstance is also mentioned in a Description of Peru, published in 1748, a great part of which is taken from Feuillée's account.

6thly. This block of marble takes a very fine polish, as appears by the specimen, the sections of which are polished: and if casts of medals, or other things, were taken in smooth moulds, well formed, their surfaces would therefore probably appear well polished, as those of the medals did, which came from Bologna.

7thly and lastly. Dr. Pococke, in his Travels, describing a very curious grotto in the island of Candia, or Crete, which exceeded all others that he ever saw in beauty, and the slenderness of the pillars, one of which is near 20 feet high, and even transparent, says, "As I had seen stones of this kind hewn out of a rock at Mount Lebanon, which were used as white marble, and appeared to be alabaster, this made me imagine, that when these sorts of petrifactions are hard enough to receive a polish, they then become the oriental transparent alabaster which is so much valued, and of which there are 2 curious columns at the high altar of St. Mark in Venice."

XXXI. Experiments and Observations on the Singing of Birds. By the Hon. Daines Barrington, V. P. R. S. p. 249.

To chirp, is the first sound which a young bird utters, as a cry for food, and is different in all nestlings, if accurately attended to; so that the hearer may distinguish of what species the birds are, though the nest may hang out of his sight and reach. This cry is very weak and querulous; it is dropped entirely as the bird grows stronger, nor is afterwards intermixed with its song, the chirp of a nightingale, for example, being hoarse and disagreeable.

The call of a bird, is that sound which it is able to make, when about a month old; it is, in most instances, a repetition of one and the same note, is retained by the bird as long as it lives, and is common, generally, to both the cock and hen. The next stage in the notes of a bird is termed, by the bird-catchers, recording, which word is probably derived from a musical instrument, formerly used in England, called a recorder. This attempt in the nestling to sing, may be compared to the imperfect endeavour in a child to babble. This

first essay does not seem to have the least rudiments of the future song; but as the bird grows older and stronger, one may begin to perceive what the nestling is aiming at. While the scholar is thus endeavouring to form his song, when he is once sure of a passage, he commonly raises his tone, which he drops again when he is not equal to what he is attempting; just as a singer raises his voice, when he not only recollects certain parts of a tune with precision, but knows that he can execute them. What the nestling is not thus thoroughly master of, he hurries over, lowering his tone, as if he did not wish to be heard, and could not yet satisfy himself. A young bird commonly continues to record for 10 or 11 months, when he is able to execute every part of his song, which afterwards continues fixed, and is scarcely ever altered. When the bird is thus become perfect in his lesson, he is said to sing his song round, or in all its varieties of passages, which he connects together, and executes without a pause.

Notes in birds are no more innate, than language is in man, and depend entirely on the master under which they are bred, as far as their organs will enable them to imitate the sounds which they have frequent opportunities of hearing. Mr. B. educated nestling linnets under the 3 best singing larks, the skylark, woodlark, and titlark, every one of which, instead of the linnet's song, adhered entirely to that of their respective instructors. When the note of the titlark-linnet was thoroughly fixed, he hung the bird in a room with 2 common linnets, for a quarter of a year, which were full in song; the titlark-linnet, however, did not borrow any passages from the linnet's song, but adhered stedfastly to that of the titlark. Having some curiosity to find out whether a European nestling would equally learn the note of an African bird, he educated a young linnet under a vengolina, which imitated its African master so exactly, without any mixture of the linnet song, that it was impossible to distinguish the one from the other. This vengolina linnet was absolutely perfect, without ever uttering a single note by which it could have been known to be a linnet. In some of his other experiments, however, the nestling linnet retained the call of its own species, or what the bird-catchers term the linnet's chuckle, from some resemblance to that word when pronounced.

Having before stated, that all his nestling linnets were 3 weeks old when taken from the nest; and by that time they frequently learn their own call from the parent birds, which consists of only a single note. To be certain therefore, that a nestling will not have even the call of its species, it should be taken from the nest when only a day or two old; because, though nestlings cannot see till the 7th day, yet they can hear from the instant they are hatched, and probably, from that circumstance, attend to sounds more than they do afterwards, especially as the call of the parents announces the arrival of their food. Mr. B. owns that he is not equal himself, nor can he procure any person to take the trouble of

breeding up a bird of this age, as the odds against its being reared are almost infinite. The warmth indeed of incubation may be, in some measure, supplied by cotton and fires; but these delicate animals require, in this state, being fed almost perpetually, while the nourishment they receive should not only be prepared with great attention, but given in very small portions at a time. Yet he has happened to see both a linnet and a goldfinch which were taken from their nests when only 2 or 3 days old. The first of these belonged to Mr. Matthews, an apothecary at Kensington, which, from a want of other sounds to imitate, almost articulated the words pretty boy, as well as some other short sentences: and Mr. Matthews assured him that he had neither the note nor call of any bird whatsoever.

The goldfinch was reared in the town of Knighton in Radnorshire, which Mr. B. happened to hear as he was walking by the house where it was kept. He thought a wren was singing, and he went into the house to inquire after it, as that little bird seldom lives long in a cage. The people of the house however told him, that they had no bird but a goldfinch, which they conceived to sing its own natural note, as they called it; on which he staid a considerable time in the room, while its notes were merely those of a wren, without the least mixture of goldfinch. On further inquiries, he found that the bird had been taken from the nest when only 2 or 3 days old; that it was hung in a window which was opposite to a small garden, whence the nestling had undoubtedly acquired the notes of the wren, without having had any opportunity of learning even the call of the goldfinch.

These facts seem to prove very decisively, that birds have not any innate ideas of the notes which are supposed to be peculiar to each species. But it will possibly be asked, why in a wild state they adhere so steadily to the same song, in so much that it is well known, before the bird is heard, what notes you are to expect from him. This however arises entirely from the nestlings attending only to the instruction of the parent bird, while it disregards the notes of all others, which may perhaps be singing round him. But, to prove this decisively, Mr. B. took a common sparrow from the nest when it was fledged, and educated him under a linnet: the bird however by accident heard a goldfinch also, and his song was therefore a mixture of the linnet and goldfinch. Mr. B. educated a young robin under a very fine nightingale; which however began already to be out of song, and was perfectly mute in less than a fortnight. This robin afterwards sung 3 parts in 4 nightingale, and the rest of his song was what the bird-catchers call rubbish, or no particular note whatever. He educated a nestling robin under a woodlark-linnet, which was full in song, and hung very near to him for a month together: after which, the robin was removed to another house, where he could only hear a skylark-linnet. The consequence was, that the nestling

did not sing a note of woodlark, though he afterwards hung him again just above the woodlark linnet, but adhered entirely to the song of the skylark linnet. Birds in a wild state do not commonly sing above 10 weeks in the year; which is then also confined to the cocks of a few species: Mr. B. conceives that this last circumstance arises from the superior strength of the muscles of the larynx.

Strength however in these muscles, seems not to be the only requisite; the birds must have also great plenty of food, which seems to be proved sufficiently by birds in a cage singing the greatest part of the year, when the wild ones do not continue in song above 10 weeks. Mr. B. knows well, that the singing of the cock bird in the spring is attributed by many to the motive only of pleasing its mate during incubation. Those however who suppose this, should recollect, that much the greater part of birds do not sing at all: why should their mate therefore be deprived of this solace and amusement? The bird in a cage, which perhaps sings 9 or 10 months in a year, cannot do so from this inducement; and, on the contrary, it arises chiefly from contending with another bird, or indeed against almost any sort of continued noise. Superiority in song gives to birds a most amazing ascendency over each other; as is well known to the bird catchers by the fascinating power of their call birds, which they contrive should moult prematurely for this purpose.

But, to show decisively that the singing of a bird in the spring does not arise from any attention to its mate, a very experienced catcher of nightingales informed him, that some of these birds have jerked the instant they were caught. He has also brought to him a nightingale which had been but a few hours in a cage, and which burst forth in a roar of song. Yet this bird is so sulky on its first confinement, that he must be crammed for 7 or 8 days, as he will otherwise not feed himself: it is also necessary to tie his wings, to prevent his killing himself against the top or sides of the cage.

Mr. B. believes there is no instance of any bird's singing which exceeds our blackbird in size; and possibly this may arise from the difficulty of its concealing itself, if it called the attention of its enemies, not only by bulk, but by the proportionable loudness of its notes. He rather conceives it is for the same reason that no hen bird sings, because this talent would be still more dangerous during incubation; which may possibly also account for the inferiority in point of plumage.

Mr. B. considers how far the singing of birds resembles our known musical intervals, which are never marked more minutely than to half notes; because, though we can form every gradation from half note to half note, by drawing the finger gently over the string of a violin, or covering by degrees the hole of a flute; yet we cannot produce such a minute interval at command, when a quarter note for example is required. Some passages of the song in a

few kinds of birds correspond with the intervals of our musical scale, of which the cuckoo is a striking and known instance: much the greater part however of such song, is not capable of musical notations. As a bird's pitch is higher than that of any instrument, we are at a loss when we attempt to mark their notes in musical characters, which we can so readily apply to such as we can distinguish with precision. An unsurmountable difficulty is, that the intervals used by birds are commonly so minute, that we cannot judge at all of them, from the more gross intervals into which we divide our musical octave. Though we cannot attain the more delicate and imperceptible intervals in the song of birds, yet many of them are capable of whistling tunes with our more gross intervals, as is well known by the common instances of piping bullfinches, and canary birds.

This however arises from mere imitation of what they hear when taken early from the nest; for if the instrument from which they learn is out of tune, they as readily pipe the false, as the true notes of the composition.

The next point of comparison to be made between our music and that of birds is, whether they always sing in the same pitch. The first requisite to make such sounds agreeable to the ear is, that all the birds should sing in the same key, which he believes they do. Now, of all the musical tones which can be distinguished in birds, those of the cuckoo have been most attended to, which form a flat 3d, not only by the observations of a harpsichord tuner, but likewise by those of Kircher, in his Musurgia. Another proof of our musical intervals being originally borrowed from the song of birds, arises from most compositions being in a flat third, where music is simple, and consists merely of melody. The oldest tune Mr. B. heard, is a Welsh one, called Morvar Rhydland, which is composed in a flat 3d; and if the music of the Turks and Chinese be examined in Du Halde and Dr. Shaw, half of the airs are also in a flat 3d. The music of 2 centuries ago is likewise often in a flat 3d, though 99 compositions out of 100 are now in the sharp 3d. The reason however of this alteration seems to be very clear: the flat 3d is plaintive, and consequently adapted to simple movements, such as may be expected in countries where music has not been long cultivated. There is on the other hand a most striking brilliancy in the sharp 3d, which is therefore proper for the amazing improvements in execution, which both singers and players have arrived at within the last fifty years. When Corelli's music was first published, our ablest violinists conceived that it was too difficult to be performed; it is now however the first composition attempted by a scholar. Every year also now produces greater and greater prodigies on other instruments, in point of execution.

Mr. B. before observed, that by attending to a nightingale, as well as a robin which was educated under him, he always found that the notes, reducible to our intervals of the octave, were precisely the same; which is another proof that

birds sing always in the same key. In this circumstance, they differ much from the human singer; because those who are not able to sing at sight, often begin a song either above or below the compass of their voice, which they are not therefore able to go through with. As birds however form the same passages with the same notes, at all times, this mistake of the pitch can never happen in them. Few singers again can continue their own part, while the same passages are sung by another in a different key; or if the same or other passages are sung, so as not to coincide with the musical bar, or time of the first singer. As birds however adhere so stedfastly to the same precise notes in the same passages, though they never trouble themselves about what is called time in music; it follows that a composition may be formed for 2 piping bulfinches, in 2 parts, so as to constitute true harmony, though either of the birds may happen to begin, or stop, when they please.

Mr. B. had observed, that perhaps no bird may be said to sing which is larger than a blackbird, though many of them are taught to speak: the smaller birds however have this power of imitation; though perhaps the larger ones have not organs which may enable them, on the other hand, to sing. And he mentions several expressions among the ancients noticing the speaking of birds.

As it appears from these citations, that so many different sorts of birds have learned to speak, and as Mr. B. has showed that a sparrow may be taught to sing the linnet's note, he scarcely knows what species to fix on, that may be considered as incapable of such imitations; for it is clear, from several experiments before stated, that the utmost endeavours will not be wanting in the bird, if he is endowed with the proper organs. It can therefore only be settled by educating a bird, under proper circumstances, whether he is thus qualified or not; for if one was only to determine this point by conjecture, one should suppose that a sparrow would not imitate the song of the linnet, nor that a nightingale or partridge could be taught to speak.

Considering the size of many singing birds, it is rather amazing at what a distance their notes may be heard. Thus, a nightingale may be very clearly distinguished at more than half a mile, if the evening is calm. Mr. B. has also observed the breath of a robin, which exerted itself, so condensed in a frosty morning, as to be very visible. To make the comparison however with accuracy, between the loudness of a bird's and the human voice, a person should be sent to the spot from which the bird is heard; Mr. B. conceives that, on such trial, the nightingale would be distinguished farther than the man. It must have struck every one, that, in passing under a house where the windows are shut, the singing of a bird is easily heard, when at the same time a conversation cannot be so, though an animated one. Most people, who have not attended to the notes

of birds, suppose that those of every species sing exactly the same notes and passages; which is by no means true, though it is admitted that there is a general resemblance. Thus the London bird catchers prefer the song of the Kentish goldfinches, but Essex chaffinches; and when they sell the bird to those who can thus distinguish, inform the buyer that it has such a note, which is very well understood between them. Some of the nightingale fanciers also prefer a Surry bird to those of Middlesex. These differences in the song of birds of the same species cannot perhaps be compared to any thing more apposite, than the varieties of the provincial dialects. The nightingale seems to have been fixed on, almost universally, as the most capital of singing birds, which superiority it certainly may boldly challenge: one reason however of this bird's being more attended to than others is, that it sings in the night.

In the first place, its tone is infinitely more mellow than that of any other bird, though, at the same time, by a proper exertion of its musical powers, it can be excessively brilliant. When this bird sang its song round, in its whole compass, Mr. B. has observed 16 different beginnings and closes, at the same time that the intermediate notes were commonly varied in their succession with such judgment, as to produce a most pleasing variety. The bird which approaches nearest to the excellence of the nightingale, in this respect, is the skylark; but then the tone is infinitely inferior in point of mellowness: most other singing birds have not above 4 or 5 changes. The next point of superiority in a nightingale is its continuance of song, without a pause, which Mr. B. has observed sometimes not to be less than 20 seconds. Whenever respiration however became necessary, it was taken with as much judgment as by an opera singer. The skylark again, in this particular, is only second to the nightingale. Mr. B. here inserts a table, by which the comparative merit of the British singing birds may be examined, in which the number 20 denotes the point of absolute perfection.

	Mellowness of tone.	Sprightly Notes.	Plaintiff Notes.	Compass.	Execution.
Nightingale	19	14	19	19	19
Skylark	4	19	4	18	18
Woodlark	18	4	17	12	8
Titlark	12	12	12	12	12
Linnet	12	16	12	16	18
Goldfinch	4	19	4	12	12
Chaffinch	4	12	4	8	8
Greenfinch	4	4	4	4	6
Hedge-sparrow	6	0	6	4	4
Aberdavine (or siskin)	2	4	0	4	4
Redpoll	0	4	0	4	4
Thrush	4	4	4	4	4
Blackbird	4	4	0	2	2
Robin	6	16	12	12	12
Wren	0	12	0	4	4
Reed-sparrow	0	4	0	2	2
Blackcap, or the Norfolk mock nightingale	14	12	12	14	14

And here he again repeats, that what he describes is from a caged nightingale, because those which we hear in the spring are so rank, that they seldom sing any thing but short and loud jerks, which consequently cannot be compared to the notes of a caged bird, as the instrument is overstrained. But it is not only in tone and variety that the nightingale excels; the bird also sings with superior judgment and taste. He has commonly observed, that his nightingale began softly like the ancient orators; reserving its breath to swell certain notes, which by this means had a most astonishing effect, and which eludes all verbal description.

It may not be improper here to consider, whether the nightingale may not have a very formidable competitor in the American mocking bird; though almost all travellers agree, that the concert in the European woods is superior to that of the other parts of the globe. As birds are now annually imported in great numbers from Asia, Africa, and America, Mr. B. has often attended to their notes, both singly and in concert, which are certainly not to be compared to those of Europe. It must be admitted, that foreign birds, when brought to Europe, are often heard to a great disadvantage; as many of them, from their great tameness, have certainly been brought up by hand. The soft billed birds also cannot be well brought over, as the succedaneum for insects, their common food, is fresh meat, and particularly the hearts of animals.

Mr. B. has heard the American mocking bird in great perfection at Mess. Vogle's and Scott's, in Love-lane, Eastcheap. This bird had been in England 6 years. During the space of a minute, he imitated the woodlark, chaffinch, blackbird, thrush, and sparrow. He would also bark like a dog; so that the bird seems to have no choice in his imitations, though his pipe comes nearest to our nightingale of any bird yet met with. With regard to the original notes however of this bird, we are still at a loss; as this can only be known by those who are accurately acquainted with the song of the other American birds. Kalm indeed informs us, that the natural song is excellent; but this traveller seems not to have been long enough in America, to have distinguished what were the genuine notes: with us, mimics do not often succeed but in imitations. Mr. B. has little doubt however, but that this bird would be fully equal to the song of the nightingale in its whole compass; but then, from the attention which the mocker pays to any other sort of disagreeable noises, these capital notes would always be debased by a bad mixture.

We have one mocking bird in England, which is the skylark; as, contrary to a general observation before made, this bird will catch the note of any other which hangs near it; even after the skylark note is fixed. For this reason, the bird fanciers often place the skylark next one which has not been long caught, in order, as they term it, to keep the cage skylark honest. The question,

indeed may be asked, why the wild skylark, with these powers of imitation, ever adheres to the parental note; but it must be recollected, that a bird when at liberty is for ever shifting its place, and consequently does not hear the same notes eternally repeated, as when it hangs in a cage near another. In a wild state therefore the skylark adheres to the parental notes; as the parent cock attends the young ones, and is heard by them for a considerable time.

It may be asked, how birds originally came by the notes which are peculiar to each species. The answer however to this is, that the origin of the notes of birds, together with its gradual progress, is as difficult to be traced as that of the different languages in nations. The loss of the parent cock at the critical time for instruction has doubtless produced those varieties, which are in the song of each species; because then the nestling has either attended to the song of some other birds; or perhaps invented some new notes of its own, which are afterwards perpetuated from generation to generation, till similar accidents produce other alterations. The organs of some birds also are probably so defective, that they cannot imitate properly the parental note, as some men can never articulate as they should do. Such defects in the parent bird must again occasion varieties, because these defects will be continued to their descendants, who will only attend to the parental song. Some of these descendants also may have imperfect organs; which will again multiply varieties in the song. The truth is, that scarcely any two birds of the same species have exactly the same notes, if they are accurately attended to, though there is a general resemblance. Thus most people see no difference between one sheep and another, when a large flock is before them. The shepherd however knows each of them, and can swear to them, if they are lost; as can the Lincolnshire gosherd to each goose.

But we may not only improve the notes of birds by a happy mixture, or introduce those which were never before heard in Great-Britain; as we may also improve the instrument with which the passages are executed. If, for example, any bird fancier is particularly fond of what is called the song of the canary bird, which however must be admitted to be inferior in tone to the linnet, it would answer well to any such person, if a nestling linnet was brought up under a canary bird, because the notes would be the same, but the instrument which executes them would be improved. We learn also, from these experiments, that nothing is to be expected from a nestling brought up by hand, if he does not receive the proper instruction from the parent cock: much trouble and some cost is therefore thrown away by many persons in endeavouring to rear nestling nightingales, which, when they are brought up and fed at a very considerable expence, have no song worth attending to. If a woodlark, or skylark, was educated however under a nightingale, it follows that this charge, which amounts to a shilling per week, might be in a great measure saved, as well as the trouble of chopping fresh meat every day. A nightingale, again, when

kept in a cage, does not live often more than a year or two: nor does he sing more than 3 or 4 months; whereas the scholar pitched on may not only be more vivacious, but will continue in song 9 months out of 12.*

XXXII. *On the Tokay and other Wines of Hungary. By Sylvester Douglas,† Esq.* p. 292.

The town, or rather village, of Tokay, whence this celebrated wine derives its name, stands at the foot, and to the east of a high hill, close by the conflux of the river Bodrog, with the Theis or Tibiscus. In the Norimberg map of Hungary, it is erroneously placed between these rivers, for it is on the west side of both. The inhabitants are chiefly either Hungarians of the protestant religion, or Greeks, who came originally from Turkey, but have been long settled here for the purpose of carrying on the wine trade. The hills on which the wine grows, lie all to the west of the river Bodrog, and beginning close by the town of Tokay, thence extend westward and northward, occupying a space of perhaps 10 English miles square; but they are interrupted and interspersed with a great many extensive plains, and several villages. Near some of these the wine is better than what grows on the hill of Tokay, but it all goes under the same general name. The vineyards extend beyond the 48th degree of northern latitude. The soil, on all the hills where the wine grows, is a yellow clayish earth, extremely deep, and there are interspersed through it large loose stones, which it seems are limestone; but he had not an opportunity of examining them.

As the hills do not run in a regular chain, but are scattered among the intervening plains, all kinds of exposures are met with upon them, and there is wine on them all, except perhaps where they are turned directly towards the south. Yet the general rule is, that the exposures most inclining to the south, the steepest declivities, and the highest part of those declivities, produce the best wine. It is a vulgar error, that the Tokay wine is in so small quantity, as never to be found genuine, unless when given in presents by the court of Vienna. The extent of ground on which it grows is a sufficient proof to the contrary. It is a common dessert wine in all the great families at Vienna, and in Hungary, and is very generally drank in Poland and Russia, being used at table in those countries, like Madeira in this.

Another vulgar error is, that all the Tokay wine is the property of the empress queen. She is not even the most considerable proprietor, nor of the best wine; so that every year she sells off her own, and purchases from the other proprietors, to supply her own table, and the presents she makes of it. The greatest

* The above is only a short sketch of the principal parts of Mr. Barrington's paper. But the whole of it may be consulted in the 3d vol. of Pennant's British Zoology.

† Now Lord Glenbervie.

proprietor is the Prince Trautzon, an old man, at whose death indeed his estate will escheat to the crown; but many others of the German and Hungarian nobility have large vineyards at Tokay; most of the gentlemen in the neighbourhood have part of their estates there; the Jesuits college at Ungwar has a considerable share of the best wine; and besides these, there are many of the peasants who have vineyards, which they hold of the queen, or other lords, by paying a tithe of the annual produce.

There is never any red wine made at Tokay, and, as far as he recollects, the grapes are all white. The vintage is always as late as possible. It commonly begins at the feast of St. Simon and Jude, October 28, sometimes as late as St. Martin's, November 11. This is determined by the season, for they have the grapes on the vines as long as the weather permits; as the frosts, which from the end of August are very keen during the nights, are thought to be of great service to the wine. By this means it happens, that when the vintage begins, a great many of the grapes are shrivelled, and have in some measure the appearance of dried raisins.

There are 4 sorts of wine made from the same grapes, which they distinguish at Tokay by the names of Essence, Auspruch, Masslasch, and the common wine. The process for making them is as follows. The half-dried and shrivelled grapes, being carefully picked out from the others, are put into a perforated vessel, where they remain as long as any juice runs off by the mere pressure of their own weight. This is put into small casks, and is called the Essence. On the grapes from which the essence has run off, is poured the expressed juice of the others from which they had been picked, and then they tread them with their feet. The liquor obtained in this manner stands to ferment during a day or two, after which it is poured into small casks, which are kept in the air for about a month, and afterwards put into the cellars. This is the Auspruch.

The same process is again repeated, by the addition of more of the common juice to the grapes which have already undergone the two former pressures, only they are now also wrung with the hands, and this gives the Masslasch. The 4th kind is made by taking all the grapes together at first, and submitting them to the greatest pressure. It is chiefly prepared by the peasants, who have not a sufficient quantity of grapes, and cannot afford the time and apparatus necessary for making the different sorts. It is entirely consumed in the country, and forms the common vin du pays.

The Essence is thick, and never perfectly clear, very sweet and luscious. It is chiefly used to mix with the other kinds, and when joined to the Masslasch, forms a wine equally good with the Auspruch, and often sold for it. The Auspruch is the wine commonly exported, and what is known in foreign countries under the name of Tokay. The following are the best rules for judging of it;

though in this and all similar cases, it requires experience to be able to put such rules in practice. 1. The colour should neither be reddish, which it often is, nor very pale, but a light silver. 2. In trying it, you should not swallow it immediately, but only wet your palate and the tip of the tongue. If it discover any acrimony to the tongue, or bite it, it is not good. The taste ought to be soft and mild. 3. It should, when poured out, form globules in the glass, and have an oily appearance. 4. When genuine, the strongest is always of the best quality. 5. When swallowed, it should have an earthy astringent taste in the mouth, which they call the taste of the root. The Poles particularly are fond of this astringency and austerity in their Tokay. There is so great a difference between the Tokay used in Poland and what Mr. D. drank both at Tokay and Vienna, which, he was sure, was of the best and most genuine kind, that he thinks their wine is composed of the Masslasch, which, by the severe pressure it suffers, must carry with it much of the astringent quality which, in all grapes, resides in the skin, and a smaller proportion than usual of the essence. But this is mere conjecture.

Besides the qualities already mentioned, all Tokay wine has an aromatic taste; so peculiar, that nobody who has ever drank it genuine can confound it with any other species of wine. The only species that bears a resemblance to it grows, in a very small quantity, in the Venetian Friule, and is only to be met with in private families at Venice, where, in the dialect of the place, it is called vin piccolit. The Tokay wine, both the Essence and Auspruch, keeps to any age, and improves by time. Mr. D. has drank of the latter at Vienna, which had been in the same cellar since the year 1686. It is never good till it is about 3 years old. All the sorts are generally kept in small casks, called antheils, which legally hold 80 Hungarian mediæ, a measure containing about two-thirds of an English quart. When you buy it of the gentlemen who are proprietors, you have commonly more than the legal quantity in the antheil; if from the Greek merchants, always less.

The particular year, or vintage, and the age, vary the price of this, as of all other wines. The medium price of the antheil of Essence is between 60 and 70 ducats. It is sometimes sold on the spot for more than 100. Prince Radzivil paid 300 ducats for 2 antheils about 4 years before. When the price is 60 ducats, and the antheil large measure, that is, about 90 mediæ, it is exactly a ducat the English quart. The price of the Auspruch is from 26 to about 30 ducats the antheil. This is at the rate of two florins, or near a crown the English quart. The variety in the prices of the Essence and Auspruch, accounts for the opposite accounts of people, who say sometimes that it costs half a guinea, sometimes 5 shillings, on the spot.

There are people who come every year from Poland, about the time of the

vintage, to choose their own wine on the ground, and see it carefully managed. But it is a false opinion of many, that they contract for the wine of several years forwards: no such thing has ever been practised. For these last 20 years the court of Petersburg has had an agent, who resides constantly at Tokay, for the purpose of buying wine. He commonly purchases every year from 40 to 60 antheils of Auspruch, but never of any other sort.

It is much the best way to transport it in casks; for when it is on the seas, it ferments 3 times every season, and refines itself by these repeated fermentations. When in bottles, there must be an empty space left between the wine and the cork, otherwise it would burst the bottle. They put a little oil on the surface, and tie a piece of bladder on the cork. The bottles are always laid on their sides in sand.

Mr. D. is persuaded an English merchant, or company of merchants, would find their account in establishing a correspondence with one of the principal proprietors in the country, or in sending an agent to reside at Tokay, who might watch the opportunity of the good vintages, choose the best exposures, and bargain with the proprietors themselves. They should have cellars there to keep the wine to a proper age, and an agent at Warsaw, and another at Dantzic, to receive it. This is the road it must take.

There is not, Mr. D. believes, in Europe any country which produces a greater variety of wines than Hungary. They count as many as 100 different sorts. The most valuable white wines, after the Tokay, are, 1. *The St. George wine,* which grows near a village of that name, about 2 German miles north of Presburg, and in the same latitude with Vienna. This wine approaches the nearest of any Hungarian wine to Tokay. Formerly they used to make Auspruch at St. George; but this was prohibited by the court about 16 years ago, it being supposed that it might hurt the traffic of the Tokay wine. 2. *The Edenburg wine,* resembling the St. George, but inferior in quality and value. Edenburg is a town situate about 9 German miles north-west of Presburg. 3. *The Carlowitz wine*, something like that of the Cote rotie on the banks of the Rhone. Carlowitz is the seat of the metropolitan of the Greek church in Hungary. It stands on the banks of the Danube, between 45 and 46 degrees of latitude.

The best red wines are, 1. *The Buda wine,* which grows in the neighbourhood of the ancient capital of the kingdom. This wine is like, and perhaps equal to, Burgundy, and is often sold for it in Germany. A German author of the last century says, that a great quantity of this wine used to be sent to England in the reign of James I., over land by Breslaw and Hamburg, and that it was the favourite wine both at court and all over England. 2. *The Sexard wine,* a strong deep-coloured wine, not unlike the strong wine of Languedoc, which is said to be sold at Bourdeaux for claret. The Sexard wine on the spot costs about

5 creuzers, or $2\frac{1}{2}$d. a bottle. It belongs to the Abbot of Constance, and is chiefly consumed in Germany. Sexard is on the Danube, between Buda and Esseh. 3. *The Erlaw wine,* which is reckoned at Vienna almost equal to that of Buda. Erlaw is in Upper Hungary, south-west of Tokay, between 47 and 48 degrees of latitude. 4. *The Gros Wardein wine,* a strong bodied wine, and very cheap. It belongs chiefly to the Duke of Modena, whose ancestor got a large estate in this country, in grant from the Emperor Leopold, as a reward for his services in the Hungarian wars. Gros Wardein is an old fortress near the confines of Transylvania, between 46 and 47 degrees of latitude.

XXXIII. On the Figure and Composition of the Red Particles of the Blood, commonly called the Red Globules. By Mr. Wm. Hewson, F.R.S. p. 303.

This paper is reprinted in Mr. Hewson's collected works.

XXXIV. On the Effects of a Thunder-storm, March 15th, 1773, on the House of Lord Tylney at Naples. In a Letter from the Hon. Sir Wm. Hamilton, F.R.S. Dated Naples, March 20, 1773. p. 324.

This accident was on his lordship's assembly night; so that most of the nobility of this country, many of the foreign ministers, foreigners of distinction, particularly English, were present at the time of the explosion; there were not less than 250 in the apartments; and including servants, the whole number under Lord Tylney's roof could not be less than 500. The lightning passed through 9 rooms, 7 of which were crouded with parties at cards, or conversing; it was visible in every one, notwithstanding the quantity of candles, and has left in all evident marks of its passage. Many of the company were sensible of a smart stroke, like that of electricity, and some complained for several days after of a pain they felt from that stroke, but no one received any essential hurt; a servant indeed of the French ambassador's house has a black mark on his shoulder and thigh, from a stroke he received on the staircase; and another servant, who was asleep on the same staircase, his head reclining against the wall, had the hair entirely singed from it on that side.

The confusion at the moment was very great: the report, which seems to have been equally heard in every room, was certainly as loud as that of a pistol; and every one flying the room they were in, thinking the danger there, met of course in the door-ways, and stopped all passage. A Polish prince, who was playing at cards, hearing the report, as he thought of a pistol, and feeling himself struck, jumped up, and clapping his hand to his sword, put himself in a posture of defence. Sir Wm. H. was sitting on a card-table, and conversing with M. de Saussure, Professor of Natural History at Geneva; they happened to be looking different ways, and each thought that the bright light and report was immedi-

ately opposite to him; and every one was persuaded that the greatest explosion had been directly before himself. Hearing however a voice saying, un fulmine, un fulmine! they began to examine the gallery in which they were, and soon discovered that the gilding of the cornish had been affected, for in the corners, and at every junction, it was quite blackened; those that had been sitting under the cornishes were covered with the shining particles of the varnish that went over the gilding, and which was thrown off in small dust, at the moment of the explosion. In the apartment above, the same operation had been performed on the gildings; and it is certain that the profusion of gildings, and the bell-wires, prevented the lightning from making more use of the company to conduct it in its course. For further particulars, see Sir Wm. Hamilton's Essays collected.

XXXV. On some Improvements in the Electrical Machine. By Dr. Nooth. p. 333.

It is evident, that the electric matter is excited in the instant that the glass passes over the rubber, and that it becomes sensible to us by its adhering to the revolving surface of the glass. It also appeared highly probable, that the quantity of fire, which we find on the glass in motion, is not the whole of that which is excited by the passage of the glass on the rubber. The luminous appearance in the angles between the glass and rubber, and which is extremely distinct in a dark room, rendered it next to certain, that a part of the excited electric fluid returns immediately to the cushion without performing a revolution with the glass; and that of course a circulation of the fire is thus kept up in the substance of the cushion in the common method of constructing the machines.

To be convinced of this, Dr. N. attempted to make the passage of the fire from the glass to the anterior part of the cushion, or to that part which corresponds with the ascending side of the cylinder, demonstrable, by placing a piece of silk between the glass and cushion. This silk was larger than the cushion; and part of it was allowed to adhere, by the attraction of the electric fire, to the ascending part of the cylinder. His view in doing this was to cut off, in that part, the immediate communication between the excited glass and cushion, and by that means render the circulation of electric matter visible, which he suspected to take place in the machine; as it was thus forced to turn over the loose edge of the silk before it could return to the cushion. The event answered his expectation; and he then perceived, that the greatest part of the excited fluid was commonly re-absorbed by the fore part of the cushion without becoming sensible on the superior part of the glass.

Having thus verified his supposition by actual experiments with silken flaps of different sizes, he endeavoured to discover a method of preventing that circulation of the electric fluid, and if possible, of obliging the whole, or the greater part of it, that is once excited, to make the revolution with the glass. This

indeed the silk, when of considerable breadth, in some measure effected; but he thought that this obstruction to the immediate return of the fire might be rendered more complete by increasing the thickness of the silk, or by applying to it some nonconducting substance, that might confine the excited fluid more perfectly to the surface of the revolving cylinder. Bees-wax being a nonconducting substance easily procured, he rubbed the silken flap with it, and found that the return of the fire to the cushion at the anterior part of the machine was by that means much diminished, and consequently the excitation of the glass was apparently increased. The addition however of more silk was still more effectual, in confining the fire to the glass; and when it was employed 10 or 12 times doubled, it seemed to deny any passage from the glass to the cushion.

As Dr. N. thus discovered the method of remedying the common defect in the construction of the anterior part of the cushion, he next attended to that part which corresponds with the descending side of the cylinder. Being convinced that this part of the rubber was alone concerned in the excitation, he imagined that the reverse of what was necessary anteriorly should be adopted in the structure of the posterior part; that instead of placing nonconducting substances between the glass and cushion, we should here make the afflux of the electric matter as great as possible, by the application of the most perfectly conducting bodies. Confining therefore the amalgam to that place where the glass first comes in contact with the rubber, he placed some tinfoil close to the amalgam, and bending it back, secured it to the metallic plate below the cushion. By this means the electric matter found an easy access to the place of excitation; and the effect of the machine was thereby greatly increased. A piece of leather, covered with amalgam, and fixed to the posterior part of the rubber, in such a manner as to allow about an inch of it to pass under the cylinder, answered every purpose of the tinfoil; and, as it was not liable to be corroded by the mercury, like tinfoil, it was on that account much preferable.

From the above experiments it was apparent that the excitation was altogether performed by the posterior portion of the cushion; and that the anterior part, when made of conducting substances, re-absorbs the greater quantity of the excited matter. In the structure therefore of electrical machines, we should always have a free electric communication behind, to facilitate the excitation; and the most perfectly nonconducting substances before, to prevent the re-absorption. To answer these intentions, it will perhaps be advisable to make the cushion of silk, stuffed with hair, and to lay some metallic conductor round the posterior part, that a free access may be allowed to the electric matter coming to the place of excitation from the inferior part of the machine. Cushions, made in this manner, and then covered with silk 10 or 12 times doubled, are much more powerfully excitant than any others that he had yet tried. Various other methods

however may be pursued in the construction of the rubber; but it should be an invariable rule, to place nonconducting bodies before, and conducting substances behind, the cylinder. From the preceding principles, it follows, that the support to the rubber should likewise have its conducting and nonconducting side. For this purpose, it may be necessary to employ baked wood, and to cover the posterior half with tinfoil. The place of excitation will be thus sufficiently supplied with electric matter, and the cylinder will not be robbed of a part of the excited fire, before that fire has made a revolution with the glass.

By attending to the place where the excitation is effected, it must appear evident, that the amalgam is only to be laid on the posterior part of the cushion; its presence indeed would be useless, if not injurious, in any other situation. It will however be found somewhat difficult to confine the pure amalgam to the posterior part of the rubber; but if it is mixed with a little hair powder and pomatum, it pretty well keeps its place. The strewing the amalgam thus prepared on the glass, as it revolves, is perhaps the best method of applying it; as, by that means, it is in a great measure prevented from passing on to the nonconducting substances that are placed before. Should any of the amalgam be carried forward by the revolution of the glass, it should be carefully removed. The necessity of keeping that part free from conducting bodies cannot be too much insisted on; and when fresh amalgam is applied as before mentioned, to the proper part of the rubber, the flap should be held down during half a dozen turns of the machine, lest it might collect some of the amalgam before it is properly fixed. It is a probable conjecture that, when the flap of silk is covered with amalgam, part of the amalgam, which is not immediately subservient to the excitation, acts as a conductor in restoring the fire again to the cushion; and that thus, by an improper disposition of it, we suppress, instead of increasing, the quantity of the excited matter.

In short, when an electrician attends to the preceding principles in the construction of his rubber, and to the proper disposition of the amalgam, he has nothing to fear from the humidity of the atmosphere, as his machine will work equally well in all kinds of weather. The rest of the electrical apparatus may be made according to the directions that have been given by the different electrical writers. Each has had his favourite machine; and perhaps no one has been yet contrived that has not had its peculiar advantages.

XXXVI. Properties of the Conic Sections; deduced by a Compendious Method. Being a Work of the late Wm. Jones, Esq., F.R.S., which he formerly communicated to Mr. J. Robertson, Libr. R. S., and by him addressed to the Rev. N. Maskelyne, F.R.S., &c. p. 340.

It is well known that the curves formed by the sections of a cone, and there-

fore called conic sections, have, from the earliest ages of geometry, engaged the attention of mathematicians, on account of their extensive utility in the solution of many problems, which were incapable of being constructed by any possible combination of right lines and circles, the magnitudes used in plane geometry. The properties of these curves are become far more interesting within the last 2 centuries, since they have been found to be similar to those described by the motions of the celestial bodies in the solar system.

Two different methods have been taken by the writers who have treated of their properties; the one, and the more ancient, is to deduce them from the properties of the cone itself; the other is to consider the curves, as generated by the constant motion of 2 or more straight lines moving in a given plane, by certain laws. There are various methods of generating these curve lines in plano; one method will give some properties very easily; but others, with much trouble: while, by another mode of description, some properties may be readily derived, which, by the former, were not so easily come at: so that it appears there may be a manner of describing the curves similar to the conic sections, by the motion of lines on a plane, which in general shall produce the most essential properties, with the greatest facility.

That excellent mathematician, the late Wm. Jones, Esq. F.R.S. had drawn up some papers on the description of these curves, or lines of the second kind, very different from what he gave in his Synopsis Palmariorum Matheseos, published in the year 1706; or from that of any other writer on this subject. A copy of these papers he let Mr. R. take about the year 1740, who, though they were in an unfinished state, thought them of too much consequence to be lost; and therefore was desirous of preserving them in the Phil. Trans. in the manner he at first transcribed; though he is aware they might have been put into a form more pleasing to the generality of readers: Mr. R. indeed annexed larger diagrams than what accompanied the author's copy, in order to render the lines more distinct, as all the relations are to be represented in a single figure, of each kind. Mr. R. then proceeds to state that Mr. Jones, having laid down a very simple method of describing these curves, seems to have been desirous of arriving at their properties in as expeditious a way as he could contrive; and therefore he has used the algebraic method, in general, of reducing his equations; and on some occasions has used the method of fluxions, to deduce some properties chiefly relating to the tangents; and by a judicious use of these, he has very much abridged the steps which otherwise he must have taken, to have deduced the very great variety of relations he has obtained: these he intended to have arranged in tables, whence an equation expressing the relation between any 3 or more lines of the conic sections, might be taken out as readily as a logarithm out of their tables; this he has only partly executed; but it may easily be con-

tinued by those who are desirous to have it done, and are sufficiently acquainted with what follows. Mr. Jones first gives the organical description of the lines of the 2d kind, or curves of the 1st kind, in the following manner.

The Description of Lines of the Second Kind.

Let the right lines AD, AQ, be drawn on a plane, at any inclination to each other. See pl. 7, fig. 13, 14, 15. In AD, AQ, take A*a*, AM, of any given magnitude, and draw MN parallel to AD. On the points A, *a*, let two rulers AP, *a*P, revolve, and cut MN, AQ, in N and Q, so that AQ be every where equal to MN. Then shall the intersection P of the rulers describe lines of the 2d kind, or curves of the first kind.

Where the right line A*a*, is the first, or transverse diameter. The point C, bisecting the diameter A*a*, is the centre. The right line PD, drawn parallel to AQ, is the ordinate to the diameter A*a*. The part AD, or CD, of the diameter, is the absciss, when reckoned to begin from A to C, or from C to A. The right line B*b* drawn from the centre C parallel to the ordinate PD, and terminated in the curve, is called the 2d, or conjugate diameter. Those diameters to which the ordinates are perpendicular, are called the axes. And AM is the parameter to the diameter A*a*.

From this description, Mr. Jones then proceeds to deduce the several numerous properties of these lines, in short algebraical expressions or equations: but these, in the present advanced state of the conic sections, may well be spared on this occasion.

XXXVII. An Essay, towards Elucidating the History of the Sea Anemonies. By Abbé Dicquemare, Prof. of Exper. Philos., &c. at Havre de Grace. Translated from the French.* p. 361.

There is great confusion in the descriptions which naturalists have given of these animals, and no less in the names bestowed upon, and the divisions or classes assigned to them. Some have called them sea-nettle, urticæ marinæ, though these animals are not prickly, as some of the wandering nettles are. Other writers have called them sea anemonies. The sea anemonies found on the coast of the Havre seem to constitute 3 different species. Those here put in the first class, because in certain positions they resemble most the flower known by the name of anemone, cling or adhere to rocks and stones, and are often found in the holes that chance to be in them, and seem to like the surface of the water. The outer shape of the body of this animal, when it contracts itself, is much like a truncated cone, pl. 8, fig. 1,† with its basis fixed and strongly clinging to the rock. Its upper part is terminated with a hollow. This cone is often perpendicular to its basis; sometimes it lies in an oblique position to it, or the basis spreads itself irregularly; so that from a round, it alters to an elliptical shape. Sometimes it imitates pretty exactly the inclosing out-leaves of anemo-

* The Sea Anemonies belong to the genus *Actinia*, and are by no means uncommon on most of the European coasts.

† This seems to be the *Actinia Mesembryanthemum* of Ellis.

nies, while the limbs of the animal are not unlike the shag, or inner part of these flowers, fig. 2. At other times it assumes the shape expressed by fig. 3. Indeed these animals alter their forms so often, that it would be difficult, perhaps even impossible, to describe them exactly. One part of their body or limbs swells at times very considerably, at the expence of the rest. The figures and the particular observations will supply what is wanting here. With regard to their colours, they vary amazingly. Every hue of purple, green, brown and violet is to be seen blended together. A great number of them are of one uniform colour; while others are spotted either symmetrically, as in stripes, or in an irregular, but always pleasing manner. Most of them have round their basis a blue or white streak, broader or narrower, which produces a sort of ring. When many of these animals are put together at the bottom of a flattish and wide vessel, the whole appears as a bed of anemonies.

The sea anemonies of the 2d species are pretty nearly shaped out as those of the 1st, but they are much larger. Mr. D. had some, kept in sea water, that were 18 or 20 inches in circumference. Their cloak or outer skin is rough like shagreen, or full of little knobs.* See fig. 10, 11, 12, where they are shown in half the natural size. They remain in the sand, sticking to the loose stones in it, and stretch out their limbs to the top, in order to lay hold of their prey, as soon as it touches the superficies of the sand. The flower of poppies is said to be the plague and distress of painters, to represent exactly the variety and brilliancy of its colours; the same may be said of the sea anemonies of this larger species. The purest white, carmine, and ultramarine, would hardly be bright enough to paint them properly. The limbs of some of them are of a moderate or dim colour, at the same time that the cloak is made up of the brightest colours.

The 3d species seems to deviate a little more from the 2d, than this from the 1st. Its body, not unlike for shape and colour to the stalk of a mushroom, is terminated in its lower part by a basis, which the animal fixes to the stones in the sand, while by lengthening out its body, it affords means to the superior part, where the limbs and the mouth are placed, of spreading out and opening themselves at the surface of the sand. See fig. 4.† This species has some slight variety in point of shape, and still more of colour. Some have their limbs of a bright white, or fine violet colour; others of an ivory white. Some are found of the same sort of yellow with the inside of melons. Some are greenish, or of a fine brown, with the middle white, which gives them a likeness to auriculas.

Others again have their limbs of a greyish tint, somewhat like the inside of a broken piece of silver; or alternately mixed with black and white in the manner of the quills of a porcupine.

* This species is the *Actinia crassicornis*.

† There is no fig. 4 in the original plate.

What first offered itself to Mr. D.'s observations, is what distinguishes these animals from plants, viz. progressive motion, by the help of which they can shift their place; the other determinate motions, by which they are enabled to lay hold of their prey; the means they make use of to defend themselves; their deglutition, digestion, evacuations, and lastly the propagation of their species, &c. What little he has had an opportunity to see of those functions, appears sufficient to place these creatures in the class of spontaneous animals, rather than in the dark indeterminate list of zoophytes.

In May 1772, he clipped all the limbs of a purple anemone of the 1st species. Soon after, these limbs began to bud out again. The 30th of July they were clipped a 2d time, and grew again in less than a month. Having cut them a 3d time, they had a 3d shooting out. The same experiment on a green anemone had the like success. It seems these reproductions might extend as far, or be as often repeated, as patience and curiosity would admit. Several experiments have convinced him that one single limb of these anemonies being cut off, retains a power to fasten itself to any small body that is brought near it, either by its end, or by the side towards the end, but not by that part where the clipping was made. This induces him to think that the effect is produced by suction, rather than by any glutinous matter, which might be supposed to ooze out at the pores. This limb, after being cut off, has also a power to stretch or contract itself alternately.

July the 12th he cut one of these purple anemonies through the body, rather nearer the basis. This part remained adhering to the side of the vessel in which it was; and for several days made various motions. At last it got loose, and then fastened in another place. The 27th it began again to move about, till the end of August, when it became as it were lifeless, very flabby, and had often an offensive smell. He concluded it to be dead; but as it did not lose its shape, he resolved to keep it, and to shift it every day into some fresh sea water. From time to time, he thought it had some sort of motion, and in the beginning of November these motions became more perceptible. It shifted its position, when contrary to its natural state. November the 28th, this stump climbed up to the top of the vessel. He then began to perceive some new limbs growing out. January 13, 14, and 15, 1773, it again moved about; and on the 16th, seeing these growing new limbs, he offered them some bits of muscles, which however were neither eaten, nor even laid hold of. That same day, after several motions in various directions, it loosened its adhesion, and remained motionless and flabby, but without any bad smell, till the beginning of February, when it appeared adhering, but weakly, to the bottom of the vase. The 16th, after several motions, it climbed up to the top, where it remained till the 11th of March, and then loosened its hold. These alternate stations and motions lasted till the

8th of April, without showing any plain and full reproduction. However, the animal continues to grow stronger and thicker. He owns it was very wrong to throw away, a few days after the operation, the upper part that had been cut off. But he did not foresee what would happen.

November the 9th, 1772, he clipped a brown anemone through the body. The basis, together with that part of the stump which was left to it, shrunk up, as in fig. 5, and remained motionless where it was at first, till January 13, when it shifted its place. The 15th, he very distinctly perceived 2 rows of limbs growing out of the part where the section was made, fig. 6, and the animal moved along. The next day he offered it bits of muscles, which it laid hold of and ate. These growing limbs were, at first, of a sullied white; but they became browner and browner every day; and are at present of the same colour with the coat of the animal. They are pretty near as large as they were before the operation; but he had not perceived, as yet, some of those small fine blue knobs, that are to be seen round the rim or upper knurl of the coat, as may be seen in fig. 2. As to the part cut off, seen in fig. 7, which consists of about half the body, and wherein the limbs and mouth are placed, he offered it, after the operation was performed, that brown part of a muscle, by the help of which it moves along, and whence the beard spreads out. This bit, which is not easily digested by sea-anemonies, was directly snapped up by the limbs. They drew it to the mouth, which lengthened itself out to catch it, and swallowed it down. But, as the body was wanting to receive it, the bit came out at the opposite end, just as a man's head, being cut off, would let out at the neck the bit taken in at the mouth. He offered it a 2d time, and the animal swallowed it again; but threw it up at the mouth the next day. He still kept that part of the anemone, which daily grew stronger and stronger, and which appeared to suck in the bits of muscles he offered it. The limbs lay hold of them, and the mouth takes them in, either whole or in part, and throws them up a good deal altered; and pretty often these bits go through as they did the first time. Some persons, who were eye-witnesses of these particulars, were of opinion that, from the remains of organization and habit, this part of the animal still endeavoured to gratify a natural want, though no longer subsisting; but Mr. D. is inclined to think that it still exists. In his opinion the part is nourished by means of suckers, of which he suspects it to be full, both inwardly and outwardly. He was in expectation to have his conjecture confirmed by experience, and by it to be enabled to convince other people. The microscope seems to have already corroborated his notion on this subject. When the limbs of these anemonies, especially those of the 2d species, are touched, the person's fingers are felt to adhere and strongly to stick to the limbs. Mr. D. therefore let both these, and several other species of these animals, fasten several times on the fingers of one of his hands, in order to see

whether any glutinous matter should remain on them; but he never perceived any; and by applying the fingers of that hand to the other, he perceived no adhesion. Mr. D. afterwards, for some days, clipped several anemonies of the first species, diametrically and perpendicularly to the basis. They stood the operation extremely well. Time will teach us what the result of these operations will be.

These animals can live a whole year, and perhaps much longer, without any other food than what they chance to find disseminated in the sea-water. They do not want many motions to procure their food, besides stretching out their limbs, to receive such as comes within their reach; and they remain surrounded with muscles, &c. without laying hold of any of them. He has given anemonies some of these muscles alive, but with their shells closed, and about six lines in length. They were swallowed in that state; and 40, 50, and 60 hours after, the shells were thrown up at the mouth, empty and perfectly cleared, even from the small tendons which connect the fish to its shells. The anemonies swallow and digest small fish, and bits of larger fish, or of raw meat, when offered to them. When they cannot digest some of the food, they throw it up at the mouth, either whole or partly dissolved into a viscous liquor, which may in some measure be considered as their excrements. They void by the same way, and in the same manner, various parts of a white and brown substance, and small bodies are thrown out at the extremity of the limbs, and more sensibly in the 2d species. There also oozes out of their body a sort of excrementitious matter, which by coagulating produces round them a sort of girdle.

These animals are known to be viviparous. Several of them have brought forth, even in Mr. D.'s hand, 8, 10, and 12 young ones. Some, though almost imperceptible, as in fig. 8, have even the power of clinging, and are endowed with 2 rows of limbs, which they open immediately on their birth, in order to catch their prey, which they swallow afterwards. He has kept some for 10 months. They have appeared not to have increased in their bulk more than twice the diameter of their size. Indeed he fed them sparingly, the others grew as large as half a green pea. Fig. 9.

The sea anemonies have a progressive motion; which, though slow, is performed in every direction, with a degree of facility, and effected by means of muscles which cross each other at right angles. Mr. D. cannot as yet display the mechanism of these muscles, because the last mutilations he was obliged to make discompose the conjectures that some learned men have published on this subject; and he had not sufficiently fixed his thoughts in consequence of his observations.

The 2d species of sea-anemonies keeps itself hid more than the 1st, it is not to be come at but in neap tides, when the sea recedes farthest, and cannot be

so easily observed. With great difficulty are these anemonies loosened from the stones they adhere to; part of the basis is often left behind, and they are not easily preserved at home. Seeing however some of the individuals that voided muscle shells whole, and still joined together, but empty, he found out the way to feed them. Crab shells, about the size of a hen's egg, are likewise discharged whole; and on offering them some live ones, he found that they swallowed them down, and voided the remains sucked dry in about 20 hours. He cut open some of this 2d species, which he could not loosen from the stones, and among them he found one that had swallowed an anemone of the 3d species; but this had received no harm. For, having put it into some sea-water, it opened and spread as usual. He offered them several of the same species, which they swallowed down; but threw up again alive within 8, 10, or 12 hours, or even later. Is then a live anemone an undigestible body for another?

On the 21st of June, 1772, having caught the instant that an individual of the 3d species was stretching out, as expressed in fig. 13, he snipped off at once with sharp scissars the whole upper part where the limbs and mouth are placed. It was with great satisfaction that, 8 days after, he perceived new limbs growing out, as in fig. 15. The 3d of July, the animal began to eat some bits of muscles; and towards the middle of the month, the upper part was so completely formed, that it might easily have been mistaken for one of its unclipped neighbours, had there been many in the glass. It is neither the row of the centrical or inner limbs, nor the most outward, which first bud out, but the intermediate ones. The part which had been clipped off gave signs of sensibility to the 17th of July, contracting and dilating itself, in the same manner as a whole anemone; but it was much smaller than before the operation. This experiment has been repeated by clipping, on the 11th of July, the whole upper part and one-third of the body of another anemone. New limbs began to shoot out the 21st. There were 2 rows of them on the 25th; and on the 3d of August, 4 very distinct and well shaped, which caught and kept fast the food that was offered the animal. The mouth itself was sufficiently well formed to take in several times bits of muscles. On the 11th, he perceived in the limbs the faint alternate marks of ivory white and black, and soon after, there scarcely appeared any sign of the operation having been performed.

Being induced to try further experiments, on the 7th of August he clipped an anemone across the body. Like the others, it moved or wriggled a little at first: but he did not perceive any new limbs growing till towards the end of the month. During that time, it continued in such a state as gave but little hopes of its doing well again. Two rows of limbs appeared at last, and the insect recovered its strength. There was on the 9th of September a 3d row of limbs, and the mouth appeared to be shaped out and formed; yet the anemone neither

ate nor kept the bits of muscles he gave it. A 4th row was growing out on the 19th, which gathered strength by degrees; so that on the 3d of October it began to eat, and in a short time became a complete animal. On September the 22d, the upper part appeared to be withering away. But he soon saw how much he had reason to think he was mistaken.

He cut another anemone, of the same species, across the middle of the body, in such a manner as that the 2 parts were only left hanging together by one-fourth part of the diameter. His design was, to try whether nature would produce limbs on the edge of the lower half-part, in the same manner as when the body is cut quite asunder; or whether the wound, though very deep, would heal up again. Nature was not wanting to itself; for, notwithstanding the largeness of the incision, the 2 severed parts were joined up together, and in a few days the wound was healed up. The animal did not even seem to have suffered so much as one might have expected.

Mr. D. witnessed a fact remarkable enough to be inserted here. Having sliced a bit of fish, he offered one end of it to an anemone, whose limbs are of the melon-yellow colour, and the other end to a grey anemone, whose superior part had been reproduced after it had been cut off. The 2 insects, which were adhering pretty near each other at the bottom of the glass, directly seized their prey with their arms; but the yellow one happened to lay hold of the larger share of the slice. Each swallowed on, by the respective ends, till at last each other's mouth came within contact. The grey one seemed at first to get the better; but the other soon recovered her share, then lost it again, and again recovered it. These alternate victories lasted about 3 hours: and there was a time, during which the yellow anemone was nearly worsted; till at last, the grey one losing hold of her end of the slice, the other carried off the prize. Yet, as she sucked in but slowly, the grey one ventured with her mouth on a last tug at her end, still in sight, which she had slipped; but this fresh effort proved fruitless; the yellow champion gave a last pull, and swallowed down the whole. During this whole strife, the two parties did not seem to be animated by any other passion than that of snatching the slice of fish from each other; and though the 2 animals continued afterwards to remain neighbours, they lived very quiet and peaceably together.

These animals are sometimes very voracious. Could it be believed that the same creature that can continue in pretty good plight for a whole year, and perhaps longer, without taking in any other food, besides what may be disseminated in the sea-water, and does not seem very active to lay hold of its prey, but rather waits patiently till chance throws it within reach of its limbs; that this animal should still be so greedy as to gulp down 2 whole muscles, which he gave to one of them by piece-meal, and burst the next day with indigestion, when it

has a power of throwing up so easily what it has swallowed down? This was the case with an anemone of the 3d species, and of a middle size, which had been fished lately.

On the 8th of October, the same sea anemonies (of which he had clipped out the upper part with the mouth and limbs, instead of which new ones were reproduced, perfect enough to enable them to eat) were divided a 2d time, and again renewed, so as not to be distinguished from those which had undergone no operation. And having taken particular care to feed one of these halves clipped the 2d time, he saw it grow stronger and stronger every day, and perform with equal facility the same functions as any other complete original anemone. The only difference or exception is, that its basis is not yet perfect enough, to enable the animal to adhere or fix itself to the glass; he makes no doubt however, but this new anemone will, in a short time, acquire the only powers yet wanting, to render it a perfect one, see fig. 16 and 17. Might not we ask, on all these facts, what is become of the original animal? is it that which continued adhering by its basis to the glass? or is it the upper half? Are there animals among which an individual is not a simple being?

P. S. Just as this essay was concluded, Mr. D. found out a 4th species of sea anemonies, of the size of the 2d, and of an elegant form, having the appearance of a cluster of white or flesh-coloured feathers.* These anemonies are found in oyster beds, &c. He observed that there grows, or comes out of their body and mouth, a sort of threads about the size of a horse-hair, which being examined with a solar microscope of 5 inches diameter, appear as if made up of a prodigious number of vessels, in which a liquor is seen to circulate. The largest of these unite together, much in the same manner as the optic nerves do in man. Such an organization is doubtless intended for very important purposes. Some young ones of this species, which still adhered to each other by a string of communication, shut themselves up in the same instant, when this string was touched in the middle. As he could not directly contrive a total section of this large species, he tried it on the young ones; and these shooted out again after the operation; and so have the old ones done since. Mr. D. has met with a sort of monster among these anemonies, viz. one which seemed to inclose or contain 3 others, 2 of which were united at their basis, and the 3d lay, as it were, concealed in the folds.

Nature has resources little known to us; it seems some times to vary its operations, with an intent, as it were, the more to stimulate our curiosity, and perhaps to disclose her secrets to those who are endowed with a degree of sagacity or patience, sufficient to follow and investigate the effects offered to our ob-

* This species is the *Actinia plumosa.*

servations. Among the great number of anemonies of the 3d species, which Mr. D. had clipped across the body, there happened to be 2, whose lower part has in the usual way shot forth new limbs; but the upper half, where the limbs and mouth were, instead of healing up into a new basis, has produced both another mouth and limbs. Hence an animal was formed, which caught its prey, and fed by both ends at the same time.

The sea anemonies of the first 3 species mentioned before, and perhaps those likewise of the 4th, feed on those floating transparent animals of a white glassy, or of a blue or purplish hue, called wandering nettles, or sea jellies. An anemone of a middle size, of the 1st and 3d species, such as that represented by fig. 1 and 13, swallows one of these animals of the size of half an orange. All these 4 species are good to eat.

Particular Explanation of some of the Figures.

Fig. 14 shows the anemone of the 3d species, when shrunk up. One sees round it a ring of sand and broken pieces of shells sticking together by means of the excrementitious humour habitually oozing out of the body of the animal, or out of the little granulated knobs, with which it is covered towards the upper part. This ring is also to be seen in the same anemone, when lengthened out, as expressed in fig. 13.

Fig. 10* shows a sea anemone, of the 2d species, concealed under the sand, and covered over in different places with broken shells and gravel, with which the animal forms a coat of mail to secure itself under, but out of which it can slip in an instant. The figure shows it when it spurts out water at its mouth, and at the end of its limbs.

Fig. 11* shows the same anemone open. The mouth is in the centre of the upper part; it is not always shaped in the same manner in other anemonies as it is seen here, or at least does not always appear to be so. Fig. A shows amouth as engraved for another anemone, but which alters or shifts its form every moment. This anemone has 5 rows of limbs. There are 10 in the innermost row: the like number in the 2d: 20 in the 3d: 30 in the 4th: and 80 in the 5th. When the animal is out of the water, and is squeezed, it spurts out water at the mouth and at several of its limbs at the same time; so that it imitates pretty well the play of water-works. When the limbs are drawn in closer together, they give it the look of a flower, especially of an anemone.

Fig. 12 shows an anemone of the same species, turned inside out, as when a purse or stocking is so. A thin transparent membrane, with white stripes, lines the whole inside of the animal; and through it are seen the bowels, part of which hang or come out at the middle. One may observe besides, in this figure, 2 hollows sinking in, which are formed by 2 pretty strong cartilages.

N. B. Dr. Solander being consulted about these sea worms, which are evidently of the class of the actinia, referred the first species, fig. 1—3, to the *Actinia equina*, Linn. Syst. Nat. 1088, 1; the 2d species, fig. 10, 11, and 12, to the *Actinia senilis*, ib. 1088, 2; and the 3d, fig. 13 and 14, to the *Actinia felina*, ib. 1088, 3.

XXXVIII. Of a New Hygrometer. By M. J. A. De Luc, Citizen of Geneva, F. R. S. p. 404.

In order to proceed regularly in this investigation, Mr. D. began by examining the essential requisites in a machine intended to measure humidity, which he

** Fig. 10, 11, 12, represent the *Actinia crassicornis* in its different states.

found to be the 3 following: 1st. The settling of a fixed point, from which every measure of the same kind should be taken; such, for instance, as that of boiling water in a thermometer, when the barometer is at a certain height. 2d. Degrees equally determined, or comparable, in different hygrometers, such as are in the thermometer, the scales of Fahrenheit, Delisle, Reaumur, &c. 3d. Constancy in the variations produced by the same differences of humidity.

The result of Mr. D.'s elaborate researches on this subject, was, the adopting for an hygrometer a small tube of ivory, inclosing a portion of quicksilver, which was made to rise and fall in the tube, like a thermometer, by the contracting or widening of the tube, in consequence of more or less moisture in the surrounding medium.

XXXIX. On the Electric Property of the Torpedo.. In a Letter from John Walsh, Esq.,† F. R. S., to B. Franklin, Esq., LL. D., F. R. S., &c.* p. 461.

Letter from Mr. Walsh to Dr. Franklin, dated la Rochelle, 12th July, 1772.—" It is with particular satisfaction I make to you my first communication, that the effect of the torpedo appears to be absolutely electrical; by forming its circuit through the same conductors with electricity, for instance, metals and water; and by being intercepted by the same non-conductors, for instance, glass and sealing-wax. I will not at present trouble you with the detail of our experiments, especially as we are daily advancing in them; but only observe, that we have discovered the back and breast of the animal to be in different states of electricity; I mean in particular the upper and lower surfaces of those 2 assemblages of pliant cylinders, of which you have seen engravings in Lorenzini.‡ By the knowledge of this circumstance we have been able to direct his shocks, though they were very small, through a circuit of 4 persons, all feeling them; likewise through a considerable length of wire held by 2 insulated persons, one touching his lower surface, and the other his upper. When the wire was exchanged for glass, or sealing-wax, no effect could be obtained: but as soon as it was resumed, the 2 persons became liable to the shock. These experiments have been varied many ways, and repeated times without number, and they all determined the choice of conductors to be the same in the torpedo as in the Leyden phial. The sensations likewise, occasioned by the one and the other in the human frame, are precisely similar. Not only the shock, but the numbing sensation which the animal sometimes dispenses, expressed in French by the words engourdissement and fourmillement, may be exactly imitated with the

* *Raja Torpedo*, Linn.

† For this ingenious paper the author was presented with the Copleian medal.

‡ Osservazioni intorno alle torpedini di Stef. Lorenzini 1678. Redi appears to be the first who remarked these singular parts of the torpedo in 1666. Franc. Redi, Exper. Nat.—Orig.

phial, by means of Lane's electrometer; the regulating rod of which, to produce the latter effect, must be brought almost into contact with the prime conductor which joins the phial. We have not yet perceived any spark to accompany the shock, nor the pith balls to be ever affected. Indeed all our trials have been on very feeble subjects, whose shock was seldom sensible beyond the touching finger: I remember but one, of at least 200, that I myself must have received, to have extended above the elbow. Perhaps the Isle of Ré, which we are about to visit, may furnish us with torpedos fresher taken and of more vigour, by which a further insight into these matters may be had. Our experiments have been chiefly in the air, where the animal was more open to our examination than in water. It is a singularity that the torpedo, when insulated, should be able to give us, insulated likewise, 40 or 50 successive shocks, from nearly the same part; and these with little, if any diminution in their force: indeed they were all very minute. Each effort in the animal to give the shock is constantly accompanied with a depression of his eyes, by which even his attempts to give it to non-conductors can be observed. The animal, with respect to the rest of his body, is in a great degree motionless, but not wholly so. You will please to acquaint Dr. Bancroft, of our having thus verified his suspicion concerning the torpedo,* and make any other communication of this matter you may judge proper. Here I shall be glad to excite, as far as I am able, both electricians and naturalists, to push their inquiries concerning this extraordinary animal, while the summer affords them the opportunity."

Extracts of a Letter from Mr. Walsh to Dr. Franklin, dated Paris, 27th Aug., 1772.

—— "I spent a complete week in my experiments at the Isle of Ré, and had there every convenience for prosecuting them to their extent, except that I was restrained by the jealousy of the government from making them where the animal was caught. At my return to La Rochelle, I communicated to the members of the academy of that place, and to many of the principal inhabitants, all that I had observed concerning the torpedo, in the intention of stirring up a spirit of inquiry, both as to its electricity and general economy."

—— "The vigour of the fresh taken torpedos at the Isle of Ré, was not able to force the torpedinal fluid across the minutest tract of air; not from one link of a small chain, suspended freely, to another; not through an almost invisible separation, made by the edge of a penknife, in a slip of tin foil pasted on sealing wax. The spark therefore, of course the attendant snapping noise, was denied to all our attempts to discover it, not only in day light, but in

* Bancroft's Natural History of Guiana. p. 194.—Orig.

complete darkness. I observed to you, in my last, the singularity of the torpedo being able, when insulated, to give to an insulated person a great number of successive shocks: in this situation I have taken no less than 50 from him in the space of a minute and a half. All our experiments confirmed, that this electricity was condensed, in the instant of its explosion, by a sudden energy of the animal; and as there was no gradual accumulation, nor retention of it, as in the case of charged glass, it is not at all surprizing that no signs of attraction or repulsion were perceived in the pith balls. In short, the effect of the torpedo appears to arise from a compressed elastic fluid, restoring itself to its equilibrium in the same way, and by the same mediums, as the elastic fluid compressed in charged glass. The skin of the animal, bad conductor as it is, seems to be a better conductor of his electricity, than the thinnest plate of elastic air. Notwithstanding the weak spring of the torpedinal electricity, I was able, in the public exhibitions of my experiment at La Rochelle, to convey it through a circuit, formed from one surface of the animal to the other, by 2 long brass wires, and 4 persons, which number, at times, was increased even to 8. The several persons were made to communicate with each other, and the 2 outermost with the wires, by means of water contained in basins, properly disposed between them for the purpose; each person dipping his hands in the nearest basins, connective with his neighbour on either side.

"The effect produced by the torpedo, when in air, appeared, on many repeated experiments, to be about 4 times as strong as when in water."

A clear and succinct narrative of what passed at one of the public exhibitions, alluded to in the last letter, appeared in the French gazette of the 30th Oct., 1772. As it came from a very respectable quarter, not less so from the private character of the gentleman, than from the public offices he held, I must desire leave of the society to avail myself of such a testimony to the facts I have advanced, by giving a translation of that narrative.

Extract of a Letter from the Sieur Seignette, Mayor of La Rochelle, and second perpetual Secretary of the Academy of that City, to the publisher of the French Gazette.

"In the gazette of the 14th Aug., you mentioned the discovery made by Mr. Walsh, member of the parliament of England, and of the R. S. of London. The experiment of which I am going to give you an account, was made in the presence of the academy of this city. A live torpedo was placed on a table. Round another table stood 5 persons insulated. Two brass wires, each 13 feet long, were suspended to the ceiling by silken strings. One of these wires rested by one end on the wet napkin on which the fish lay; the other end was immersed in a basin full of water placed on the 2d table, on which stood 4 other basins likewise full of water. The first person put a finger of one hand in the basin in

which the wire was immersed, and a finger of the other hand in the 2d basin. The 2d person put a finger of one hand in this last basin, and a finger of the other hand in the 3d; and so on successively, till the 5 persons communicated with one another by the water in the basins. In the last basin one end of the second wire was immersed; and with the other end Mr. Walsh touched the back of the torpedo, when the 5 persons felt a commotion which differed in nothing from that of the Leyden experiment, except in the degree of force. Mr. Walsh, who was not in the circle of conduction, received no shock. This experiment was repeated several times, even with 8 persons; and always with the same success. The action of the torpedo is communicated by the same mediums as that of the electric fluid. The bodies which intercept the action of the one, intercept likewise the action of the other. The effects produced by the torpedo resemble in every respect a weak electricity."

This exhibition of the electric powers of the torpedo, before the academy of La Rochelle, was at a meeting, held for the purpose in my apartments, on the 22d July, 1772, and stands registered in the journals of the academy.

The effect of the animal was, in these experiments, transmitted through as great an extent and variety of conductors as almost at any time we had been able to obtain it, and the experiments included nearly all the points, in which its analogy with the effect of the Leyden Phial had been observed. These points were stated to the gentlemen present, as were the circumstances in which the 2 effects appeared to vary. It was likewise represented to them, that our experiments had been almost wholly with the animal in air: that its action in water was a capital desideratum: that indeed all as yet done was little more than opening the door to inquiry: that much remained to be examined by the electrician as well as by the anatomist: that as artificial electricity had thrown light on the natural operation of the torpedo, this might in return, if well considered, throw light on artificial electricity, particularly in those respects in which they now seemed to differ: that for me, I was about to take leave of the animal, as nature had denied it to the British seas; and that the prosecution of these researches rested in a particular manner with them whose shores abounded with it.

The torpedo, on this occasion, dispensed only the distinct, instantaneous stroke, so well known by the name of the electric shock. That protracted but lighter sensation, that torpor or numbness which he at times induces, and from which he takes his name, was not then experienced from the animal; but it was imitated with artificial electricity, and shown to be producible by a quick consecution of minute shocks. This, in the torpedo, may perhaps be effected by the successive discharge of his numerous cylinders, in the nature of a running fire of musketry: the strong single shock may be his general volley. In the continued effect, as well as the instantaneous, his eyes, usually prominent, are withdrawn into their sockets. The same experiments, performed with the same

torpedos, were on the 2 succeeding days repeated before numerous companies of the principal inhabitants of La Rochelle. Besides the pleasure of gratifying the curiosity of such as entertained any on the subject, and the desire I had to excite a prosecution of the inquiry, I certainly wished to give all possible notoriety to facts, which might otherwise be deemed improbable, perhaps by some of the first rank in science. Great authorities had given a sanction to other solutions of the phenomena of the torpedo; and even the electrician might not readily listen to assertions, which seemed, in some respects, to combat the general principles of electricity. I had reason to make such conclusions from different conversations I had held on the subject with eminent persons, both at London and Paris. It is but justice to say, that of all in that class you gave me the greatest encouragement to look for success in this research, and even assisted me in forming hypotheses, how the torpedo, supposed to be endued with electric properties, might use them in so conducting an element as water.

After generally recommending to others an examination of the electric powers of these animals when acting in water, I determined, before I took my final leave of them, to make some further experiments myself with that particular view; since, notwithstanding the familiarity in which we may be said to have lived with them for near a month, we had never detected them in the immediate exercise of their electric faculties against other fish, confined with them in the same water, either in the circumstance of attacking their prey, or defending themselves from annoyance: and yet that they possessed such a power, and exercised it in a state of liberty, could not be doubted.

A large torpedo, very liberal of his shocks, being held with both hands by his electric organs above and below, was briskly plunged into water to the depth of a foot, and instantly raised an equal height into air; and was thus continually plunged and raised, as quick as possible, for the space of a minute. In the instant his lower surface touched the water in his descent, he always gave a violent shock, and another still more violent in the instant of quitting the water in his ascent; both which shocks, but particularly the last, were accompanied with a writhing in his body, as if meant to force an escape: besides these 2 shocks from the surface of the water, which may yet be considered as delivered in the air, he constantly gave at least 2, when wholly in the air, and constantly 1, and sometimes 2, when wholly in the water. The shock in water appeared, as far as sensation could decide, not to have near a 4th of the force of those at the surface of the water, nor much more than a 4th of those entirely in air. The shocks received in a certain time were not, on this occasion, counted by a watch, as they had been on a former, when 50 were delivered, in a minute and a half, by the animal in an insulated and an unagitated state: but from the quickness with which the immersions were made, it may be presumed there were full 20 of

these in a minute; whence the number of shocks, in that time, must have amounted to above a hundred. This experiment, therefore, while it discovered the comparative force between a shock in water and one in air, and between a shock delivered with greater exertion on the part of the animal and one with less, seemed to determine, that the charge of his organs with electricity was effected in an instant, as well as the discharge.

The torpedo was then put into a flat basket, open at top, but secured by a net with wide meshes, and, in this confinement, was let down into the water, about a foot below the surface; being there touched, through the meshes, with only a single finger, on one of his electric organs, while the other hand was held at a distance in the water, he gave shocks which were distinctly felt in both hands. The circuit for the passage of the effect being contracted to the finger and thumb of one hand, applied above and below to a single organ, produced a shock, to our sensation, of twice the force of that in the larger circuit by the arms.

The torpedo, still confined in the basket, being raised to within 3 inches of the surface of the water, was there touched with a short iron bolt, which was held, half above and half in the water, by one hand, while the other hand was dipped as before, at a distance in the water; strong shocks, felt in both hands, were thus obtained through the iron. A wet hempen cord being fastened to the iron bolt, was held in the hand above water, while the bolt touched the torpedo; and shocks were obtained through both those substances. A less powerful torpedo, suspended in a small net, being frequently dipped into water and raised again, gave, from the surface of the water, slight shocks through the net to the person holding it.

These experiments in water manifested, that bodies, immersed in that element, might be affected by immediate contact with the torpedo; that the shorter the circuit in which the electricity moved, the greater would be the effect; and that the shock was communicable, from the animal in water, to persons in air, through some substances. How far harpoons and nets, consisting of wood and hemp, could in like circumstances, as it has been frequently asserted, convey the effect, was not so particularly tried as to enable us to confirm it. I mention the omission in the hope that some one may be induced to determine the point by express trial.

We convinced ourselves, on former occasions, that the accurate Kaempfer,* who so well describes the effect of the torpedo, and happily compares it with lightning was deceived in the circumstance, that it could be avoided by holding in the breath, which we found no more to prevent the shock of the torpedo,

* Kæmpf. Amœn. Exot. 1712, p. 514.—Orig.

when he was disposed to give it, than it would prevent the shock of the Leyden Phial.

Several persons, forming as many distinct circuits, can be affected by one stroke of the animal, as well as when joined in a single circuit. For instance, 4 persons, touching separately his upper and lower surfaces, were all affected; 2 persons likewise, after the electricity had passed through a wire into a basin of water, transmitted it from thence, in 2 distinct channels, as their sensation convinced them, into another basin of water, whence it was conducted, probably in a united state, by a single wire. How much further the effect might be thus divided and subdivided into different channels, was not determined; but it was found to be proportionably weakened by multiplying these circuits, as it had been by extending the single circuit.

Something may be expected to be said of the parts of the animal immediately concerned in producing the electrical effect. The engraving, which accompanies this letter, while it shows the general figure of the torpedo, gives an internal view of his electric organs. The society will, besides, have a full anatomical description of these parts from the ingenious Mr. John Hunter, in a paper he has expressly written on the subject at my request. It would therefore be superfluous for me to say any thing either in regard to their situation or structure.

I have to observe however, that in these double organs resides the electricity of the torpedo; the rest of his body appearing to be no otherwise concerned in his electrical effect, than as conducting it: that they are subject to the will of the animal; but whether, like other double parts so controlable, they are exercised, at times, singly as well as in concert, is difficult to be ascertained by experiment: that their upper and under surfaces are capable, from a state of equilibrium with respect to electricity, of being instantly thrown, by a mere energy, into an opposition of a plus and minus state, like that of the charged phial: that when they are thus charged, the upper surfaces of the two are in the same state of electricity; as are the under surfaces of the two, though in a contrary to that of the upper; for no shock can be obtained by an insulated person touching both organs above, or both below: and that the production of the effects depends solely on an intercourse being made between the opposite surfaces of the organs, whether taken singly or jointly.

All the parts bordering on the organs act, more or less, as conductors, either through their substance or by their superficies. While an insulated person, placing 2 fingers on the same surface of one or both organs, cannot be affected; if he removes one of his fingers to any such contiguous part, he will be liable to a shock: but this shock will not be near, perhaps not half, so violent, as one taken immediately between the opposite surfaces of the organ; which shows the

conduction to be very imperfect. The parts which conduct the best, are the 2 great lateral fins bounding the organs outwardly, and the space lying between the 2 organs inwardly. All below the double transverse cartilages scarcely conduct at all, unless when the fish is just taken out of water and is still wet, the mucus, with which he is lubricated, showing itself, as it dries, to be of an insulating nature.

The organs themselves, when uncharged, appeared to be, not interiorly we might suppose, but rather exteriorly, conductors of a shock. An insulated person touching 2 torpedos, lying near one another on a damp table, with fingers placed, one on the organ of one fish, and another on the organ of the other, was sensible of shocks, sometimes delivered by one fish, and sometimes by the other, as might be discovered by the respective winking of their eyes. That the organs uncharged, served some way or other as conductors, was confirmed with artificial electricity, in passing shocks by them; and in taking sparks from them, when electrified. The electric effect was never perceived by us to be attended with any motion or alteration in the organs themselves, but was frequently accompanied with a little transient agitation along the cartilages which surround both organs: this is not discernible in the plump and turgid state of the animal, while he is fresh and vigorous; but as his force decays, from the relaxation of his muscles, his cartilages appear through the skin, and then the slight action along them is discovered.

May we not from all these premises conclude, that the effect of the torpedo proceeds from a modification of the electric fluid? The torpedo resembles the charged phial in that characteristic point of a reciprocation between its 2 surfaces. Their effects are transmitted by the same mediums; than which there is not perhaps a surer criterion to determine the identity of subtile matter: they besides occasion the same impression on our nerves. Like effects have like causes. But it may be objected, that the effects of the torpedo, and of the charged phial, are not similar in all their circumstances; that the charged phial occasions attractive or repulsive dispositions in neighbouring bodies; and that its discharge is obtained through a portion of air, and is accompanied with light and sound; nothing of which occurs with respect to the torpedo. The inaction of the electricity of the animal in these particulars, while its elastic force is so great as to transmit the effect through an extensive circuit, and in its course to communicate a shock, may be a new phenomenon, but is no ways repugnant to the laws of electricity; for here too, the operations of the animal may be imitated by art.

The same quantity of electric matter, according as it is used in a dense or rare state, will produce the different consequences. For example, a small phial, whose coated surface measures only 6 square inches, will, on being highly

charged, contain a dense electricity capable of forcing a passage through an inch of air, and afford the phenomena of light, sound, attraction, and repulsion. But if the quantity condensed in this phial, be made rare by communicating it to 2 large connected jars, whose coated surfaces shall form together an area 400 times larger than that of the phial (I instance these jars because they are such as I use); it will, thus dilated, yield all the negative phenomena, if I may so call them, of the torpedo; it will not now pass the 100th part of that inch of air, which in its condensed state it sprung through with ease; it will now refuse the minute intersection in the strip of tin foil; the spark and its attendant sound, even the attraction or repulsion of light bodies, will now be wanting; nor will a point, brought however near, if not in contact, be able to draw off the charge: and yet, with this diminished elasticity, the electric matter will, to effect its equilibrium, instantly run through a considerable circuit of different conductors perfectly continuous, and make us sensible of an impulse in its passage.

Let me here remark, that the sagacity of Mr. Cavendish in devising, and his address in executing, electrical experiments, led him the first to experience with artificial electricity, that a shock could be received from a charge which was unable to force a passage through the least space of air. But, after the discovery, that a large area of rare electricity would imitate the effect of the torpedo, it may be inquired, where is this large area to be found in the animal? We here approach to that veil of nature, which man cannot remove. This however we know, that from infinite division of parts infinite surface may arise, and even our gross optics tell us, that those singular organs, so often mentioned, consist, like our electric batteries, of many vessels, call them cylinders or hexagonal prisms, whose superficies taken together furnish a considerable area.

I rejoice in addressing these communications to you. He, who predicted and showed that electricity wings the formidable bolt of the atmosphere, will hear with attention, that in the deep it speeds a humbler bolt, silent, and invisible: He, who analyzed the electrified phial, will hear with pleasure that its laws prevail in animate phials: He, who by reason became an electrician, will hear with reverence of an instinctive electrician, gifted in his birth with a wonderful apparatus, and with the skill to use it.

Explanation of the Plate of the Male and Female Torpedo, or Electric Ray.

Pl. 9, fig. 1, is a view of the under surface of the female.—a, An exposure, on flaying off the skin, of the right electric organ, which consists of white pliant columns, in a close, and for the most part hexagonal arrangement, giving the general appearance of a honey comb in miniature. These columns have been sometimes denominated cylinders; but, having no interstices, they are all angular, and chiefly 6 cornered. —b, The skin which covered the organ, showing on its inner side, a hexagonal net work.—c, The nostrils in the form of a crescent.—d, The mouth in a crescent contrary to that of the nostrils, furnished with several rows of very small hooked teeth.—e, The branchial apertures, 5 on each side.—f, The place of the heart.—g, g, g, The place of the 2

anterior transverse cartilages, which, passing one above and the other below the spine, support the diaphragm, and uniting towards their extremities, form on either side a kind of clavicle and scapula. h, h, The outward margin of the great lateral fin.—i, i, Its inner margin, confining with the electric organ.—k, The articulation of the great lateral fin with the scapula.—l, The abdomen.—m, m, m, The place of the posterior transverse cartilage, which is single, united with the spine, and supports on each side the smaller lateral fins.—n, n, n, n, The 2 smaller lateral fins.—o, The anus.—p, The fin of the tail.

Fig. 2, A view of the upper surface of the female.—a, a, An exposure of the upper part of the right electric organ,—b, The skin which covered the organ,—c, The eyes, prominent and looking horizontally outwards, but capable of being occasionally withdrawn into their sockets.—d, Two circular apertures communicating with the mouth, and furnished each with a membrane, which in air, as well as in water, plays regularly backwards and forwards across the aperture in the office of inspiration.—e, The place of the right branchia.—f, The two fins of the back.—g, g, The place of the anterior transverse cartilages.

Fig. 3, A view of the under surface of the male, whose size, as here represented, is in general smaller than that of the female.—a, a, Two appendices, distinguishing the male species.

XL. *Anatomical Observrtions on the Torpedo. By John Hunter, F. R. S.** p. 481.

I was desired some time since, by Mr. Walsh, whose experiments at La Rochelle had determined the effect of the torpedo to be electrical, to dissect and examine the peculiar organs by which that animal produces so extraordinary an effect. This I have done in several subjects furnished to me by that gentleman. I am now desired by him to lay before the society, the observations I have made; and for the better understanding of them, to present, on his part, a male and female torpedo in spirits; in the latter of which the electric organs are exposed in different views and sections; likewise a copper plate, which he took care to have engraved, exhibiting those organs.

Of the general structure and anatomy of the torpedo I say nothing, since the animal does not differ very materially, excepting in its electric organs, as they have been properly named by Mr. Walsh, from the rest of the rays, of which family it is well known to be. I will only premise, that the torpedo, of which I treat, is about 18 inches long, 12 broad, and, in its central or thickest part, 2 inches thick; which is nearly the size of the female specimen, now presented to the society, as well as of that from which the plate was taken: but where there is any difference in the organ arising from difference in size, notice will be taken of it in this account.

The electric organs of the torpedo are placed on each side of the cranium and gills, reaching from thence to the semicircular cartilages of each great fin, and extending longitudinally from the anterior extremity of the animal to the transverse cartilage, which divides the thorax from the abdomen; and within these

* Though this paper has been reprinted in Mr. J. H.'s Observations on the Animal Economy; yet as being so immediately connected with Mr. Walsh's Memoir, it was thought proper to retain it in these Abridgments.

limits they occupy the whole space between the skin of the upper and of the under surfaces: they are thickest at the edges near the centre of the fish, and become gradually thinner towards the extremities. Each electric organ, at its inner longitudinal edge, is unequally hollowed; being exactly fitted to the irregular projections of the cranium and gills. The outer longitudinal edge is a convex elliptic curve. The anterior extremity of each organ, makes the section of a small circle; and the posterior extremity makes nearly a right angle with the inner edge. Each organ is attached to the surrounding parts by a close cellular membrane, and also by short and strong tendinous fibres, which pass directly across, from its outer edge, to the semicircular cartilages.

They are covered, above and below, by the common skin of the animal; under which there is a thin fascia spread over the whole organ. This is composed of fibres, which run longitudinally, or in the direction of the body of the animal: these fibres appear to be perforated in innumerable places; which gives the fascia the appearance of being fasciculated; its edges all around, are closely connected to the skin, and at last appear to be lost, or to degenerate into the common cellular membrane of the skin. Immediately under this, is another membrane, exactly of the same kind, the fibres of which in some measure decussate those of the former, passing from the middle line of the body outwards and backwards. The inner edge of this is lost with the first described; the anterior, outer, and posterior edges, are partly attached to the semicircular cartilages, and partly lost in the common cellular membrane.

This inner fascia appears to be continued into the electric organ, by so many processes, and thereby makes the membranous sides or sheaths of the columns which are presently to be described; and between these processes the fascia covers the end of each column, making the outermost or first partition. Each organ, of the fish under consideration, is about 5 inches in length, and at the anterior end 3 in breadth, though it is but little more than half as broad at the posterior extremity. Each consists wholly of perpendicular columns, reaching from the upper to the under surface of the body, and varying in their lengths, according to the thickness of the parts of the body where they are placed; the longest column being about an inch and a half, the shortest about $\frac{1}{4}$ of an inch in length, and their diameters about $\frac{2}{10}$ of an inch.

The figures of the columns are very irregular, varying according to situation and other circumstances. The greatest number of them are either irregular hexagons, or irregular pentagons; but from the irregularity of some of them, it happens that a pretty regular quadrangular column is sometimes formed. Those of the exterior row are either quadrangular or hexagonal; having one side external, 2 lateral, and either 1 or 2 internal. In the 2d row they are mostly pentagons. Their coats are very thin, and seem transparent, closely connected with each other, having a kind of loose network of tendinous fibres, passing

transversely and obliquely, between the columns, and uniting them more firmly together. These are mostly observable where the large trunks of the nerves pass. The columns are also attached by strong inelastic fibres, passing directly from the one to the other.

The number of columns in different torpedos, of the size of that now offered to the society, appeared to be about 470 in each organ; but the number varies according to the size of the fish.* These columns increase, not only in size, but in number, during the growth of the animal: new ones forming perhaps every year on the exterior edges, as there they are much the smallest. This process may be similar to the formation of new teeth in the human jaw, as it increases. Each column is divided by horizontal partitions, placed over each other, at very small distances, and forming numerous interstices, which appear to contain a fluid. These partitions consist of a very thin membrane, considerably transparent. Their edges appear to be attached to one another, and the whole is attached by a fine cellular membrane to the inside of the columns. They are not totally detached from one another: I have found them adhering, at different places, by blood vessels passing from one to another.

The number of partitions contained in a column of 1 inch in length, of a torpedo which had been preserved in proof spirit, appeared on a careful examination to be 150: and this number in a given length of column, appears to be common to all sizes in the same state of humidity; for by drying them they may be greatly altered: whence it appears probable that the increase in the length of a column, during the growth of the animal, does not enlarge the distance between each partition in proportion to that growth; but that new partitions are formed, and added to the extremity of the column from the fascia.

The partitions are very vascular; the arteries are branches from the veins of the gills, which convey the blood that has received the influence of respiration. They pass along with the nerves to the electric organ, and enter with them; they then ramify, in every direction, into innumerable small branches on the sides of the columns, sending in from the circumference all around, on each partition, small arteries, which ramify and anastomose on it; and passing also from one partition to another, anastomose with the vessels of the adjacent partitions. The veins of the electric organ pass out, close to the nerves, and run between the gills, to the auricle of the heart.

The nerves inserted into each electric organ, arise by 3 very large trunks, from the lateral and posterior part of the brain. The first of these, in its passage outwards, turns round a cartilage of the cranium, and sends a few branches to the first gill, and to the anterior part of the head, and then passes into the organ towards its anterior extremity. The 2d trunk enters the gills between the

* In a very large torpedo, the number of columns in one electric organ were 1182.—Orig.

1st and 2d openings, and, after furnishing it with small branches, passes into the organ near its middle. The 3d trunk, after leaving the skull, divides into 2 branches, which pass to the electric organ through the gills; one between the 2d and 3d openings, the other between the 3d and 4th, giving small branches to the gill itself. These nerves, having entered the organs, ramify in every direction, between the columns, and send in small branches, on each partition, where they are lost.

The magnitude and the number of the nerves bestowed on these organs, in proportion to their size, must on reflection appear as extraordinary as the phenomena they afford. Nerves are given to parts either for sensation or action. Now if we except the more important senses of seeing, hearing, smelling, and tasting, which do not belong to the electric organs, there is no part, even of the most perfect animal, which, in proportion to its size, is so liberally supplied with nerves; nor do the nerves seem necessary for any sensation which can be supposed to belong to the electric organs. And with respect to action, there is no part of any animal, with which I am acquainted, however strong and constant its natural actions may be, which has so great proportion of nerves. If it be then probable, that those nerves are not necessary for the purposes of sensation, or action, may we not conclude that they are subservient to the formation, collection, or management of the electric fluid; especially as it appears evident, from Mr. Walsh's experiments, that the will of the animal does absolutely control the electric powers of its body; which must depend on the energy of the nerves. How far this may be connected with the power of the nerves in general, or how far it may lead to an explanation of their operations, time and future discoveries alone can fully determine.

An Explanation of the Engraving of the Torpedo.

Pl. 9, fig. 4, The upper surface of the electric organ.—AA, The common skin of the animal.—B, The inspiratory opening.—C, The eye.—D, The part in which the gills are inclosed.—EEE, The skin dissected off from the electric organ, and turned outwards; the honeycomb appearance on its internal surface corresponding with the upper surface of the organ.—F, The part of the skin which covered the gills, with some ramifications of an excretory duct on it.—GGG, The upper surface of the electric organ, formed by the upper extremities of the perpendicular columns.

Fig. 5, The right electric organ, divided horizontally into nearly 2 equal parts, at the place where the nerves enter; the upper half being turned outwards.—AA, BB, CC, DD, The corresponding parts of the trunks of the nerves, as they emerge from the gills, and ramify in the electric organ.—AA, The 1st or anterior trunk arising just before the gills.—BB, The 2d or middle trunk arising behind the 1st gill.—CC, The anterior branch of the 3d trunk arising behind the 2d gill.—DD, The posterior branch of the 3d trunk arising behind the 3d gill.

Fig. 6, A perpendicular section of the torpedo a little below its inspiratory openings.—AA, The upper surface of the fish.—BB, The muscles of the back, as divided by the section.—C, The medulla spinalis. D, The oesophagus.—E, The left gill split, to expose the course of a trunk of the nerve through it.—F, The breathing surface of the right gill.—GG, The fins.—HH, The perpendicular columns which

compose the electric organ, with a representation of their horizontal partitions.—1, One of the trunks of the nervess, with its ramifications.

END OF THE SIXTY-THIRD VOLUME OF THE ORIGINAL.

I. Observations on the Solar Spots. By Alexander Wilson, M. D., Prof. of Astronomy, Glasgow. Anno 1774, *Vol. LXIV.* p. 1.

Many astronomers of the first note were very early engaged in the inquiry concerning the solar spots. Of all these Schiener and Hevelius deservedly hold the first place, and nothing but the charms of so noble an investigation could have induced them to prosecute their observations with so much assiduity. Scheiner began his in 1624, 14 years after Galileo had first made the discovery. In 1630, he at last published his Rosa Ursina, in which we have a detail of his labours during that long interval of time. Hevelius came after Scheiner, and diligently watched the appearances of the spots for 2 years, the result of which application he has given in his Selenographia and Cometographia.

But notwithstanding these attempts, so worthy of men actuated by a true desire of knowledge, it must be confessed that nothing of moment has been derived from them. If we except a few conclusions concerning the rotation of the sun round his axis, and the inclination of his axis to the plane of the ecliptic, every thing else, which has been inferred from the phenomena of the spots, seems altogether to be matter of conjecture. Hevelius, from his great fondness of the subject, and from a desire to avail himself of that long course of observation, to which he had so patiently submitted, has been led into many speculations concerning the spots and the nature of the sun's body. In his Cometographia, p. 360, he furnishes us with a remarkable instance of this, which serves to give us a view of the ideas he came to entertain on these subjects. But all that we there find, however plausible and ingenious, can be regarded only as conjecture. It does not appear, that any who have followed Hevelius have met with more success. Their observations seem not to differ from his in any remarkable circumstance; nor do we find that their inferences from them, though sometimes different, have any better pretensions to the truth. The many strange and variable circumstances of the spots, which were discoverable from a minute observation, still remained unaccountable; and we often find them at a loss in framing any hypothesis, which could fully satisfy the mind concerning them. In process of time atsronomers began to withdraw their attention from a subject which remained so dark and perplexing, and for many years all researches of this sort have been in a great measure laid aside. Chance, or a happy concur-

rence of circumstances, has sometimes effected more than could have been expected from the most promising measures: a remark which, it is hoped, will in some degree be found justified in the sequel of this paper. The observations on the solar spots, now to be related, appear to be totally different from any hitherto to be found, and such as seem to open a new and curious field of speculation into the whole of this subject.

Astronomers will remember, that a spot of an extraordinary size appeared on the sun, in Nov. 1769. On the 22d, Dr. W. had a view of the sun through an excellent Gregorian telescope, of 26 inches focus, which magnified 112 times. It was not far from the sun's western limb, and below his equatorial diameter. The atmosphere being very clear, and free from all tremor and undulation, it was pleasant to see the nucleus of the spot, and the shady zone or umbra which surrounded it, so very distinct. Next day he again saw the spot, having its nucleus and umbra very sharply defined. He now found however a remarkable change; for the umbra, which before was equally broad all round the nucleus, appeared much contracted on that part which lay towards the centre of the disc, while the other parts of it remained nearly of their former dimensions. This change of the umbra seemed somewhat extraordinary, as it was the very reverse of what he expected from the motion of the spot towards the limb. But next day, at 10 o'clock, he had another observation, and discovered changes which were still more unexpected. The distance of the spot from the limb was now about 24″. By this time the contracted side of the umbra had entirely vanished; and the figure of the nucleus was now remarkably changed, from what it had been the preceding day. This alteration of the figure appeared evidently to have taken place on that side which had now lost the umbra, the breadth of the nucleus being thereby more suddenly impaired than it ought to have been, by the motion of the spot across the disc.

Regarding these circumstances as new, Dr. W. began to consider what might be the cause of them. One of two things seemed necessarily to be the case; either, that they were owing to some physical alteration or wasting of the spot, and of that part of it where the deficiency of the umbra was observed; or else, that they were owing to the nearer approach of the spot to the limb, by the sun's rotation on his axis. The last of these two ideas had no sooner struck him, than he began to suspect that the central part, or nucleus of this spot, was beneath the level of the sun's spherical surface; and that the shady zone or umbra, which surrounded it, might be nothing else but the shelving sides of the luminous matter of the sun, reaching from his surface, in every direction, down to the nucleus: for, on this supposition, he perceived that a just account could be given of the changes of the umbra, and of the figure of the nucleus, above described. The opinion therefore which he ventured to form, from what he had

seen this day, was, that this spot might probably be a vast excavation in the luminous matter of the sun; the nucleus, commonly so called, being the bottom, and the umbra the shelving sides of the excavation: and that the umbra, next the centre of the disc, though out of view, did still however exist, and was rendered invisible by its present position only; and further, that the sudden alterations, now discernible in the figure of the nucleus, were occasioned by some part of it also being hid, by the interposition of the edge of the excavation, between the nucleus and the eye.

These considerations made him attentively wait its return. At last, on December 11th, he again discovered it, on the opposite side of the disc, it having by that time advanced a little way from the eastern limb, being distant from it 1′ 30″. And now he could only perceive 3 sides of the umbra, namely, the upper and under sides, and that towards the limb, which was the side that formerly had vanished. The side towards the centre of the disc was not as yet visible; but he concluded, on the same grounds as formerly, that it was hid from sight, by its averted position only, and that, after the spot had advanced a little farther, it would make its appearance. Accordingly, the next day, at 10 o'clock, it came into view, and he saw it distinctly, though narrower than the other sides. After this, his observations were interrupted by unfavourable weather, till the 17th, when the spot had passed the centre of the disc, the umbra now appearing to surround the nucleus equally. All the foregoing appearances, when taken together, and when duly considered, seem to prove in the most convincing manner, that the nucleus of this spot was considerably beneath the level of the sun's spherical surface.

The next thing was, to think of some means by which he might form an estimate of its depth. At the time of the observation on Dec. 12th, he had remarked that the breadth of the side of the umbra, next the limb, was about 14″; but, for determining the point in question, it was also requisite to know the inclination of the shelving side of the umbra to the sun's spherical surface. And here it occurred, that in the case of a large spot, this would, in some measure, be deduced from observation. For, at the time when the side of the umbra is just hid, or begins first to come in view, it is evident, that a line joining the eye and its observed edge, or uppermost limit, coincides with the plane of its declivity. By measuring therefore the distance of the edge from the limb, when this change takes place, and by representing it by a projection, the inclination or declivity in some measure may be ascertained. Dr. W. had not an opportunity, in the course of the foregoing observations, to see the spot at the time when either of the sides of the umbra changed. It is however certain, that when the spot came on the disc for the 2d time, this change happened some time in the night between the 11th and 12th of December; and he judged that

the distance of the plane of the umbra, when in a line with the eye, must have been about 1′ 35″ from the sun's eastern limb; from which we may safely conclude, that the nucleus of the spot was at that time not less than a semidiameter of the earth below the level of the sun's spherical surface, and formed the bottom of an amazing cavity, from the surface downwards, whose other dimensions were of much greater extent.

Being thus persuaded of the depression of this great spot below the surface, he immediately set about examining smaller ones, in order to discover if they were of the same kind. With this view, he began a course of observations, that from them he might either make the inference universal, or limit it, as the phenomena should point out. Dr. W. was not long engaged in this pursuit, before he perceived in them the same changes of their umbræ, which have been described above. This was manifest in spots of any considerable size, when the air was favourable, and the telescope well adjusted for distinct vision. In general, he found that the umbra thus changes, when a spot is about a minute distant from the limb, at a medium. From all these observations, may we not safely conclude, that every spot consisting of a nucleus and surrounding umbra, as defined by Scheiner and Hevelius, is of the same kind with those above described.

In the course of the foregoing observations, Dr. W. had occasion to remark, 5 different times, another extraordinary circumstance of the spots, which he had not seen mentioned by any one who has written on the subject. It consists of changes which seem to arise from a disturbing force, when one spot breaks out in the neighbourhood of another. The first case of this sort which he met with, was on Nov. 9th, 1770, when the umbra of a spot, though a great way from the limb, was deficient towards the right hand, at which side, and very near it, there lay another spot much smaller. In like manner, 2 other spots, which lay very near each other, had each of them that side of its umbra, which faced the other, taken away. But it was remarkable, that 3 days after, the one spot had nearly vanished, when the side of the umbra of the other spot, which faced it, began now to dilate. Another spot had its umbra flattened on opposite sides, by 3 small spots on one hand, and one on the other. Again, 2 other spots had their umbræ deficient, by the intervention of some small spots, that lay between them.

Now it must here be particularly remarked, that though a spot, when undisturbed, will, when near the sun's limb, exhibit the change of the umbra before mentioned; yet it is plain that a case may now and then occur, when this change will be counteracted, by means of the phenomenon just now described. And accordingly, in the course of the observations, he in reality met with 3 cases, when this change did not take place.

Dr. W. is sensible, that it may be thought strange, that none of the observers, who had looked at the solar spots with so much attention, should ever have taken notice of the gradual changes above described. This partly may be accounted for from the following considerations. We have found that conjectures, concerning the nature of the sun, were early indulged in the course of this inquiry. His body was thought to be an immense globe of fire, which was for ever raging with the most fervent heat. Hence the first observers, reflecting on the perpetual generation, changes, and decay of the spots, and that through so wide an extent of his surface, very naturally imagined, that they could consist of nothing but smoke and grosser exhalations, or such transient and perishable materials. This hypothesis had at least the air of being supported by a very plausible analogy. The minds of men being carried away by such prepossessions, it would less readily occur, that successful observations were only to be made, by an accurate and critical attention to those minute changes, which the spots sometimes undergo. But what would still more conduce to this oversight, was the method which most of them followed, in making their observations. This was by the camera obscura, which both Scheiner and Hevelius often used, and which we find greatly extolled by them, and described at great length in their writings. But spots, when seen in this way, have nothing of that distinctness, which is so remarkable, and so pleasing, when they are viewed directly through a good telescope armed with an helioscope, or glass properly smoked.

It appears then, that the solar spots are immense excavations in the body of the sun; and that what hitherto has been called the nucleus, is the bottom, and what has been called the umbra, the sloping sides of the excavation. It also appears, that the solar matter, at the depth of the nucleus, does not emit light, or emits so little, as to appear dark compared to that resplendent substance at the surface; that this beauteous substance is at the surface most fulgid; and when any of it is seen below the general level, forming the sides of an excavation, that then its lustre is some how impaired, so as to give the appearance of a surrounding umbra. Here our induction ends. To proceed further would be to carry it beyond its true limits, and to intermix with conclusions, which are certain and manifest, the suggestion of hypotheses, which at best are precarious and liable to error. But from what we have now seen, many curious speculations do naturally present themselves. By what mysterious process is it, that those astonishing excavations are at first produced? What is the nature of that shining substance, which is thereby perpetually disturbed? To what are we to ascribe the darkness of the nucleus, and the diminished lustre of the umbra? And what conceptions are we to form of the many strange changes, and at length of the final decay of all these appearances, by which those regions of the sun, that were so hurt and disfigured, again undergo a renovation?

We often find Scheiner and Hevelius mentioning many things concerning the spots, which appeared to them very inexplicable. Hevelius, when speaking of the vast number of spots which break out upon the sun, and of the prodigious size of some of them, admires how from his single body so much matter, exhalations, &c. could be generated, as in any degree to be adequate to so many and so vast phenomena. Now every theory, how ingenious soever, which is founded on a misapprehension of things, is apt to be pressed with many difficulties; and whenever palpable contradictions appear, they may be regarded as proofs of our having fallen into error. It must indeed be acknowledged, that it is very disadvantageous to science, to indulge much in hypotheses, the truth being rarely hit upon in this way, and very often missed. Sometimes however it may not be improper to throw out hints and conjectures, when we can attain to nothing better, provided we are at due pains to distinguish between such, and that real knowledge which we derive, by strict induction, from incontestable principles. The best way therefore, of preserving so proper and necessary a distinction, will be to propose what further remains to be said on this subject, in the form of queries; because, however plausible they may appear, they are at best but matter of conjecture. Hints, when propounded in this way, are freed from the danger of making us rest in any error, while, sooner or later, they may become helps in leading us to a right understanding of the subject.

The queries which Dr. W. makes, are chiefly founded on the following phenomena of the spots, as described by Scheiner and Hevelius. 1. Every spot which has a nucleus, has also an umbra surrounding it. 2. The boundary between the nucleus and umbra is always distinct and well defined. 3. The increase of a spot is gradual, the breadth of the nucleus and umbra dilating at the same time. 4. In like manner the decrease of a spot is gradual, the breadth of the nucleus and umbra contracting at the same time. 5. The exterior boundary of the umbra never consists of sharp angles, but is always curvilinear, how irregular soever the outline of the nucleus may be. 6. The nucleus of a spot, while on the decrease, in many cases changes its figure, by the umbra encroaching irregularly on it; insomuch that, in a small space of time, new encroachments are discernible, by which the boundary, between the nucleus and umbra, is perpetually varying. 7. It often happens, by these encroachments, that the nucleus of a spot is divided into 2 or more nuclei. 8. The nuclei of spots vanish sooner than the umbræ. Many instances of this sort are to be seen in Hevelius' plates, and the same is affirmed by Mr. Derham in the Phil. Trans. 9. Small umbræ are frequently seen without nuclei. 10. An umbra of any considerable size is seldom seen without a nucleus in the middle of it. 11. When a spot, which consisted of a nucleus and umbra, is about to disappear, if it is not succeeded by a facula; or more fulgid appearance; the place which it occupied is soon after

not distinguishable from any other part of the sun's surface. This is certain from the accounts of all observers.

Queries and Conjectures, tending to explain the above Properties of the Spots.

When we consider that the solar spots, some of whose properties have just now been enumerated, are so many vast excavations in the luminous substance of the sun, and that, wherever such excavations are found, we always discern dark and obscure parts situated below; is it not reasonable to think, that the great and stupendous body of the sun is made up of two kinds of matter, very different in their qualities; that by far the greater part is solid and dark; and that this immense and dark globe is encompassed with a thin covering of that resplendent substance, from which the sun would seem to derive the whole of his vivifying heat and energy? And will not this hypothesis help to account for many phenomena of the spots in a satisfactory manner? For if a portion of this luminous covering were by any means displaced, so as to expose to our view a part of the internal dark globe, would not this give the appearance of a spot? In this case, would not that part of the dark globe, which is now laid bare, correspond to the nucleus, and the sloping sides of the luminous matter to the umbra? And is not this consonant to that property of a spot mentioned in the first article; for would it not hence follow, that every spot, having a nucleus, should also have an umbra surrounding that nucleus, a natural account being at the same time suggested, for the boundary between the nucleus and umbra being always distinctly defined, as mentioned in the 2d article.

Though we may never have a competent notion of the nature and qualities of this shining and resplendent substance, or of the means by which the excavations in it are formed; we however discover, in their production, the agency of some mighty, though unknown cause, which is there often exerting itself. Though we manifestly behold its effects, yet the mode of its operations may perhaps remain unsearchable. But if we were here to venture a conjecture, might we not suppose, that the luminous matter is so disturbed, and the excavations in it occasioned, by the working of some sort of elastic vapour, generated within the dark globe? And might not this elastic principle, by its expansion, swell into such a volume, as to reach up to the surface of the luminous matter, which would thus be separated and laid aside in all directions? And as there is no regularity in the time of a spot's enlarging, compared to the time of its decreasing, some enlarging quickly, and decreasing slowly, and vice versa, may we not imagine, that this is owing to the duration and quantity of the elastic principle now mentioned? and in general, may we not hence form some idea of the production and subsequent enlargement of a spot, as mentioned in the 3d article?

But to proceed: as we know from experience, that the spots are of a transient

nature, not lasting on the sun for a long space of time, does it not seem reasonable to think, that their gradual decrease, as mentioned in article 4th, is occasioned by the luminous matter encroaching again on that part of the dark globe, which had been uncovered? And from this may we not infer, that the luminous matter gravitates, and is in some degree fluid; for thus would it not have a tendency to flow down, in all directions, and encroach, so as at last to cover the nucleus? And do not these things appear further probable, when we reflect on that uniform inclination, which the sides of the umbra or excavation, have to the external surface of the sun's body? For does not this indicate a fluid sort of matter gradually yielding to the force of gravity? And again, is not this notion further supported, when we consider the property mentioned in the 5th article, namely, that the exterior boundary of the umbra never consists of sharp angles or turnings, but is always curvilinear, and most frequently of a round form: for we know that this boundary is nothing else but the lip of the excavation, which, on supposition that the luminous matter possesses some degree of fluidity, will not be disposed, either in enlarging or contracting, to become irregular by sudden or sharp turnings?

On supposition that the surface of the dark globe of the sun is smooth and level, it may be urged, that the nucleus of a spot, while on the decrease, should, according to the present view of things, always acquire a figure at least nearly circular, and that the luminous matter, continuing to flow down on all sides by an equal gravity, should so encroach on the nucleus, as to make it retain that figure, till at last it be entirely overflowed. But this not being the case, and because it most frequently happens, that the encroachments of the umbra on the nucleus are extremely variable, as mentioned in the 6th article, may we not from this infer, that the surface of the internal dark globe of the sun, is by no means smooth and level, but on the contrary very irregular, for, on this supposition, if for example the area of the nucleus of a great spot were so diversified by mountains and vallies, would not the encroachments of the luminous matter be consequently irregular: and, according as it was more or less retarded or accelerated, at different places, by being contiguous to prominencies or hollows, would not all the alterations in the figure of the decreasing nucleus, how variable soever, be thus plainly accounted for? and because it often happens, that the nucleus of a spot, while on the decrease, is gradually cut in pieces by a luminous zone or zones, which wander across it, as mentioned in the 7th article, does not this look like the gradual flowing in of the luminous matter, as it were, into deep channels, which would thus appear to abound in the surface of the sun's dark body? If we reflect on the irregularities on the surface of this earth, and on the enormous mountains and cavities in the moon, may we not, from such analogy,

imagine, that there may be the like, or much greater, irregularities in the surface of the sun?

Is not the property mentioned in the 8th article, namely, that the nucleus of a spot vanishes sooner than the umbra, also agreeable to the present views? From this state of the phenomenon, we suppose that that part of the sun's dark body, which had been uncovered and exposed to our view, when the spot first broke out, is now again just overflowed by the gradual inundation of the luminous matter. But, after the nucleus thus disappears, may there not however, in many cases, be still left a cavity in the luminous matter, large enough to be perceived? and will not this cavity, so long as it continues, give the appearance of a small undivided umbra? and will not this umbra still be perceivable, till the luminous matter, by continuing to flow in, has filled up the cavity? after which, will not the place of the umbra acquire the same lustre with the rest of the sun's surface, and thus will not all traces of the spot vanish from his body? And do not the particulars mentioned in the 9th, 10th, and 11th articles seem agreeable to what is now said?

Both Scheiner and Hevelius seem to think, that spots sometimes alter their place on the disc, not only by the sun's rotation round his axis, but also by a motion, which they impute to the spots themselves. This Dr. W. could never observe. It is very true, that when a number of small spots lie near one another, there may be from time to time a change of their relative situation; but it is plain that this may proceed entirely from some of them increasing and others diminishing irregularly. But what would further contribute towards forming a judgment of this kind is, the apparent alteration of the relative place, which must arise from the motion across the disc on a spherical surface.

What has been advanced, in the course of the foregoing queries, may perhaps be rendered still more probable, by considering the observations related in the first part of this paper, concerning the changes which are made on the figure of a spot, when another breaks out in its neighbourhood; and which seem to arise from a disturbing force. For, from the cases there laid down, would it not appear, that when a spot is breaking out, the luminous matter is then forced, in all directions, from the nucleus, and is affected much in the same manner, as it would be, were it a fluid matter encompassing the sun's dark body? As to the particular nature and qualities of this luminous matter, we have been sometimes apt to imagine, that it cannot well be any very ponderous fluid, but that it rather must resemble, as to its consistence, a very dense and thick fog, that broods on the surface of the sun's dark body. How far will this idea tend to facilitate our conceptions of the various phenomena of the spots above described?

It has been gathered from many observations, that the time which the spots

take to traverse the whole disc, is nearly equal to the time that they are hid by being on the opposite surface. It is plain, that the time of their appearing on the disc must be some small matter shorter than that of their being hid behind it, on account of our not seeing a complete hemisphere of the sun. But further, it must now be considered, that when a spot just enters the disc, the part which is first visible, is the farthest umbra, by which time the spot has really advanced a whole diameter of itself on the disc. And again, when the same spot goes off the disc, it is evident, that the part which is last visible, is then the farthest umbra, on which account the continuance of the spot on the disc will be shortened by an interval of time, which corresponds nearly to the whole breadth of it. This, as well as the other appearances, described in the first part of this paper, concerning the change of the umbra and figure of the nucleus, when spots approach the limb, are all well illustrated, by making, in a sphere, an excavation similar to what we have described, the bottom of which may be painted black to represent the nucleus, and the sloping sides shaded, if the sphere be of a light colour.

According to the view of things given in the foregoing queries, there would seem to be something very extraordinary in the dark and unignited state of the great internal globe of the sun. Does not this seem to indicate, that the luminous matter which encompasses it derives not its splendour from any intensity of heat? For if this were the case, would not the parts underneath, which would be perpetually in contact with that glowing matter, be heated to such a degree, as to become luminous and bright? At the same time it must be confessed, that though the internal globe was in reality much ignited, yet when any part of it, forming the nucleus of a spot, is exposed to our view, and is seen in competition with a substance of such amazing splendor, it is no wonder that an inferior degree of light should in these circumstances be unperceivable. But from the nature of the thing, does there seem any necessity for thinking, that there prevails there any such raging and fermenting heat, as many have imagined? It is proper here to attend to the distinction between this shining matter of the sun, and the rays of light which proceed from it. It may perhaps be thought, that the re-action of the rays on the matter, at their emission, may be productive of a violent degree of heat. But whoever would urge this argument, in favour of the sun being intensely heated, as arising from the nature of the thing, ought to consider, that all polished bodies are less and less disposed to be heated, by the action of the rays of light, in proportion as their surfaces are more polished, and as their powers of reflection are brought to a greater degree of perfection. And is there not a strong analogy between the re-action of light on matter, in cases where it is reflected, and in cases where it is emitted?

It may perhaps be expected, that in this paper, mention should be made of the

other appearances, that are discernible on the surface of the sun, besides the spots properly so called; viz. the faculæ, luculi, &c. as described by Scheiner and Hevelius. But all these phenomena seem to be so different from any thing above considered, and so unconnected with the present discovery, that little assistance can be brought from that quarter towards a right conception of them. As to the faculæ, or brighter parts of the sun, we are at a loss for their origin. It may in general be remarked, that though we have obtained an experimental proof, that the luminous matter acquires some degree of shade, when forming the sides of an excavation, yet it is uncertain if this be merely the effect of position, and much more so, if any different modification of position could ever dispose it to put on a brighter or more fulgid appearance. Yet, after all, may not these faculæ, &c. depend on some irregularities in the bright surface of the sun? For may not the luminous matter, by being agitated by the same cause to which the spots owe their origin, though in a less degree, have its surface perpetually disturbed, and made irregular, and thus give occasion to a variety of light and shade, sufficient perhaps to produce the phenomena under consideration? And does not this conjecture receive further confirmation, when we consider, that these faculæ, &c. are found only in that zodiac, within which the spots appear, and that they always abound most in the neighbourhood of the spots themselves, or where spots recently have been? For in those undisturbed regions of the sun that lie towards his poles, and where no spots ever appear, and which Scheiner calls the plagæ æquabiles, we never discover any diversity of appearance.

Thus Dr. W. has endeavoured to give a general idea of the production, changes, and decay of the solar spots, considered as excavations in the body of the sun; a thing which seems to be established from the observations described in the first part of this paper. But concerning the nature of that mighty agency, which occasions those amazing commotions in the luminous matter, or concerning the density, viscidity, and other qualities of this matter, or the manner in which it is disturbed in the middle zone only, and not at the polar regions, and many such other questions, he freely confesses, that they far surpass his knowledge.

II. Astronomical Observations by the Missionaries at Pekin. Transmitted to the Supra-cargoes at Canton, by the Rev. Father Louis Cipolla, of the Tribunal of Mathematics, and communicated to the R. S. by the Court of Directors of the East India Company. p. 31.

Preface by the Astronomer Royal.—Most of the following observations appear to have been made with a telescope of 8 feet, to which a micrometer, for measuring differences of right ascension and declination, was occasionally adapted.

There was besides another telescope of 8 feet, consisting of 2 object-glasses, in the manner of Röemer, which might be brought nearer together or separated, in order that the moon's diameter might completely fill a fine reticule, in the focus, divided into 12 equal parts, for measuring the digits eclipsed in lunar eclipses. It is mentioned in the account of the lunar eclipse of Nov. 12, 1761, that the clock was regulated by a transit instrument. It is therefore probable it was regulated in the same manner in all the succeeding observations, which consist of the following particulars:

1. Observations of the last transit of Venus over the sun, viz. of differences of right ascension and declination between Venus and the sun, and the internal and external contact of the limbs at the egress. It is however remarked, that the clock was counted by a person, who was sometimes found to make mistakes. —2. The eclipse of the sun, May 25, 1770. The beginning and end were observed, and the lucid parts measured, during the eclipse, with the micrometer. —3. The beginning and end of the eclipse of the moon, Oct. 23, 1771.—4. Emersion of Jupiter from occultation by the moon, July 5, 1770.—5. An occultation of Spica Virginis by the moon; the immersion and emersion both observed Jan. 25, 1772.—6. The occultation of a star in Scorpio by the moon; the immersion and emersion both observed.—7. The observation of Venus in the sun's parallel, Jan. 5, 1772, by taking the difference of right ascension and declination of Venus and the sun.—8. The total eclipse of the moon, Nov. 12, 1761. The beginning, total immersion, emersion, and end, were observed by 3 different observers, with telescopes of 5, 7, and 8 feet, in the domestic observatory of the College of the Jesuits; where also all the former observations were made. The same eclipse was also observed at the Royal Observatory at Pekin, 14″ west of the Jesuit's College, with a telescope of 8 feet, composed of 2 object-glasses, with a reticule at the focus, divided into 12 equal intervals for measuring the digits eclipses, in the manner of Röemer. It is remarked, that the leaf, in which this observation was recorded, had been lost, and was found again, Oct. 12, 1772; on which account this observation was never transmitted to Europe before. NEVIL MASKELYNE.

The observations themselves are however omitted, being not now of any further use.

III. The Lunar Eclipse, Oct. 11, 1772, observed at Canton. Communicated by John Blake, Esq., of Parliament-street. p. 46.

The time being taken only by a watch regulated by the sun the day before, the observation is not much to be depended on. NEVIL MASKELYNE.

IV. Experiments on Dying Black. By Mr. James Clegg, of Redivales, near Bury. p. 48.

Lime having been proved to increase the solvent power of water, on astringent

vegetables, for medical purposes, Mr. C. was desirous of knowing if it would be equally useful in the art of dying black: to this end he made the following experiments.

Exper. 1. Four pennyweights of each of the following astringents, viz. galls, sumach, oak bark, bistort root, and logwood, were boiled during 10 minutes, in half a pint of pure river water; on mixing the decoctions with a saturated solution of martial vitriol, in the proportion of $\frac{1}{3}$ of the solution to $\frac{2}{3}$ of the decoction, they struck colours differently inclining to blackness, in the following order: viz. oak bark, bistort root, sumach, galls. He then boiled the same weight of all the astringents, in the same quantity of lime water, and on mixing them as above, the colours they produced were inferior to those with plain water, the astringency of the logwood, or whatever gives it the property of striking black with green vitriol, was entirely destroyed; it produced not the least blackness with any quantity of vitriol.

Exper. 2. Four pennyweights of each of the astringents above-mentioned, were tritured in plain water, and 4 others in lime water; the measures of water used were equal to those left, after boiling, in the last experiment; and, on being mixed with martial vitriol, as in the last experiment, the colours produced, by this means, were superior to those produced by boiling. Those tritured in lime-water were judged to be the deepest, which agrees with Mr. Henry's experiments; but we must again except the logwood, which gave no colour by trituration, more than by boiling in lime-water.

Exper. 3. All the above mixtures having been written with as inks, and exposed 6 months to the air; those boiled in lime-water had failed much; those tritured in lime-water, and in plain water, had faded a little; those boiled in plain water evidently preserved their colour best. On slightly rubbing the faded writings, with a fresh astringent liquor, they recovered their original blackness; by which it appears, that it was the astringent parts of those inks which had failed.

Does it not appear, by these experiments, that, though lime water tends to deepen the colour produced by some astringents and martial vitriol, it by no means adds to the duration of those colours; and as lime-water, either by trituration or coction, entirely destroys the property, in logwood, of striking black with martial vitriol, it can by no means be of service, in the black dye, where logwood is a material ingredient. Does it not also appear, that a slight boiling is preferable to trituration, for the purposes of dying, when a durable colour is wanted?

Having observed a solution of iron, in a vegetable acid, struck a deeper black, on mixture with an astringent, and produced its effects much more expeditiously, than a strong solution of martial vitriol; it occurred, that the iron, being more slightly combined with the vegetable acid than with the vitriolic, made it more

easy for the astringent matter to decompound the former, and produce an ink; if this was the case, he suspected, that lime-water deepened the colour of astringent and chalybeate mixtures, not so much by its action on the astringent, as on the chalybeate, the lime uniting with the superabundant acid, and leaving the iron with so much of the acid, as is necessary for the formation of an ink, to be more easily attached by the astringent matter of the vegetable. But if this theory was well founded, it followed, from analogy, that any substance, which had a greater affinity with the vitriolic acid than iron had, would produce the same effect, in some degree, as lime. To determine this:

Exper. 4. He took 2 vessels, containing equal measures of a strong astringent liquor, composed of galls and logwood: into one vessel he put a small quantity of pearl ashes; the other remained as a standard. Pieces of linen and cotton cloth, after maceration in these liquors, were thrown together into a strong solution of copperas; they were soon after taken out, and washed in cold water; when dry, the pieces prepared in ashes were, all of them, much deeper than the others.

He made use of different kinds of pearl and pot-ashes, as well as of many kinds of astringents; the ashes had the same effect, whatever astringent was made use of, and the strongest alkali always produced the deepest colour; and though ashes, used with an astringent, always gave a deeper black, than the same astringent without ashes, yet logwood, which without ashes gave not so deep a colour as galls with them, gave a much deeper black than galls with the same addition. There was a remarkable difference, in this case, between lime and ashes, in their effect on logwood; with lime it gave no blackness; but with ashes, it produced a deeper black than any other astringent he made use of.

Being desirous of trying the duration of colours, produced by astringents, in which different quantities of pearl-ashes had been dissolved;

Exper. 5. In 2 pints of river water, he boiled 1 oz. of logwood, during 10 minutes; he then added half an ounce of Aleppo galls, and boiled them together 10 minutes longer; the liquor having stood to cool, was decanted off, and divided into 6 equal quantities. N° 1 remained as a standard; into N° 2 he put 6 grains of fine pearl-ashes; N° 3, 12 grains; N° 4, 18 grains; N° 5, 24 grains; N° 6, 30 grains: to 6 drops of each of these liquors, he added 2 drops of a saturated solution of copperas; N° 2 and 3 struck a deep black; N° 1 and 4 black, but inferior to 2 and 3; N° 5, a brown black; N° 6, brown.

From this experiment it appears, that N° 5 and 6 were spoiled by an over proportion of ashes. Before seeing experiments, wherein it had been demonstrated, that a quantity of acid enters into the composition of ink, Mr. C. imagined the alkali decompounded the copperas too suddenly, and disengaged the iron faster than the astringent matter could unite with it. But most probably

the alkali neutralized too great a portion of the acid. All these writings having been now exposed 6 months to the air, in N° 5 and 6 the blackness is quite destroyed; N° 4 is somewhat faded; N° 1, 2, 3, remain nearly as they were; N° 2 and 3 being still superior to the standard.

V. Observations on the State of Population in Manchester, and other adjacent Places. By Dr. Percival. p. 54.

Reprinted in this author's collected works, recently published (1807) in 4 vols. 8vo. by his son.

VI. Observations on the Bill of Mortality, in Chester, for the year 1772. By Dr. Haygarth. p. 67.

A writer, of distinguished abilities in political arithmetic, has offered many arguments, which give cause to apprehend that England, in about 70 years, has lost near a quarter of her people. Accurate registers of mortality, with other collateral inquiries, can, with most certainty, confirm or confute this opinion, and determine a question of the most striking importance to our very existence as a nation. The doctrine of annuities for widows, and other persons in old age, the value of reversionary payments, and of assurances on lives, and other important questions in civil society, can only be determined by faithful registers, showing the duration of human life, in various situations of town and country. The slightest view of tables of mortality show, how erroneous every calculation relating to this subject must be, drawn from the London bills, or perhaps those of most other considerable towns, and applied to the inhabitants of this city.

Chester is healthy to an uncommon degree, when compared with towns of the same size. Various circumstances, which contribute to render this place so remarkably salubrious, might be pointed out; but it can here be only observed in general, that this salutary effect may, with great probability, be chiefly attributed to the dry situation, clear air, pure water, and general temperance of the people. In August 1772, the inhabitants of St. Michael's, one of the 9 parishes into which Chester is divided, and situated in the very centre of the city, were numbered with great accuracy: in this parish were 151 families, 127 houses, 618 inhabitants, 246 males, 372 females, 166 married, 41 widows, 21 widowers, and 137 children under 15 years old. Hence the number of persons, never married above 15, is 253. From this account also it appears, that near $4\frac{5}{6}$ persons dwell in each house; that the proportion of females to males is as 62 to 41, or nearly as 3 to 2; that the widows are to the widowers nearly as 2 to 1; that the number married is little more than one quarter of the inhabitants: the common proportion of married people is about $\frac{1}{3}$ of the whole. The number of christenings, at St. Michael's, for the last 10 years, are 147, or 14.7 yearly;

the burials, during the same period, are 127, or 12.7 yearly. Hence the proportion of annual births to inhabitants is nearly as 1 to 42, and burials nearly as 1 to $48\frac{2}{5}$. During 1772, only 9 persons died in this parish; hence the proportion of deaths to the living, this year, is less than 1 in 68. These facts must appear most astonishing to any one who reflects, that in the largest towns, such as London, 1 in $20\frac{3}{4}$ dies annually; and that in towns of a moderate size, as Leeds, 1 in $21\frac{3}{5}$; that in Northampton and Shrewsbury, either of them less than Chester, 1 in $26\frac{2}{5}$ dies yearly. These facts, relating to this parish, are true, beyond a possibility of doubt; and yet they are so very extraordinary, that one cannot, without further inquiries, apply to the whole town, by analogy, the observations which were made on only a small proportion of the inhabitants. However no peculiarity of air, water, or any other obvious circumstance, can be supposed to render this parish more healthy than the rest of the town. How far these facts have been accidental, the following, and other collateral inquiries, will discover.

For the last 8 years, preceding 1772, there have been 385 births, and 375 deaths annually in Chester. The number of deaths this year, excluding those who were killed by the dreadful explosion of gunpowder, is 379; so that, probably, the conclusions drawn from tables, which have been executed with great care and fidelity, will not be liable to any considerable errors; and such errors, by continuing this account for a period of years, will most effectually be corrected. The following observations are offered as a small specimen of the conclusions, that may then with more certainty, be deduced from such a register of mortality. From such bills, which distinguish the ages at which the inhabitants die, it appears, as far as one year's observation may be trusted, that, taking the whole town, 1 in 31.1 dies annually. This proportion, of deaths to the living, is probably too high, because the births, on an average, exceed the burials; a fact, which affords another proof, that the place is uncommonly healthy. Other facts amply confirm this observation.

Half the inhabitants, born in London, die under $2\frac{3}{4}$ years old; in Vienna, under 2; in Manchester, under 5; in Norwich, under 5; in Northampton, under 10; in Chester, this year, above half who died were 20 years old. Of all the children born in this city, 1 in $5\frac{1}{3}$ lives to above 70, and 1 in $15\frac{3}{4}$ attains 80 years of age; whereas in Northampton, only 1 in $21\frac{2}{3}$; in Norwich, 1 in 27; and in London, 1 in 40 lives till 80. In the Hotel Dieu, a large hospital in Paris, above 1 in 5 dies, of all that are admitted; in St. Thomas's and St. Bartholomew's, in London, 1 in 13; in the Chester infirmary, since its first institution in 1755, till 1772 inclusive, only 1 in $25\frac{1}{6}$.

But the annexed table, at one view, shows the comparative state of health, between this and some other towns of different magnitudes. It is curious to

compare, by this table, in the early part of life, the probability that the inhabitants in Chester have, to live longer than in Northampton, Norwich, and especially much longer than in London. But when they have arrived at 70 years old, the chance of living, at all the places, is nearly equal. It is a matter of curiosity, to observe how much longer women live than men. This fact is well established by former observations on this subject, and is confirmed by this register. During the last year 12 widowers have died, and 53 widows; that is above 4 times the number. Between 80 and 90 years old, 2 men and 18 women have died; that is 9 times as many. Above 90 years old, 4 have died, and all women.

The year to which the several ages below have an equal chance to live.

Ages.	Chester.	Northam.	Norwich.	Lond.
0	$21\frac{1}{4}$	$9\frac{1}{4}$	5	$2\frac{3}{4}$
3	$55\frac{2}{3}$	$43\frac{1}{2}$	$43\frac{1}{4}$	$34\frac{1}{2}$
5	$58\frac{1}{2}$	$46\frac{1}{2}$	47	40
10	60	50	$52\frac{1}{4}$	44
20	63	$53\frac{1}{4}$	$55\frac{3}{4}$	$47\frac{1}{2}$
40	69	$62\frac{1}{2}$	$63\frac{1}{3}$	58
50	$71\frac{1}{2}$	$67\frac{1}{2}$	67	65
60	$73\frac{2}{3}$	$72\frac{3}{4}$	$71\frac{1}{3}$	$70\frac{1}{2}$
70	77	78	77	77

VII. Electrical Experiments. By Mr. Edw. Nairne, of London, Mathematical Instrument-maker. p. 79.

These experiments were made with an instrument of Mr. Nairne's own workmanship, a description of which is prefixed. The glass cylinder of this machine was 12 inches diameter, and the cylindrical part 19 inches long, exclusive of the necks; the cushion or rubber 14 inches long, and 5 inches broad, supported by 2 wooden springs; which springs were fixed on 2 glass rods, which lie horizontal under the cylinder, and serve to insulate the cushion. The conductor to this machine was 5 feet long, and 12 inches diameter; at the end of it was a short brass rod, with a ball; it was supported on 2 stands, with solid glass rods or pillars. The ball, for receiving the electrical spark from the conductor, was of brass, and fixed to the end of a brass tube, movable in a hole in the top of the receiving stand; from the bottom of this stand a chain passes along the floor, till it is in contact with the chain hanging from the back of the cushion. Mr. N. with this machine has frequently drawn electrical sparks, at the distance of 12, 13, or $13\frac{1}{2}$ inches, from the prime conductor. These were indeed the distances, to which the electrical fire would commonly strike. It would sometimes reach the distance of 14 inches; though but seldom.

Mr. N. used also a small brass conductor, instead of the large one, for charging the batteries, which batteries are composed of 4 boxes, each containing 16 jars of 12 inches high and 4 inches diameter, coated 8 inches high; so that, in the 64 jars, there are very nearly 50 square feet of coated surface. The electrometer is raised, so as to be 4 feet from the bottom, which rests on the jars, to the ball at top. Discharging this battery, through a piece of iron wire (not steel) of $\frac{1}{151}$ of an inch diameter, and 45 inches long, it flew about the room in in-

numerable red-hot balls; on examining these balls, they were in general hollow, and seemed to be nothing but scoria. Mr. N. has made a piece of the same wire, of 47 inches long, red-hot, from end to end, so that it separated into several pieces. After this, he took a piece of the fine iron wire before-mentioned, of 6 inches in length, and, to the end of it connected a piece of iron wire $\frac{1}{25}$ of an inch in diameter, and 48 feet long. Then, on discharging the battery, the electrical fire from the inside passed immediately along the discharging rod to the fine wire, and afterwards had 48 feet to pass, to get to the outside coating of the battery: he then laid another piece, so that the electrical fire passed 48 feet, from the inside of the battery, before it came to the small wire; and again another, so that the electrical fire passed from the inside of the battery 24 feet, before it came to the fine wire, and had 24 feet afterwards to pass, before it could get to the outside coating of the battery; in each case, the 6 inches of the small wire was melted into red-hot balls; and he could not perceive that there was the least difference in the melting of the wire, on its being placed in different parts of the circuit.

Next, he connected to a piece of the same fine iron wire, of 6 inches in length, a piece of the iron wire $\frac{1}{25}$ of an inch in diameter, and in one continued piece of 274 feet in length. In this arrangement, when the battery was discharged, the electrical fire passed immediately from the discharging rod to the fine wire, and had 274 feet to pass afterwards, to get to the outside coating; then the fine wire was laid next the outside coating of the battery, so that the electrical fire passed 274 feet before it reached it. This experiment was repeated several times, with this difference, that before every discharge of the battery, he shortened the fine wire, till at last there was only half an inch of it connected with the 274 feet of wire; but even that short piece was not made red-hot by the discharge of the 64 jars. The electrical fire, in passing that 274 feet of wire, though it was one entire piece without any joinings, seemed to meet with great resistance, for the explosion from the battery was not so loud, as when a very small electrical bottle is discharged.

Next, he took some silver thread, and made a circuit, of 40 feet, from the inside of the battery to the outside; and at the distance of about 12 feet from the battery, he held the silver thread between his finger and thumb, so that the electrical fire, passing along the thread, passed between them; on discharging the battery, he received a smart shock, particularly in both his ancles, though the thread was held $3\frac{1}{2}$ feet from the dry floor, on which he stood; by the electrometer, the battery did not appear to be half discharged. He then made a circuit, of 40 feet, with an iron wire $\frac{1}{25}$ of an inch in diameter, and this was held in the same manner as the silver thread: on discharging the battery through

the iron wire, there was not the least shock felt, though the whole of the battery was discharged, the iron wire of that length conducted it so perfectly.

Mr. N. then tried the effect of the battery on some platina. Several of the grains, or laminæ, were laid on a piece of white wax, so as to make a length of half an inch. On discharging the battery through the platina he found, that not only the surface of some of the laminæ, or grains, had been in fusion, but that part of it was melted in beautiful white spherules, visible to the naked eye.

Another experiment that Mr. N. tried, was on a duck; a chain was fastened to its legs, and, holding it by the wings, the head was brought up to one of the rods of the battery, so that the battery was discharged through it, from the head to the feet: the consequence was, the duck was thrown into violent convulsions, and expired in 2 or 3 minutes. He then took a turkey, and fastened a wire round its neck, and another on its rump, in such manner, that the nearest distance between the wires was along the back bone, thinking the charge of the batteries might pass down the spine, and that the turkey would be made paralytic: but, on discharging the battery, the turkey opened its bill, and died instantly. He then took a cock, and fastened a wire on his rump, and placed one of the balls of the discharging rod on the middle of his back, so that the charge might not pass near his vital parts: the battery being discharged, the body of the cock was violently agitated, for about half a minute, and the head was turned, so that the bill came against its breast; the head and neck however soon recovered, so that it moved its neck, to all appearance, as well as it did before it was struck; but the body was quite motionless, for about 20 minutes; after that it recovered very fast, and, in about 10 minutes more, was able to stand, and walk a little. After this, he put a wire round its neck, in the same manner as on the turkey: the effect was exactly the same; for, on discharging the battery through it, it died instantly. The wire, that conducted the electrical stroke which killed the turkey and cock, was $\frac{1}{25}$ of an inch in diameter.

The next experiment was on some plants. He discharged the battery through a branch of a balsam, and examined it very attentively immediately after it was struck, but could not perceive, there was the least alteration in the branch, till about 10 or 15 minutes after; and then the upper part of the branch began to droop its head, and continued drooping it, till it hung quite straight down, and in 2 or 3 days entirely withered, though the other part of the plant was very vigorous, and did not appear to be in the least affected; this experiment he repeated, several times, on several balsams, as well as many other plants, and always found the same appearances.

From these experiments we find, that electricity, accumulated to a certain degree, puts an end to vegetable as well as animal life. After having recited

these experiments, Mr. N. mentions a caution, which may be of service to future electricians who may use large batteries. It is, never to discharge their batteries, if it is through a ready conductor, unless the charge passes at least 5 feet from the inside of the battery to the outside; by making use of this precaution, which he learnt from experience, he has discharged the battery near 100 times, and never broke a single jar, by the electrical discharge; before which he was continually breaking them, by discharging the battery in the common method.

There is another experiment, which he mentions, as it probably may give some light in respect to balls, or points, for conductors, for buildings or ships: the apparatus and manner of trying the experiments, is as follows: in fig. 1, pl. 10, A represents the end of the large conductor of the electrical machine; B a brass ball screwed into the end of it, of $1\frac{9}{10}$ inch diameter; C a small conductor, which was 5 feet 11 inches long, and $1\frac{4}{10}$ inch diameter; it was made of wood, covered with tin foil, and was insulated, by being supported on a stand, the part D of which was of solid glass. The ball E, at the end of this conductor, was 3 inches diameter, and the ball F $1\frac{9}{10}$ inch diameter; under this ball F, was a stand G, made of wood covered with tin foil, having a moveable part H, which might be raised higher or lower. On the top of this moveable part was screwed, either a pointed wire, or a wire with a ball $\frac{3}{4}$ of an inch diameter, and from the bottom of this stand a chain passed along the floor, till it was connected with the chain, which hung from the cushion: he then placed the conductor C, so that the ball E was 4 inches distance from the ball B; and having screwed into the top of the moveable part H, of the stand G, a pointed wire, he moved it till the point was directly under the ball F, at the distance of 3 or 4 inches; and, on exciting the electrical machine, the fire passed from the ball B, to the ball E, and almost at the same instant struck on the point from the ball F. He increased the distance slowly between the point and the ball F, till he found the utmost distance to which it would strike to the point, which was 6 inches; he continued to move the point to 9 inches distance or more; it then was luminous, and the fire continued to strike from the ball B, to the ball E; which showed that the point carried off all the electrical fire from the conductor C, otherwise it would not continue to strike from B to E. He then removed the point, every thing else remaining as before, and in its stead placed a wire, with a ball of $\frac{3}{4}$ of an inch diameter, at the top of it, at the distance of 3 or 4 inches, directly under the ball F, in the same manner as the point; then, on increasing this distance slowly, the electrical fire was found to strike to the ball at 9 inches, which is half as far again as to the point, and with this remarkable difference, that the quantity of fire was much greater, and

the explosion much stronger and louder, at its striking the ball, than at its striking the point.

It may here be observed, that a point cannot possibly be placed in circumstances more unfavourable than these, to its operation as a point: the body of electric fluid falling on it almost instantaneously, with the stroke from B to E, so that it had scarcely any measurable time, wherein to act as a point, in diminishing the quantity, before the whole fell on it as a conductor. In the use of points to receive and conduct lightning, they generally act on the electrical atmosphere of a cloud, while the cloud is yet at a distance, diminishing gradually that atmosphere, before the cloud approaches near enough to give the stroke, and thus diminishing the stroke, if not quite preventing it. If the small conductor C be placed so as to be in contact with the large conductor A, instead of being 4 inches distant, as before, the electrical fire will not strike to the point at any distance whatever; but the point will carry off silently all the electrical fire from the conductors, as fast as the cylinder supplies them, even if the point is placed at the distance of 10 inches or more.

To this machine there was another large conductor, 12 inches diameter, and 5 feet long, which being applied with its points to the back of the cushion, the machine was either negative or positive, only by hanging a chain on either conductor.

VIII. On the Noxious Quality of the Effluvia of Putrid Marshes. By the Rev. Dr. Priestley to Sir John Pringle. p. 90.

" Since the publication of my papers, (says Dr. P.), I have read 2 treatises, written by Dr. Alexander, of Edinburgh, and am exceedingly pleased with the spirit of philosophical inquiry which they discover. They appear to contain many new, curious, and valuable observations; but one of the conclusions, which he draws from his experiments, I am satisfied, from my own observations, is ill founded, and from the nature of it, must be dangerous. I mean his maintaining, that there is nothing to be apprehended from the neighbourhood of putrid marshes. I was particularly surprized to meet with such an opinion as this, in a book inscribed to yourself, who have so clearly explained the great mischief of such a situation, in your excellent treatise on the diseases of the army. On this account I have thought it not improper, to address to you the following observations and experiments, which I think clearly demonstrate the fallacy of Dr. Alexander's reasoning, indisputably establish your doctrine, and indeed justify the apprehensions of all mankind in this case.

I think it probable enough, that putrid matter, as Dr. Alexander has endeavoured to prove, will preserve other substances from putrefaction; because, being

already saturated with the putrid effluvium, they cannot readily take any more; but Dr. Alexander was not aware, that air thus loaded with putrid effluvium, is exceedingly noxious when taken into the lungs. I have lately however had an opportunity of fully ascertaining how very noxious such air is.

Happening to use at Calne, a much larger trough of water, for the purpose of my experiments, than I had done at Leeds, and not having fresh water so near at hand as I had there, I neglected to change it, till it turned black, and became offensive, but by no means to such a degree as to deter me from making use of it. In this state of the water, I observed bubbles of air to rise from it, and especially in one place, to which some shelves, that I had in it, directed them; and having set an inverted glass vessel to catch them, in a few days I collected a considerable quantity of this air, which issued spontaneously from the putrid water; and, putting nitrous air to it, I found that no change of colour or diminution ensued; so that it must have been, in the highest degree, noxious. I repeated the same experiment several times afterwards, and always with the same result.

After this, I had the curiosity to try how wholesome air would be affected by agitation in this water; when, to my real surprize, I found, that after one minute only, a candle would not burn in it; and, after 3 or 4 minutes, it was in the same state with the air which had issued spontaneously from the same water. I also found, that common air, confined in a glass vessel, in contact only with this water, and without any agitation, would not admit a candle to burn in it after 2 days.

These facts certainly demonstrate, that air, which either arises from stagnant and putrid water, or which has been for some time in contact with it, must be very unfit for respiration; and yet Dr. Alexander's opinion is rendered so plausible by his experiments, that it is very possible that many persons may be rendered secure, and thoughtless of danger, in a situation in which they must necessarily breathe it. On this account, I have thought it right to make this communication as early as I conveniently could; and as Dr. Alexander appears to be an ingenuous and benevolent man, I doubt not but he will thank me for it.

That air issuing from water, or rather from the soft earth, or mud, at the bottom of pits containing water, is not always unwholesome, I have also had an opportunity of ascertaining. Taking a walk, about 2 years ago, in the neighbourhood of Wakefield, in Yorkshire, I observed bubbles of air to arise, in remarkably great plenty, from a small pool of water, which, on inquiry, I was informed had been the place where some persons had been boring the ground, in order to find coal. These bubbles of air having excited my curiosity, I presently returned, with a basin, and other vessels proper for my purpose, and having stirred the mud with a long stick, I soon got about a pint of this air; and,

examining it, found it to be good common air; at least a candle burned in it very well. I had not then discovered the method of ascertaining the goodness of common air, by a mixture of nitrous air. Previous to the trial, I had suspected that this air would have been found to be inflammable.

I shall conclude this letter with observing, that I have found a remarkable difference in different kinds of water, with respect to their effect on common air agitated in them, and which I am not yet able to account for. If I agitate common air in the water of a deep well, near my house in Calne, which is hard, but clear and sweet, a candle will not burn in it after 3 minutes. The same is the case with the rain water which I get from the roof of my house. But in distilled water, or the water of a spring well near the house, I must agitate the air about 20 minutes, before it will be so much injured. It may be worth while to make further experiments, with respect to this property of water.

In consequence of using the rain water, and the well water abovementioned, I was very near concluding, contrary to what I have asserted in my printed papers, that common air suffers a decomposition by great rarefaction. For when I had collected a considerable quantity of air, which had been rarefied about 400 times, by an excellent pump made for me by Mr. Smeaton, I always found, that when I filled my receivers with the water above-mentioned, though I did it so gradually as to occasion as little agitation as possible, a candle would not burn in the air that remained in them. But when I used distilled water, or fresh spring water, I undeceived myself.

"P. S. I cannot help expressing my surprize, that so clear and intelligible an account, of Mr. Smeaton's air pump, should have been before the public so long, as ever since the publication of the 47th vol. of the Philos. Trans., and yet that none of our philosophical instrument makers should attempt the construction. The superiority of this pump, to any that are made on the common plan, is indeed prodigious. Few of them will rarefy more than 100 times, and in a general way not more than 60 or 70 times; whereas this instrument must be in a poor state indeed, if it do not rarefy 200 or 300 times; and when in good order, it will go as far as 1000 times, and sometimes even much farther than that; besides, this instrument is worked with much more ease, than a common air pump, and either exhausts or condenses at pleasure. In short, to a person engaged in philosophical pursuits, this instrument is an invaluable acquisition. I shall have occasion to recite some experiments, which I could not have made, and which indeed I should hardly have dared to attempt, if I had not been possessed of such an air pump as this. It is much to be wished, that some person of spirit in the trade would attempt the construction of an instrument, which would do great credit to himself, as well as be of eminent service to philosophy."

IX. Further Proofs of the Insalubrity of Marshy Situations. In a Letter from the Rev. Dr. Price. p. 96.

"Dr. Priestley's paper, on the noxious effects of stagnant waters, read to the R. S., brought to my remembrance, (says Dr. Price), a table, exhibiting the rate of mortality in a parish situated among marshes, which I have seen in Mr. Muret's Observations, published in the Memoirs of the Economical Society at Bern, for 1766. I have since reviewed this table, and found that it affords a full confirmation of Dr. Priestley's assertions. This parish is a part of the district of Vaud, belonging to the canton of Bern, in Switzerland; and contains 169 families, and 696 inhabitants. Mr. Muret's table, of the rate of mortality in it, is formed from a register of the ages at which all died in it for 15 years. With this table he has also given tables, from like registers, of the rates of mortality in 7 small towns; in 36 country parishes and villages; in 16 parishes situated in the Alps; in 12 corn parishes, and in 18 vintage parishes.—From comparing these tables, it appears, that the probabilities of life are highest in the most hilly parts of the province, and lowest in the marshy parish just mentioned. The difference is indeed remarkable, as will appear from the following particulars. One half, of all born in the mountains, live to the age of 47. In the marshy parish, one half live only to the age of 25. In the hills, 1 in 20, of all that are born, live to 80. In the marshy parish, only 1 in 52 reaches this age. In the hills, a person aged 40 has a chance, of 80 to 1, for living a year. In the marshy parish, his chance for living a year is not 30 to 1. In the hills, persons aged 20, 30, and 40, have an even chance for living 41, 33, and 25 years respectively. In the fenny parish, persons, at these ages, have an even chance of living only 30, 23, and 15 years.—In short, it appears, that, though the probabilities of life, in all this country except this one parish, are much higher than in London; yet here, after 30, they are much lower. Before the age of 30, they are indeed higher in this parish; the reason of which must be, that the London air and customs are particularly noxious to children.*

I am sensible, that observations for only 15 years, in one small parish, do not afford so decisive and ample an authority, in the present case, as there is reason to wish for; and that therefore the perfect exactness, of the particulars I have recited, cannot be depended on.—They are, however, sufficiently near the truth to demonstrate, in general, the unhealthfulness of a marshy situation."

X. Of the Culture and Uses of the Son or Sun plant of Hindostan,† *with an*

* In London, one half of all that are born, die under 3 years of age. But this is not peculiar to London. In Berlin the same proportion dies under 3; and at Vienna under 2.—Orig.

† This plant is described by Linnæus, under the name of *Crotalaria juncea,* vid. Spec. Plant.

Account of the Manner of Manufacturing the Hindostan Paper. By Lieut. Col. Ironside. p. 99.

This useful plant, Lieut. Col. I. believes, is cultivated all over Hindostan. The seeds are sown in July, before the rains begin; they should be sown near to one another, to make the stem rise higher, more erect, with fewer branches, and to increase the produce. It flowers in October, and is taken up in December. The black ladies use the seeds, reduced to powder and mixed with oil, for their hair, on a supposition that this composition will make their hair grow to a great length, which they are very fond of. From the bark are made all kinds of rope, packing cloths, nets, &c. and from these, when old, most of the paper, in this country, is prepared; for these purposes, the fresh plant is steeped 4 days in water, afterwards tried, and treated as the cannabis for hemp, to which it is so similar when prepared, that Europeans generally suppose it to be the produce of the same plant.

As the substances, producing cloths, ropes, and paper, are few in present use, this plant may perhaps be cultivated with advantage, in some of the British West-India settlements, and in other countries destitute of hemp and flax. It is not improbable, that it may be raised in the warmer climates of Europe, as it ripens here in winter. He could not say, what soils it might refuse; where he had seen it, in the greatest plenty and perfection, had generally been on an earth composed of clay, calcarious grit, and sand. There are other vegetable substances used in Hindostan for the purpose of rope making; one of them is a species of the hibiscus, a description of which he purposed for the subject of another paper: he could scarcely doubt, but that it is only for want of experiments, we had not a greater number of vegetables rendered useful in this manner. The class monadelphia, of Linnæus, promises fair for trials of this kind.

The Hindostan Method of manufacturing Paper.

The manufacturer purchases old ropes, clothes, and nets, made from the sun plant, and cuts them into small pieces, macerates them in water, for a few days, generally 5, washes them in the river in a basket, and throws them into a jar of water lodged in the ground; the water is strongly impregnated with a lixivium of sedgi mutti* 6 parts and quick lime 7 parts. After remaining in this state 8 or 10 days, they are again washed, and while wet, broken into fibres, by a stamping lever, and then exposed to the sun, on a clean terrace, built for this purpose; after which, they are again steeped, in a fresh lixivium, as before.

1004. A figure of it is given by Ehret in Trew's Plant. Select. t. 47; and another in the Hort. Malab. 9, p. 47, t. 26. Both these figures are good.—Orig.

* Sedgi Mutti is an earth, containing a large portion of fossil alkali. The νατρον of the antients. It is found in great plenty in this country, and universally used in washing, bleaching, soap-making, and for various other purposes.—Orig.

When they have undergone 3 operations of this kind, they are fit for making coarse brown paper; after 7 or 8 operations, they are prepared for making paper, of a tolerable whiteness.

The rags, thus prepared, are mixed with water in a cistern, to proper consistence; after which it is taken up by a wire frame, to form it into sheets of paper, &c. just in the manner practised in England.

XI. An Improvement proposed in the Cross Wires of Telescopes. By Dr. Wilson, of Glasgow. p. 105.

It has been hitherto a desideratum to draw silver wire fine enough for astronomical uses. The means fallen on by Dr. W. of obviating the difficulty, in practice, is extremely simple, and consists in nothing but in flattening the finest wires, which are now drawn. He made the experiment on silver wire, which is marked 500 to the inch. Having prepared a small block of steel, the face of which was made very flat and smooth, a number of the wires were stretched across it, at considerable intervals, by having their ends fastened, by pitch, at each side of the block. This done, he took another block of steel, of the same size, the face of which had been made likewise flat, and the top of it rounded, the better to determine the stroke of the hammer; on applying this, over the wires lying on the first block, which was firmly fixed in a vice, and giving a smart stroke with a hammer of about 5 pounds weight, he found all of them flattened in a very even manner.

That he might have no difficulty of fitting these wires, so flattened, into the telescope, he purposely made the face of the steel blocks a little narrower than the width of the brass ring, in our transit instrument, on which the cross wires are fixed. By this means the wires retained their roundness at both ends, and so were easily fixed across the ring, by the screw pins, when their fine edges regarded the eye. By means also of a simple contrivance, which will readily occur in practice, he made the horizontal wire to go across the others, so as just to touch them. This horizontal wire was a round one, of 500 to the inch, which he purposely used along with the others, that he might form some judgment of the effects of flattened ones, when viewed along with it in the field. He accordingly found a very striking diminution of the visible subtense of these wires, when compared with the round one; and this so considerable, as could not be obtained with round wires, unless they could be drawn to 2 or 3 thousand to the inch.

XII. The Case of a Patient voiding Stones through a Fistulous Sore in the Loins, without any concomitant Discharge of Urine by the same Passage. By Mr. S. F. Simmons. p. 108.

This subject, Eleanor Pilcher, was about 52 years of age. About 25 years before she first began to complain of pain in her back, of a difficulty in making water, and of other nephritic symptoms, which gradually increased. Soon after this she began to void gravel with her urine, and to pass several very small stones; and these symptoms continued to return very frequently, and with much severity. About 10 years after the first appearance of these complaints, a swelling came on in the left lumbar region, which, after having been very painful, for a considerable time, suppurated. This wound, which very soon became fistulous, continued open ever after, and constantly afforded an ichorous discharge. It was not till Dec. 1772, 15 years from the appearance of the tumour, that this discharge began to abate, and that the wound, from being perfectly easy, became painful and inflamed. During all this time, the nephritic symptoms had continued to return, without any variation, the urine had constantly afforded a gravelly sediment, and several small stones had passed through the meatus urinarius; but these concretions were now about to take a different course. The pain in the back, which had commonly affected the left side, became much more intense than usual, but was not attended by any of the other symptoms, which had been the usual forerunners of a fit of the gravel. The discharge, from the wound, was suddenly diminished, and the pain and inflammation exceedingly increased, though the urine continued to pass in a healthy quantity, and without difficulty. These complaints continued during 8 days, and then a round and smooth calculus, weighing about 12 grs., was extracted, with some difficulty, from the wound. After that time no gravel was voided with the urine, though no urine ever passed through the wound; and 6 other paroxysms, like that he had described, took place, in which the same symptoms occurred, and which had terminated in a similar manner, so that 7 calculi had passed through the wound, only 2 of which had been preserved, and the least of them weighed 6 grs. During the intervals of these paroxysms, the patient enjoyed a state of ease and health; and the orifice of the wound, soon after the exclusion of a calculus, returned to its usual size, admitting with difficulty a common probe. This case appeared to be a great proof of the powers of nature. The right kidney did not seem to be affected, and as no urine ever passed through the wound, it should seem as if the secretion by the left kidney was destroyed; for, as no gravel was then voided with the urine, the left ureter was probably closed.

The case however, though a very interesting one, is not perfectly singular, for Delecamphius relates, that he saw a man who passed several stones through an abscess of the loins, that had become fistulous. And Tulpius, in the 4th book of his Observationes Medicæ, gives the history of a patient, who after undergoing much pain, from a nephritic complaint which he inherited from his father, at length passed a stone from the kidneys, externally through the

loins, which occasioned a callous ulcer, through which pus and urine were perpetually flowing. Neither time, nor any of the remedies employed, afforded him any relief; but the passage through the loins closing, and the matter taking a different course, an acute fever was at length brought on, of which the patient died. And the late Mr. Cheselden observes, that he had 3 patients from whom he had extracted small stones, which had made their way from the kidnies to the integuments, and there occasioned an imposthumation. But cases like these, though not perfectly new, seem to deserve to be recorded, as very rare ones, especially when they afford more interesting circumstances than seem hitherto to have occurred.

XIII. The Disparition of Saturn's Ring, observed by Joseph Varelaz, Lieut. of the Royal Navy of the King of Spain, and Prof. of Mathematics, &c. Translated from the Spanish. p. 112.

It has been my luck to observe the celebrated phenomenon of the ring of Saturn, which was so much recommended to astronomers, in the gazette of France of July 23. From the 24th of September to the 4th of October, I saw clearly and distinctly the two ansæ of the ring; but with this particular circumstance, that the occidental ansa appeared more strongly illuminated than the oriental. The atmosphere was thicker on the 5th, and I could only see the occidental. The 6th, I thought I could discern some faint remains of the ring; but that might be a deception of my sight, because the atmosphere remained very thick, and the planet could not be seen well terminated. On the 7th, the atmosphere being more transparent, and the heavens clearer than I have ever seen them, I observed the total disparition of the ring; and, having repeated the same observation the following day, I was convinced that this famous phenomenon took place the 6th of the month, in which determination I have all the exactness which can be expected in observations of this kind. The most striking circumstances of this phenomenon were the following; 1. The occidental ansa constantly appeared more bright than the oriental. 2. On the disc of the planet, one could clearly distinguish the line of the shadow projected from the thickness of the ring. 3. On the extremities of this, some luminous points were perceived, which reflected the light more strongly than the others. 4. I did not observe a sensible variation in the apparent diameter of the ring.

XIV. Of the Gillaroo Trout. By the Hon. Daines Barrington. p. 116.

"You will find on the table a Gillaroo trout, as it is termed in Ireland, the peculiarity of which is, that the stomach very much resembles the gizzard of a bird. Since that time I have endeavoured to procure a specimen, with the entrails adhering, and have at last succeeded, the stomach on the table having

been extracted by Mr. Hunter. I do not find that any ichthyologist takes notice of such a part belonging to fish, except Gouan, who says, that the ventricle of some sorts resembles the gizzard of fowls, by being partly fleshy and partly membranous: Gouan however does not specify the species of fish, which hath such a stomach. The poke of the Gillaroo seems to perform the office of a gizzard, because several small snails were found within the present specimen, and I conclude, that this species of food abounds in the lakes which this variety of trout frequents.

By the best information I can procure, they are more common in Lough Corryb, and the lakes of Galway, than the other waters of Ireland: they are also caught in Lough Dern, through which the Shannon runs."

XV. Account of the Stomach of the Gillaroo Trout. By Mr. H. Watson. p. 121.

For a more ample dissertation on this subject, with respect to comparative anatomy, the reader is referred to the paper of Mr. Hunter.

XVI. A Description of a Petrified Stratum, formed from the Waters of Matlock, in Derbyshire. By Matthew Dobson, M.D. p. 124.

During a short stay at Matlock, this summer, Dr. D. made some observations on the petrifying quality of the waters, and examined a very singular stratum, which has been formed in their course. This stratum he found about 500 yards in length; in several places near 100 yards in breadth; and where thickest from 3 to 4 yards in depth. The manner in which this body of stone has been produced is easily ascertained. Within the memory of some persons now living, the waters of Matlock were not appropriated to the purposes either of bathing or drinking. They issued from near the bottom of the hill which lies to the west immediately behind the present houses, and ran at random down a declivity of about 100 yards, to the river Derwent. In their course they formed large petrified masses, intermingled with great quantities of petrified moss, nuts, leaves, acorns, pieces of wood, and even trunks of trees.

The waters were thus constantly raising obstacles to their own progress, and were frequently therefore forced into new channels; so as by degrees to be extended over a surface of at least 500 yards in length. And by being repeatedly returned into the same channels, a stratum of considerable thickness has been formed. On examining this stratum, some parts are discovered to be extremely hard, and others so soft as easily to be cut. The soft parts however, on exposure to the air, become as hard as flint; and on being struck sound like metal. The reason of this difference in the hardness of different parts, appears to be this: as the waters frequently changed their channels, and repeatedly likewise returned again to the same channels, if, in the intervals, there were any parts considerably

raised, and consequently longer before they were covered with fresh incrustations, these, from a long exposure to the air, would acquire a greater degree of hardness.

Whole houses in the neighbourhood are built of this stone, which they find more durable than any other they meet with; and as it has the excellent property of growing harder, from being exposed, and has likewise many little cavities and interstices, good mortar so insinuates itself into these, as to form a wall as firm as one continued stone. This stratum affords curious and beautifully varied petrifactions. Moss exhibits great varieties; for it is evident, that the moss has continued to vegetate, after the roots and lower parts had been penetrated by the stony particles; and thus, stretching itself to a considerable extent, it has in some places been mixed and interwoven with other substances. In some parts snails have been arrested in their sluggish walks, and locked up in the stony concrete. In others, the petrifying matter has shot, in different directions, and formed an intricate kind of net-work. And in others again, there are large masses, which on being broken asunder, are found hollow; and their cavities ornamented with branches of petrifaction, somewhat resembling coral, but of a darkish white colour, and generally of a rough and granulated surface.

Under the stratum there is, from a foot to a foot and a half, of good soil; and immediately under this lies the limestone rock. The soil is of the same nature with that of the adjoining fields, which form the slope of the hill, and is evidently a continuation of that soil. Any further additions, to this petrified stratum, are now inconsiderable, and in many places none at all; for the two principal springs are confined to their channels, covered from the day, through the greatest part of their course, and are rapid in their motion.

Had proper observations been made on the progress of this stratum, a tolerably exact estimate might have been formed with respect to the time when these waters were first impregnated with their mineral ingredients. From these 2 considerations however, that the stratum is not very thick, and that the soil immediately under it is a continuation of that which lies on the slope of the neighbouring hills, it is probable that many centuries have not been requisite to its production; and consequently that these mineral waters are not of very ancient date.

And if we may rely on an observation, which he had from a plain, inquisitive, and intelligent man on the spot, the source whence these waters derive their impregnation, is in some degree exhausted. This person assured him, from his own experience, that pieces of moss, and other substances, put in the course of the waters, and in the same circumstances as formerly, require more than double the time for their petrifaction that they did 30 years ago. The stratum therefore, from which the Matlock waters are impregnated, must either be consi-

derably exhausted; or the waters have deviated from their former course, and are now only partially distributed over this stratum.

XVII. Remarks on the Aurora Borealis. By Mr. Winn. p. 128.

I believe the observation is new, that the aurora borealis is constantly succeeded by hard southerly, or south-west winds, attended with hazy weather and small rain. I think I am warranted from experience to say constantly; for in 23 instances that have occurred since I first made the observation, it has invariably obtained.

The gale generally commences between 24 and 30 hours after the first appearance of the aurora. More time and observation will probably discover whether the strength of the succeeding gale is proportionate to the splendor and vivacity of the aurora, and the distance of time between them. I only suspect that the more brilliant and active the first is, the sooner will the latter occur, be more violent, but of shorter duration, than when the light is languid and dull.

XVIII. Experiments concerning the Different Efficacy of Pointed and Blunted Rods, in Securing Buildings against the Stroke of Lightning. By W. Henley, F.R.S. p. 133.

From an accident which lately happened to the chapel in Tottenham-court road, where a poor man was killed, the gentlemen who have the care of that building were desirous of erecting a proper conductor to prevent such accidents in future; which was done accordingly under Mr. H.'s direction, except 3 points at the top, to which he rather inclined to prefer a single one. On this occasion, he was willing to obtain the best information he could, on the question, whether the preference be due to points or knobs, for the termination of conductors; for which purpose he made the following experiments.

Exp. 1. He placed 2 of Mr. Canton's electrometers, A and B, pl. 10, fig. 2, insulated, on stands of sealing-wax, about 7 inches asunder, and as many from the end of a prime conductor, which was 18 inches long, and $1\frac{1}{2}$ inch in diameter; and had a ball at each end, $2\frac{1}{2}$ inches diameter; the diameter of the electrical globe being 9 inches. On the top of the box A, was placed a wire, projecting 3 inches from the end of it, and terminated by a ball $\frac{3}{4}$ inch in diameter. On the top of the box B, was placed a sharp-pointed wire, projecting also 3 inches from its end. The knob and point were now exactly at the same distance, namely, 7 inches from the end of the conductor. Then, giving the winch 5 or 6 turns, the light cork balls, hanging from the box A, were repelled to the distance of 1 inch from each other; but those hanging from the box B, separated full 2 inches. Then touching the prime conductor with a finger, the balls at A closed, while those at

B remained a full inch asunder. From this experiment, it seems evident how much better adapted a sharp point is to draw off lightning, than a knob of $\frac{3}{4}$ inch in diameter; and consequently how much more likely to cause it to pass in that conductor, to which it is affixed, rather than in any other part of the building, where it might occasion much damage, as well as endanger the lives of those who might happen to be in it. The following experiments seem to make still more strongly in favour of the same conclusion.

Exp. 2. He affixed to the top of a glass-stand a wire, $\frac{3}{8}$ inch in diameter, terminated at one end by a ball, $\frac{3}{4}$ inch in diameter; and at the other end by a very sharp point; see fig. 3. Round the middle of this wire was hung a chain 12 inches long. He then charged a bottle, containing 100 square inches of coated surface, and connecting the chain with the coating of the bottle, brought the knob of it very gently towards the ball on the insulated wire, that he might observe precisely at what distance it would be discharged on it; which he found to happen constantly at the distance of half an inch, with a loud and full explosion. Then re-charging the bottle, he brought the knob, in the same gradual manner, towards the point of the insulated wire, to try also at what distance that would be struck; but this in many trials never happened at all. The point, being approached in this gradual manner, always drew off the charge imperceptibly, leaving scarcely a spark in the bottle.

Exp. 3. Mr. H. had now recourse to the apparatus known to electricians by the name of the thunder-house, which he thought a nearer resemblance of the operations of nature on these occasions. Having connected a jar, containing 509 square inches of coated surface, with the prime conductor, see fig. 4; he observed, that if it was so charged as to raise the index of the electrometer to 60 degrees, by bringing the ball on the wire of the thunder-house to half an inch distance from that connected with the prime conductor, the jar would be discharged, and the piece in the thunder-house thrown out to a considerable distance. Using a pointed wire for a conductor to the thunder-house, instead of the knob, as in the former experiment, the charge being the same, the jar was discharged silently, though suddenly: and the piece was not thrown out of the thunder-house.

Exp. 4. Having made a double circuit to the thunder-house, fig. 5, the 1st by the knob, the 2d by a sharp-pointed wire, at $1\frac{1}{4}$ inch distance from each other, but of exactly the same height, the charge being the same; though the knob was brought first under that connected with the prime conductor, which was raised half an inch above it, and followed by the point, at $1\frac{1}{4}$ inch distance, yet no explosion could fall on the knob; the point drew off all the charge silently; and the piece in the thunder-house remained unmoved.

Exp. 5. Having insulated the jar, and connected, by chains, with the external

coating, on one side a knob, and on the other side a sharp-pointed wire, both being insulated, and standing 5 inches from each other, fig. 6, he placed a large copper ball, C, 8 inches in diameter (insulated also) so as to stand exactly at half an inch distance both from the knob and the point. The jar being fully charged, he delivered it on the copper ball by the discharging rod, whence it leaped to the knob A, which was $\frac{3}{4}$ inch in diameter, and the jar was discharged by a loud and full explosion, and the chain was very luminous. He could perceive no light on the chain, which connected the pointed wire B with the coating of the jar.

Exp. 6. Mr. H. insulated his 3 largest jars, containing together about 16 square feet of coated surface; fig. 7. From the bottom of these jars projected a wire, terminated by a ball, $\frac{3}{4}$ inch in diameter; and at the distance of $1\frac{1}{2}$ inch from it, he placed the insulated ball C; on which he brought down the charge of the 3 jars, by the discharging rod; which leaped from it to the ball in contact with the jars, and discharged them by a loud and full explosion; but the same thing did not happen if he removed the insulated ball only $\frac{1}{8}$ of an inch farther from the other. He then removed the wire, which was terminated by the ball, from the bottom of the jars; and placed another in its stead, of the same length and diameter, but very nicely tapered to a point, as usual. Then placing the insulated ball C one inch from the point, he brought down the charge of the 3 jars, as before, which flew upon the point, and melted it a little. The jars were discharged with a loud and full explosion. But having removed the ball C to the distance of $1\frac{1}{8}$ inch from the point, the charge could not strike it; though much of it was presently drawn off silently by the point, as appeared by the falling of the index of the electrometer.

From this experiment, he thinks it seems somewhat more than probable, that a conductor terminated by a ball, of $\frac{3}{4}$ inch in diameter, would be in danger of a stroke from a highly electrified cloud, at a much greater distance than another with a sharp termination. Indeed he cannot help remarking, how very improbable it appears, that a sharp-pointed conductor should at any time invite or solicit a stroke of lightning. Imagine, if you please, that a large cloud is, by the force of the wind, driven violently towards such a point, and actually strikes on it: yet as the point would act as such, at somewhat more than the striking distance, it seems probable that part of the electricity of the cloud would be drawn off silently, before the actual stroke could be made; and the stroke itself might thereby perhaps be a little lessened.

Mr. H. here inserts what seems to afford a sufficient proof of the truth of this reasoning; viz.

Extract of a Letter from Capt. Richard Nairne, of the Generous Friends, dated Montreal, June 24, 1773, to Mr. Thos. Marsham, in the Borough.

'I shall make every observation I can for the good of electricity, and the sa-

tisfaction of my friend Mr. Henley. I put up a longer top-gallant mast the day I arrived at Quebec. The conductor by this means became too short; and my mate still let it hang, without making any addition to it. There was a severe thunder storm that night; but think how pleased I was to find that, from the wetness of the ship's sides, the electricity passed into the water, without the least injury to the ship; but the spark on the point of the conductor, which was very sharp, was so lucid, that my people were very much frightened.'

Since receiving this account of Mr. Nairne's observation, I have been favoured, says Mr. H., with the following remark, by my ingenious and worthy friend, Lieut. Fairlamb, of the artillery; who informs me, that the church of St. Michael, in Charlestown, South Carolina, used to be struck and damaged by lightning, in every 2 or 3 years from its first erection; but in 14 years, that it has been furnished with a pointed conductor, it has never been struck at all. It appears also, that when a stroke of lightning fell on a stable belonging to Wm. Lyttleton, Esq., Governor of South Carolina, and split and threw down 2 of the rafters; yet the dwelling-house, at 20 yards distance, being provided with a conductor, terminated by a sharp point, escaped unhurt. I would here also just remark, that nothing can be more sharply pointed than the weather-fane which terminates the conductor, erected by Mr. Edward Nairne, on one of the pinnacles on the tower of St. Michael's church in Cornhill, which consists of two darts, with a star, having many pointed radii between them; yet in the late thunder-storm, it does not appear that the lightning struck this building; but fell on the key at the top of the spire of St. Peter's church, which is considerably lower than the fane of St. Michael's; and the distance of the two churches is not more than 200 feet. This key is terminated by a thick blunted end: the spire is covered with lead, from the key to the brick tower; and so far the lightning was conducted with safety to the building: nor could I observe, that there had been the least fusion on the metal; but having quitted the lead-work, and entered the brick tower, it there did considerable damage, till it reached the leaded roof on the body of the church; whence it seems to have been conducted by the pipes which carry down the rain water, and reach to the bottom of the building, without further damage. Almost at the same instant that this spire was struck, the lightning fell also on a Dutch ship, in the river Thames, lying off the Tower, which had an iron spindle, terminated by a thick blunted end, at her mast-head, and did her much damage. The lightning struck also on the pillar commonly called the obelisk, in the cross road in St. George's fields, Southwark. It likewise struck the chimney of the new Bridewell there, which it threw down to the ridge of that building, which was covered with lead; and then dispersed itself with little damage. The lightning fell also on another chimney at Lambeth; and on a house at the physic-garden near Vauxhall; and,

as before observed, it appears by the best information, nearly at the same time; and in many other places, considerably distant from each other.

I have observed, on another occasion, that if a round ball of metal, 2 inches in diameter, was presented towards the large prime conductor to a good cylinder, at the distance of 2 inches, it would continue to receive such strong sparks, as would give the person who held it a sensible shock in both his legs; but that if the point of a lancet, or a wire 6 inches long, nicely tapered to a point, and tipped with steel, were at the same time held towards the conductor, at the distance of 2 feet, the point would draw off all its electricity silently, and not suffer a spark to pass from it to the ball; and from this experiment I inferred, that a sharp point might probably, in some measure, produce the same effect on a cloud highly charged with electricity, or rather on the electric atmosphere surrounding the cloud; and thus perhaps contribute to lessen a little, if not actually prevent a stroke. I also observed, that if the point of the wire or lancet, was brought nearly into contact with the prime conductor; yet no sensation would be felt in the hand of the operator; and this I imagined was a kind of demonstration, that there could be no danger of inviting a stroke of lightning from a cloud by a sharp-pointed conductor; as it could make no difference in the experiment, whether the point moved towards the large prime conductor, or the conductor moved towards the point. It having however been objected to this experiment, that it was not analogous to the effect of nature operating by a cloud; forasmuch as the cloud being a loose and floating body, it might accede to, and strike on the point with its contents; which the conductor, being a fixed body, was incapable of doing, I made the following experiment.

Exp. 7. I procured a bullock's bladder, of the largest size, gilded with leaf copper, and suspended it, by a silken string, at one end of an arm of wood, placed horizontally, and turning freely on the point of a needle; the needle being stuck upright in another piece of wood, inserted in a firm base, and standing in a perpendicular direction to the floor. The bladder was balanced by a leaden weight, at the other end of the wooden arm, as in fig. 8. The apparatus being thus adjusted, I gave the bladder a strong spark from a knob of a charged bottle; when, presenting towards it a brass rod, terminated by a ball, 2 inches in diameter, I observed, that the bladder would come towards it at the distance of 3 inches; it would even come back to it, when swinging in a contrary direction; and when it had got within 1 inch of it, it would throw off its electricity in a full and strong spark: the bladder gave the spark nearly, if not quite, as large as it received it. I then gave it another strong spark as before, when, presenting towards it the pointed wire, I could never perceive that it acceded to that; and when it was brought nearly into contact with the bladder, there was no spark at all, scarcely any sensible quantity of electricity remaining

in it. I repeated the experiments many times, and always with the very same result.

Mr. H. then relates an experiment by Tho. Ronayne, Esq., to the same effect, whence this gentleman draws the following inference: "Now as bodies act at a greater distance, by how much they are more acute, and thereby diminish any known electrical force; and, as in any particular case, the smallness of the pencil, or stroke, depends on the acuteness of the point presented, I cannot avoid giving my suffrage for points, in preference to obtuse bodies."

It may not, says Mr. H., be improper to introduce, in this place, an experiment lately made by Mr. Edward Nairne, in Cornhill; which, though it does not immediately relate to the particular subject of this paper, is a very proper one to demonstrate the utility of metallic conductors in general.

Mr. Nairne's Experiment.—He affixes, in a little apparatus, resembling the hulk of a ship, a glass tube, about 8 inches long, and half an inch in diameter, to represent the main-mast. The ends of the tube, which is filled with water, are properly secured by corks; and through each cork a wire is introduced, of such a length as to reach nearly to the middle of the tube, and leave a distance of about half an inch, between the ends of the two: as in a curious experiment of Mr. Lane's, made with small phials. A slight shock, discharged through this apparatus, instantly breaks the tube in pieces, at that part, where the electric matter quits the upper wire, and expands itself in the water, before it reaches the lower one; as the natural electricity has been observed to do in bodies wherein it has met with such an interrupted and broken communication of metal; but Mr. Nairne having fixed, at the top of such a glass tube, and united with the wire of it, a piece of very small harpsichord wire, which was continued to the bottom of it, and there fastened to a regular communication of metal, in contact with the coating of the jars; he discharged through it his 4 batteries united, consisting of 64 jars, containing 50 square feet of coated surface fully charged, when the whole of the small wire was instantly exploded and lost; but the tube remained unhurt: An effect analogous to that of the natural electricity, where, though it has sometimes happened that the conductor, being too small, has been in part destroyed, or much injured by a stroke; yet the building, to which such a conductor has been affixed, has escaped, without receiving the least damage.

Among some very interesting remarks on the effects of lightning, by Professor Winthrop of New Cambridge, which have lately been communicated to me by Dr. Franklin, I find one, on the influence of sharp pointed conductors, so immediately relating to the question under consideration, that no apology will be necessary for introducing it in this place. Dr. Winthrop having given a very curious and exact account of a violent flash of lightning, which fell on and

greatly damaged Hollis-hall, in New Cambridge, observes, that Harvard-hall, being furnished with pointed wires, which wires were at the distance of 160 feet from the chimney of Hollis-hall, on which the lightning fell, escaped unhurt, though the wires were seen by many to transmit a large quantity of it, which left visible marks on the bricks, where the wires hooked together. This gentleman also observes, that a tree, standing at the distance of 52 feet from a pointed wire, erected on the steeple of a meeting-house, as a conductor for the lightning, had been struck and shivered; but that the meeting-house remained uninjured; and this, he says, is the least distance from such a conductor, so far as he knew, at which any thing had been struck by lightning. It appears therefore very clearly, from these instances, that sharp pointed wires, instead of inviting, and drawing down strokes of lightning, serve rather to prevent them, and that they extend their protecting influence to some distance around them, and ought therefore ever to be used, as the termination of the rods erected on houses, steeples, magazines, masts of ships, &c. in short, on all occasions, where conductors for the lightning may be thought necessary.

I have made many other experiments, says Mr. H., on the different effect of knobs or points, as opposed to insulated electrified bodies; but as they all concur in establishing and confirming the opinions before advanced, it seems unnecessary to mention them; and the more so, as I believe those already recited will be deemed sufficiently decisive without them.

XIX. Remarks on a Passage in Castillione's Life of Sir Isaac Newton. By John Winthrop. LL.D., at Cambridge, in New England. p. 153.

There is a passage in Castillione's life of Sir Isaac Newton, prefixed to his edition of the Opuscula, in 3 volumes, 4to., published at Lausanne and Geneva in 1744, which appears to be a palpable mistake; and tends to place Sir Isaac Newton in an inferior light to Descartes, in the eyes of foreigners. It is this, p. 32: " Sæpiùs se reprehendebat (Neutonus) quòd res merè geometricas algebraïcis rationibus tractavisset, et quòd libro suo de Algebrâ Arithmeticæ Universalis titulum posuisset, meliùs asserens Cartesium suum de re eâdem volumen dixisse Geometriam, ut sic ostenderet has computationes subsidia tantùm esse geometris ad inveniendùm." The authority he quotes for this, is Dr. Pemberton, in the preface to his View of Sir Isaac Newton's Philosophy; but I will venture to say, he has misinterpreted his author. He represents Dr. Pemberton as saying, 1st. That Sir Isaac Newton often censured himself for handling geometrical subjects by algebraic calculations. 2dly. That another thing he often censured himself for, was, his having called his book of algebra by the name of Universal Arithmetic. 3dly. That he commended Descartes, as having done better, in giving the title of Geometry to his treatise on the same subject. The

last two particulars, certainly, and I think the first also, have no foundation in the account Dr. Pemberton has given of this matter. His words are: "I have often heard him (Sir Isaac) censure the handling geometrical subjects by algebraic calculations; and his book of algebra he called by the name of Universal Arithmetic, in opposition to the injudicious title of geometry, which Descartes had given to the treatise, wherein he shows, how the geometer may assist his invention by such kind of computations."—Dr. Pemberton's expression does not at all imply, that Sir Isaac Newton censured himself for handling geometrical subjects by algebraic calculations; the only idea it suggests is, that he censured that way in general, and those who practised it, and that he had his eye particularly on Descartes; and, far from intimating, that he had inconsiderately called his book of algebra by the name of Universal Arithmetic, and afterwards censured himself for doing so, and wished that he had rather called it Geometry, as Descartes did his; it directly affirms, on the contrary, that by express design and choice, he called it Arithmetic, in opposition to Descartes's injudicious title of Geometry.

It is true indeed, that in a following passage, Dr. Pemberton says, "of their (the ancients) taste and form of demonstration, Sir Isaac always professed himself a great admirer: I have heard him even censure himself for not following them yet more closely than he did; and speak with regret of his mistake, at the beginning of his mathematical studies, in applying himself to the works of Descartes, and other algebraic writers, before he had considered the elements of Euclid with that attention, which so excellent a writer deserves."—But the mode of expression here used, is so different from the foregoing, that there can be no doubt, but that it was intended to convey a different meaning. And if in the censure first mentioned, viz. "for handling geometrical subjects by algebraic calculations," Dr. Pemberton had understood that Sir Isaac meant to include himself, this last passage would have been a mere tautology. But this last strongly implies, on the contrary, that Sir Isaac had, in general, endeavoured to follow closely the ancient geometrical form of demonstration, in preference to that by algebraic calculation; which is of modern invention.

There is a remarkable instance of the attention he paid to the distinction between these methods, and of the preference he gave to the former, in his great work of the Principia. Having in lemma 19, and its corollaries, given a concise and elegant solution of a noted geometric problem, he subjoins: "Atque ita problematis veterum de quatuor lineis ab Euclide incœpti, et ab Apollonio continuati, non calculus, sed compositio geometrica, qualem veteres quærebant, in hoc corollario exhibetur." That the words, "non calculus sed compositio geometrica," refer to Descartes's prolix algebraic solution of this problem, in his Geometry, p. 25-34, will, I believe, be readily granted by every one acquainted with Sir Isaac Newton's writings.

On the whole, I humbly conceive, that Dr. Pemberton's meaning, in the former passage, might have been better expressed in Latin, as follows: "Sæpiùs eos reprehendebat, qui res merè geometricas algebraïcis rationibus tractavissent; et libro suo de algebrâ Arithmeticæ Universalis titulum ponebat, asserens Cartesium suum de re eâdem volumen inscitè dixisse Geometriam, in quo ostendit, quomodo hæ computationes subsidia esse possunt geometris ad inveniendum." Which of these translations does most justly express the sense of the original, may, I suppose, be safely left to the judgment of every person that understands both the languages. I would only add, that this mistake of Castillione must have been owing, either to inadvertence, or to his not being perfectly acquainted with the English language; as he elsewhere appears to have had the highest veneration for Sir Isaac Newton. This mistake may, to some, appear trivial; but, in my apprehension, every circumstance relative to so illustrious a character as that of Sir Isaac Newton, derives importance from it, and ought to be marked with great exactness.

XX. M. De Luc's Rule for Measuring Heights by the Barometer, reduced to the English Measure of Length, and adapted to Fahrenheit's Thermometer, and other Scales of Heat, and reduced to a more Convenient Expression. By the Astronomer Royal. [Mr. Maskelyne.] p. 158.

M. De Luc, F. R. S., in his Treatise on the Barometer and Thermometer, has given a rule for the measurement of heights by the barometer, deduced from his experiments, and far more accurate than any published before; since it appears that he could determine heights by it generally to 10 or 15 feet, and that the error seldom, if ever, amounted to double that quantity. This valuable degree of exactness he has obtained principally by detecting the faults of the common barometer, and improving its construction; and by introducing the use of the mercurial thermometer, to accompany that of the barometer. The principal faults, which he found in the common barometers, arose from the repulsion of the quicksilver by the glass tube, from air and moisture admitted into the tube, and from the variations of the density of quicksilver by heat and cold; another very considerable error arose, in calculating heights from the barometer, by not allowing for the changes in the density of the air, whose gravity affords us this measure of heights, owing to heat and cold. The first cause of error, that of the repulsion of the tubes, he remedied, by substituting a syphon barometer instead of the simple upright tube, the repulsion of the two legs of the syphon counteracting itself. The error arising from air and moisture in the tube, he cured by boiling the quicksilver after it was put into the tube, and other precautions. The errors, in the estimation of the heights arising from the changes in the density of the quicksilver, and density of the air by heat and cold, he shows how to correct by allowances depending on two thermometers, one attached to

the frame of the barometer itself, and the other made to be exposed to the open air, to show its degree of heat; which thermometers are to be noted both at the top and bottom of the hill. Lastly, by a great number of experiments made with accurate barometers and thermometers of his own construction, he has deduced a rule for calculating heights of places; the exactness of which he has sufficiently proved by a large table of experiments. But this rule is expressed in French measure, and is adapted either to a thermometer, whose freezing point is 0, and that of boiling water 80, or to thermometers of particular scales. It may therefore be useful to reduce M. de Luc's rule to English measure, and to adapt it to the thermometer of Fahrenheit's scale, which is generally used in this country.

M. de Luc, in the winter season, heated the air of his room to as great degree as he could, and noted the rise of the barometer, owing to the diminution of its density, or specific gravity, by heat; he also noted the height of the thermometer, both before and after the room was heated. Hence he deduced a rule, that when the barometer is at 27 French inches, which was the case in this experiment, an increase of heat, from freezing to that of boiling water, will raise the barometer 6 lines, or $\frac{1}{54}$ part of the whole. It is easy to see, that when the barometer is higher than 27 French inches, this variation will increase in the same proportion; or will be always $\frac{1}{54}$ of the height of the barometer; therefore, if the height of the barometer be called B, the rise of the barometer, for an increase of heat from freezing to boiling water, will be $\frac{B}{54}$; and, as it will be less for a less difference of heat, therefore, if the number of degrees, marked on the thermometer, between freezing and boiling water, be called K, and the rise of the thermometer from any given point be called H, the correspondent rise of the barometer will be $\frac{B}{54} \times \frac{H}{K}$, by the increase of heat from the given point by the number of degrees H. If the heat, instead of increasing, was to decrease, then H would signify so many degrees decrease of heat, and the barometer would sink by $\frac{B}{54} \times \frac{H}{K}$. The fixed temperature of heat, to which M. De Luc thought best to reduce his observations of the barometer, is $\frac{1}{8}$ of the interval from freezing to boiling water above the former point: and if the thermometer was higher than this degree, he subtracted $\frac{B}{54} \times \frac{H}{K}$; if it was lower, he added it to the observed height of the barometer; and thus he obtained the exact height of the barometer, such as it would have been, if the density of its quicksilver had been the same as answers to the fixed degree of temperature. He thus corrected the height of both his barometers (that at the bottom, and that at the top of the hill) for the particular degree of heat, indicated by a thermometer attached to the barometer, at each station; for it might and would commonly happen, that the degree of heat would be different at the two stations. The heights of the barometers, thus corrected, were what he made use of in

his subsequent calculations. Calling these two altitudes of the barometer B and b, putting log. B and log. b, for the logarithms of B and b, taking only the first 4 places of figures, after the characteristic, or considering the remaining figures as decimals, and putting C for the mean height of a thermometer, exposed to the air at top and bottom of the hill, the freezing point being 0, and the pint of boiling water at 80, he finds, by his experiments, that the height of the hill will be given in French toises, when C is $16\frac{3}{4}$, by simply taking the difference of the logarithms of the heights of the barometer, or will be equal to log. B — log. b; and in any other degree of heat, will be greater or less, in proportion as the rarity of the air is greater or less, than in the fixed temperature; or greater or less by $\frac{1}{215}$ part of the whole, for every degree of the thermometer reckoned from the fixed temperature $16\frac{3}{4}$; and consequently the height of the place will be expressed generally in French toises, by this *formula*, log. B — log. b + (log. B — log. b) $\times \frac{C - 16\frac{3}{4}}{215}$ = (log B — log. b) $\times (1 + \frac{C - 16\frac{3}{4}}{215})$.

To reduce this *formula* to English measure, and to the scale of Fahrenheit's thermometer, we should first premise some particulars. The French foot is to the English foot, as 1.06575 to 1, as was found by a very accurate experiment: see Phil. Trans., vol 58, for 1768, p. 326; and it is well known, that the point of freezing, on Fahrenheit's thermometer, is at 32, and that of boiling water at 212, or the interval between them 180 degrees. But M. De Luc's point of boiling water 80, was marked when the barometer was at 27 French inches; and it is the custom of our principal English workmen to mark the point of boiling water, 212, on Fahrenheit's thermometer, when the barometer stands at 30 inches, which is equal to 28 inches 1.8 lines French measure; or 13.8 lines higher than M. De Luc's barometer, when he set off the point of boiling water on his thermometers; and it is well known, that the heat of boiling water varies with the weight of the atmosphere: M. De Luc finds, by his experiments, this rule, that an increase of 1 line in the height of the barometer, raises the quicksilver of the thermometer, placed in boiling water, by $\frac{1}{1134}$ part of the interval between the freezing point and that of boiling water: he afterwards indeed found, that this rule would not answer for such large variations of the barometer, as take place in ascending to very great heights above the earth's surface; but it is accurate enough for any small variation of the barometer, on one side or other of its mean height in these lowest regions of the atmosphere. The change therefore of the boiling point on Fahrenheit's scale, for a change of 1 line in the barometer, will be $\frac{180}{1134} = 0.16$; therefore 13.8 lines will cause $0.16 \times 13.8 = 2.2$ degrees of Fahrenheit's scale; and a thermometer, whose point of boiling water was marked 212, when the barometer stood at 30 English inches = 28 inches 1.8 lines French measure, will, when the barometer descends to 27 French inches, sink 2.2 degrees in boiling water, or to 209.8, or

in round numbers to 210 degrees, which is distant only 178 from 32, the point of freezing. Hence an extent of 80° of M. De Luc's thermometer, answers to an extent of 178 of our Fahrenheit's thermometer; and putting F for the degrees of this thermometer, corresponding to C of M. De Luc's, we shall have $C : F - 32 :: 80 : 178$, and $C = (F - 32) \times \frac{80}{178}$; which substituted in M. De Luc's *formula* gives $(\log. B - \log. b) \times (1 + \frac{C - 16\frac{3}{4}}{215}) = (\log. B - \log. b) \times (1 + \frac{(F-32) \times \frac{80}{178} - 16\frac{3}{4}}{215}) = (\log. B - \log. b) \times (1 + \frac{F - 69.27}{478.38})$. Where the answer will still come out in French toises, though adapted to Fahrenheit's thermometer. To bring it out in English fathoms, or measure of 6 feet, multiply the above expression by 1.06575, and we shall have in round numbers $(\log. B - \log. b) \times (1 + \frac{F - 40}{449})$; which will express the height between the 2 stations in English fathoms.

In the foregoing expressions, B and b, as before said, signify heights of the barometer, at the lower and higher stations, both corrected according to M. de Luc's directions, for the difference of heat between a fixed temperature, (namely $\frac{1}{8}$ of the interval between freezing and boiling water), and the present heat, indicated by the thermometer attached to the barometer at each station; but it is not necessary to correct both barometers for the effect of heat, but only one for the difference of heat of the two; which will be more convenient also on another account, because the difference of heat, at the two stations, will be generally small, and the correction to reduce one barometer to the heat of the other will consequently be small also; whereas the difference of the present heat, and the fixed temperature, and consequently the correction of both barometers, may be frequently very considerable: this is evident: because if the heat of the barometers, at both stations, was the same, however different from the fixed temperature chosen by M. de Luc, no correction would be necessary; the mercury in the barometer in both stations being expanded in the same proportion, and consequently the difference of the logarithms of its height, at both stations, being the same, as if the heat of both barometers had agreed with that of the fixed temperature. I shall now therefore suppose the upper barometer is to be corrected, to reduce it to the temperature of the lower one, and that b signifies the height of this barometer, as observed, and not yet corrected; the correction, from what has been said above, calling D the difference of height of the thermometer attached to the barometer at the two stations, will be $\pm \frac{Db}{54K}$, according as the thermometer stands highest at the lower or upper station; and the upper barometer corrected, instead of b, will be $b \pm \frac{Db}{54K}$, which substituted in the formula, gives $[\log. B - \log. (b \pm \frac{Db}{54K})] \times (1 + \frac{F - 40}{449})$. But the cor-

rection, on account of the difference of heat of the barometer at the two stations, may be reduced to a still easier expression, in which the variable quantity b, the height of the upper barometer, shall not appear. The fluxion of a logarithm is to the fluxion of its natural number, as the modulus of the system to the natural number; and 4343 is the modulus of the common logarithms, when the 4 places, next following the characteristic, are taken as whole numbers, instead of decimals, which is meant to be done in the use of the foregoing formula. Therefore $\frac{\text{D}b}{54\text{K}}$ being very small with respect to b, we shall have, variation of log. b : variation of b ($= \frac{\text{D}b}{54\text{K}}$) :: 4343 : b very nearly, and thence variation of log. $b = \pm \frac{\text{D}b}{54\text{K}} \times \frac{4343}{b} = \pm \frac{4343\text{D}}{54\text{K}}$. Which (putting $\text{K} = 178$) $= \pm\ 0.452\text{D}$. Hence $\log.(b \pm \frac{\text{D}b}{54\text{K}}) = \log.\ b \pm 0.452\text{D}$; which being substituted in the formula above, will give the difference of height, of the two stations, in English fathoms, in a more convenient expression, namely $(\log.\ \text{B} - \log.\ b \mp 0.452\text{D}) \times (1 + \frac{\text{F} - 40}{449})$; where the upper sign, —, is to be used, when the thermometer of the barometer is highest at the lower station, and the lower sign, +, is to be used, when the said thermometer is lowest at the lower station. The first case will be most common; especially where the difference of height of the two stations is considerable. It should also be observed, that when F, the height of Fahrenheit's thermometer, is less than 40°, then $+ \frac{\text{F} - 40}{449}$, becoming negative or subtractive, must be applied in the calculation accordingly.

It may perhaps be convenient to repeat here the meaning of the algebraic terms, used in the foregoing formula, that any person may make use of it, without having occasion to recur to the foregoing investigation. B signifies the observed altitude of the barometer at the lower station, and b that at the upper station; log. B and log. b, signify their logarithms taken out of the common tables, by assuming the first 4 figures next following the characteristic as whole numbers, and considering the 3 remaining figures, to the right hand, as decimals; D signifies the difference of height of Fahrenheit's thermometer, attached to the barometer at the top and bottom of the hill; and F signifies the mean of the two heights of Fahrenheit's thermometer, exposed freely for a few minutes to the open air in the shade, at the top and bottom of the hill.

The formula, for the measure of heights, may also be changed, and adapted to thermometers of particular scales, for the convenience of calculation, as M. de Luc has done; but these scales will be different from his. The thermometer attached to the barometer, had better be divided with the interval between freezing and boiling water, consisting of 81.4 degrees ($= 180 \times .452$) the freezing point may be marked 0, and the point of boiling water will be 81.4; for then,

if the difference of height of this thermometer, at the two stations, be called d, we shall have $d = 0.452 \times \text{D}$; for $d : \text{D} :: 81.4 : 180 :: 0.452 : 1$; and the number of degrees expressed by d, will show immediately the correction for the difference of heat of the two barometers. If the thermometer, designed to show the temperature of the air, be divided with the interval between freezing and boiling water $= 200$, and the freezing point be marked $- 9$, and the boiling point $+ 191$, and the heights of this thermometer, at the two stations, be called G and I, we shall have $\frac{\text{F} - 40}{449} = \frac{\text{G} + \text{I}}{2 \times 500} = \frac{\text{G} + \text{I}}{1000}$. For $\text{F} - 40 = \text{F} - 32 - 8$, is the height of Fahrenheit's thermometer, reckoned from 8 degrees above freezing, and $449 : 500 :: 180 : 200 :: 8 : 9$, and the fraction $\frac{\text{F} - 32 - 8}{449}$, if both the numerator and denominator be increased in the ratio of 449 to 500, will become $=$
$\frac{(\text{F}-32-8) \times \frac{500}{449}}{500} = \frac{(\text{F} - 32)\frac{500}{449} - 9}{500} = \frac{\text{G} + \text{I}}{2 \times 500} = \frac{\text{G} + \text{I}}{1000}$, because $\frac{\text{G} + \text{I}}{2} + 9 = (\text{F} - 32) \times \frac{500}{449}$. Therefore, if the thermometer of the barometer has the freezing point marked 0, and the point of boiling water 81.4, and the difference of its height, at the two stations, be called d; and the thermometer for measuring the temperature of the air, be divided with the interval of 200 between the freezing point and that of boiling water, and the first be marked $- 9$, and the latter $+ 191$, and the degrees shown by this, at the two stations, be called G and I; the formula, that will give the height of the upper station above the lower one, in English fathoms, will be $(\log. \text{B} - \log. b \mp d) \times (1 + \frac{\text{G} + \text{I}}{1000};)$ which consequently multiplied by 6, will give the height in English feet. It is to be observed, as before, that $- d$ or $+ d$ is to be used, according as the thermometer, attached to the barometer, is highest at the lower or upper station; and if G and I should happen to fall below 0 of the scale, or to be subtractive, they must be applied accordingly in the calculation.

I shall now add nothing more, but to give the rule for finding heights by the barometer, according to the formulæ delivered above, in common language; first, as adapted to Fahrenheit's thermometer, and next, as adapted to the 2 thermometers of particular scales. Take the difference of the tabular logarithms of the observed heights of the barometer, at the two stations, considering the first 4 figures, exclusive of the index, as whole numbers, and the 3 remaining figures to the right as decimals, and subtract or add $\frac{452}{1000}$ of the difference of the altitude of the Fahrenheit's thermometer, attached to the barometer at the two stations, according as it was highest at the lower or upper station; thus you will have the height of the upper station above the lower, in English fathoms nearly: to be corrected, as follows: make this proportion; as 449 is to the difference of the mean altitude of Fahrenheit's thermometer, exposed to the air at

the two stations, from 40°, so is the height of the upper station found nearly, to the correction of the same: which added or subtracted, according as the mean altitude of Fahrenheit's thermometer was higher or lower than 40°, will give the true height of the upper station above the lower, in English fathoms; and multiplied by 6, will give it in English feet.

The same rule, adapted to the thermometers of particular scales, is this: take the difference of the tabular logarithms of the observed heights of the barometer, at the two stations, considering the first 4 figures, exclusive of the index, as whole numbers, and the 3 remaining figures to the right as decimals; and subtract or add the difference of the thermometer, of a particular scale, attached to the barometer, at the two stations, according as it was highest at the lower or upper station, and you will have the height of the upper station above the lower one, in English fathoms, nearly; to be corrected as follows: make this proportion; as 1000 is to the sum of the altitudes of the thermometer of a particular scale, exposed to the air at both stations, so is the height of the upper station above the lower, found nearly, to the correction of the same; which added or subtracted, according as the sum of the altitudes of the thermometers, exposed to the air, is positive or negative, will give the true height of the upper station above the lower in English fathoms; and multiplied by 6, will give it in English feet.

XXI. Eclipses of Jupiter's Satellites, observed near Quebec. By Sam. Holland, Esq., Surveyor General of Lands for the Northern District of America. p. 171.

These eclipses of Jupiter's satellites were observed at a house, bearing south 36° west from Quebec, distant from the castle of St. Lewis $2\frac{1}{2}$ miles, with Dollond's long reflecting telescope.

1770.	Mean time.				
April 19	14^h	5^m	21^s	immersion of the 2d	satellite.
May 1	12	42	32	1st	
14	12	5	30	2d not exact to 4″ through the thickness of the atmosphere.	
21	13	41	30	2d	
24	12	52	20	1st	

XXII. Observations of the Immersions and Emersions of the Satellites of Jupiter, taken in 1768, *by Ensign George Sproule, of H. M.* 59*th Reg., on the South Point of the Entrance of Gaspee Basin, which bears from Cape Ferrilong, or the Cape forming the Bay to the Northward, N.* $68\frac{1}{4}$ *W. by the true Meridian, distant* $12\frac{1}{4}$ *Marine Miles. Communicated by the Astron. Royal.* p. 177.

The latitude of the place of observation, at Gaspee, accurately determined, was 48° 47′ 32″. The variation of the needle, by repeated trials different ways, was 16° 30′ w.

1768, Jan. 29..	immer.	1....	13^h	57^m	47^s apparent time.		
Mar. 15..	immer.	1....	14	21	14		
16..	immer.	2....	11	59	7		
	immer.	3....	13	30	10		
April 9..	emer.	1....	11	18	26	N. B. In these 2 emersions the satellites seemed to emerge slowly out of the shadow.	
10..	emer.	2....	11	38	8		
25..	emer.	1....	9	39	40	This is the best observation, the satellite starting out instantaneously.	
May 9..	emer.	1....	13	30	54		
12..	emer.	2....	11	15	43.		

XXIII. Astronomical Observations made by Samuel Holland, Esq., Surveyor General of Lands for the Northern District of North America, for ascertaining the Longitude of several Places in the said District. p. 182.

Kittery Point, in the province of Main, in Piscataqua harbour.

The latitude by repeated observations, 43° 4′ 27″ N.

Observed, with Dollond's 12-feet refracting telescope, immersion and emersions of ♃'s satellites as follow.

			Apparent time.		
1771. April 11th, an immersion of the 1st, at......			15^h	43^m	30^s
27th, same	same........		14	1	43
May 4th, same	same........		15	55	54.

The variation of the compass at this place, is 7° 46′ west.

Portsmouth, province of New Hampshire, the latitude by repeated observations, 43° 4′ 15″ N.

Observed, with Dollond's 12-feet refracting telescope, immersions and emersions of ♃'s satellites as follow.

				Apparent time.		
1772, Sept.	6th,	an emersion of the 2d, at		11^h	9^m	20^s
	18th,	same............	1st	9	42	35
Oct.	11th,	same............	same	10	5	4
Nov.	3d,	same............	same	10	23	54
	9th,	same............	2d	10	51	39
	12th,	same............	1st	6	48	1
	19th,	same............	same	8	42	44
	23d,	immerged entirely,	3d	6	8	6
	same	began to emerge..	same	9	28	14
Dec.	4th,	an emersion......	2d	7	50	0
	5th,	same............	1st	6	57	44

The variation of the compass at this place is 7° 48′ west.

XXIV. Observations of Eclipses of Jupiter's first Satellites made at the Royal Observatory at Greenwich, compared with Observations of the same; made by Samuel Holland, Esq., and others of his Party, in several Parts of North America, and the Longitudes of the Places thence deduced. By the Astronomer Royal. p. 184.

The result of those comparisons, give the following longitudes of the places of observations.

		h	m	s	
The meridian of	Place of observation on St. John's island	4	11	49	West of Royal Observatory, at Greenwich.
	Louisbourg	3	59	57	
	South Point, entrance of Gasper	4	17	50	
	Capt. Holland's house near Quebec	4	44	46	
	Kittery Point, province of Main, in Piscataqua harbour	4	42	58	
	Portsmouth, New Hampshire	4	42	53	

XXV. Immersions and Emersions of Jupiter's first Satellite, observed at Jupiter's Inlet, on the Island of Anticosti, North America, by Mr. Thomas Wright, Deputy Surveyor General of Lands for the Northern District of America; and the Longitude of the Place, deduced from Comparison with Observations made at the Royal Observatory at Greenwich, by the Astronomer Royal. p. 190.

These observations were made at Jupiter's inlet, 2 leagues to the westward of the south-west point of the island of Anticosti, situated at the entrance of the river St. Laurence, in latitude 49° 26′ north, with a 2-feet reflecting telescope of the late Mr. Short's construction. Mr. Wright's observations of the eclipses of Jupiter's first satellite, corrected, are as follow:

		Apparent time.	Time at Greenw. per Naut. Almanac.	Difference of meridians.
1767, Jan. 17	immer.	14h 50m 27s	19h 5m 29s	4h 15m 2s
Feb. 2	immer.	13 2 21	17 17 53	4 15 32
18	immer.	11 19 0	15 34 14	4 15 14
25	immer.	13 13 46	17 29 16	4 15 30
Mar. 29	emer.	12 10 59	16 25 27	4 14 28
April 5	emer.	14 7 23	18 22 11	4 14 48
7	emer.	8 36 19	12 51 16	4 14 57
14	emer.	10 32 56	14 47 48	4 14 52
30	emer.	8 54 17	13 8 59	4 14 42

The mean difference of meridians by the 4 immersions is $4^h\ 15^m\ 19\frac{1}{2}^s$, and by the 5 emersions is $4^h\ 14^m\ 45\frac{1}{2}^s$; both which ought to be corrected, by the help of the nearest observations made at the Royal Observatory at Greenwich. The immersions and emersions observed there, proper to compare with the preceding observations, are these: all observed with a 6-feet reflector, which, I reckon, shows an immersion of the first satellite 20^s later, and an emersion of the same as much sooner, than a 2-feet reflecting telescope.

		Observed at Greenw. App. time.	App. time per Naut. Almanac.	Correct. of Naut. Alman.
1767, Jan. 12	immer.	11h 41m 41s	11h 42m 8s	— 0m 27s
Feb. 27	immer.	11 57 7	11 58 6	— 0 59
Mar. 22	emer.	14 28 48	14 28 50	— 0 2
April 9	emer.	7 20 1	7 20 25	— 0 24
14	emer.	14 47 52	14 47 48	+ 0 4
16	emer.	9 16 13	9 16 55	— 0 42
30	emer.	13 9 10	13 8 59	+ 0 11
May 9	emer.	9 32 26	9 33 12	— 0 46

The correction of the Nautical Almanac for a 6-feet reflector, by the mean of the 2 immersions, is — 43^s, which applied to $4^h\ 15^m\ 19\frac{1}{2}^s$, the longitude of Jupiter's inlet found from immersions, by the help of the Nautical Almanac, gives

$4^h\ 14^m\ 36\frac{1}{2}^s$, the difference of longitude deduced from the immersions. The correction of the Nautical Almanac, by the mean of the 6 emersions, is — $16\frac{1}{2}''$, which applied to $4^h\ 14^m\ 45\frac{1}{2}''$, the longitude of Jupiter's inlet found by the emersions, by the help of the Nautical Almanac, gives $4^h\ 14^m\ 29^s$, the longitude deduced from the emersions. The mean of these 2 results, found from the immersions and emersions separately, is $4^h\ 14^m\ 33^s$ the proper difference of longitude of Jupiter's inlet west of Greenwich. I have here made no allowance for the difference of power of the 2-feet reflector, used at Jupiter's inlet, and the 6-feet reflector, used at Greenwich; because the mean is taken between the results from the immersions and emersions; which method includes that correction; that is to say, gives the same result whether that correction be made or not. From the foregoing comparisons it should seem that the air is much clearer at the island of Anticosti than at Greenwich, which Mr. Wright confirmed to me, since the immersions give the longitude only $7\frac{1}{2}^s$ greater than the emersions; which shows that Mr. Wright observed an immersion only 4^s sooner, and an emersion as much later, with a 2-feet reflector, than was done at Greenwich in a 6-feet reflector; though, in an equally good air, this latter telescope would have had the advantage of the former by 20^s, instead of 4^s.

XXVI. Extract of a Letter from Mr. Humphry Marshall, of West Bradford, in Chester County, Pennsylvania, to Dr. Franklin, sent with Sketches of the Solar Spots, dated May 3, 1773. p. 194.

The appearances of these spots were not engraven. Mr. M. says he is of opinion that the spots are near the sun's surface, if not closely adhering to it, for these reasons: 1. That their velocities are apparently greatest near the centre, and gradually slower towards each limb. 2. That the shape of the spots varies, according to their position on the several parts of the sun's disc; those that appear broad, and nearly round, when on the middle, seeming, at their first appearance on the eastern limb, but as lines; and, as they advance towards the centre, become oval, then round, and, in their progress to the western limb, appear again as ovals and lines. His other remarks were, that the spots were $12\frac{1}{2}$ days, and about 2 or 3 hours, in passing; that though some continued visible from one limb to the other, a few would disappear, after having been visible several days; and others divided into parts; that scarcely any spots ever appeared beyond what may be called the polar circles of the sun; and that the same spot never appeared a 2d time, on the eastern limb, at least not in the same form and position.

XXVII. Account of the House-martin, or Martlet. By the Rev. Gilbert White. p. 196.

Reprinted in Mr. White's History of Selborne.

XXVIII. Extract of a Register of the Barometer, Thermometer, and Rain, at Lyndon in Rutland, 1773. *By T. Barker, Esq.* p. 202.

This is Mr. Barker's usual annual communication, of the highest, lowest, and mean state, of the barometer and interior and exterior thermometers, for each month in the year. Also the rain of each month, the whole sum being 29⅞ inches.

XXIX. On certain Receptacles of Air in Birds, which Communicate with the Lungs, and are lodged both among the Fleshy Parts and in the Hollow Bones of those Animals. By John Hunter, F. R. S. p. 205.

Reprinted with additions in this author's Observations on the Animal Economy, 4to., 1786.

XXX. M. de Luc's Rules, for the Measurement of Heights by the Barometer, Compared with Theory, and Reduced to English Measures of Length, and adapted to Fahrenheit's Scale of the Thermometer: with Tables and Precepts, for Expediting the Practical Application of them. By Samuel Horsley, LL.D. p. 214.

After the Astronomer Royal's clear and practical paper on the very same subject, in the 20th article preceding, (p. 520) it is quite unnecessary to reprint this very diffuse and elaborate work; and the rather, as other and later accounts of the same thing are to be seen elsewhere, treated in a manner much more simple and perspicuous.

XXXI. A Catalogue of the Fifty Plants from Chelsea Garden, presented to the Royal Society by the Apothecaries' Company, for 1773, *&c. By Wm. Curtis, clariss. Soc. Pharmaceut. Lond. Soc. Hort. Chelsean. Præfect. et Prælector Botan.* p. 302.

This is the 51st annual presentation, amounting to 2550 plants.

XXXII. Observations on the Gillaroo Trout, commonly called in Ireland the Gizzard Trout. By John Hunter, F. R. S. p. 310.

Reprinted in this author's Observations on the Animal Economy, before referred to.

XXXIII. Explication of a most Remarkable Monogram on the Reverse of a very Ancient Quinarius, never before published or explained. By the Rev. John Swinton, B. D., F. R. S. p. 318.

This piece is a very ancient, or rather an original, quinarius, extremely well preserved. It has on one side a female head in a helmet, with the letter V be-

hind, standing for 5, the number of asses it contains; and on the reverse, Castor and Pollux, or, according to Sig. Olivieri, two Castors, on horseback, with seven stars over each of their helmets, or caps. In the exergue we discover the word ROMA, formed of very ancient characters; and under the belly of one of the horses the monogram, which distinguishes this quinarius from all the other similar pieces that ever fell under my view or observation. Nor have I ever met with it in any author I had occasion to consult or peruse. To me therefore it cannot but appear in the light of an inedited coin.

The Romans first coined silver money, according to Pliny, with whom Livy, in this point, agrees, in the 485th year of the city. Some of the earliest pieces, of which several still remain in the cabinets of the curious and the great, exhibited a female galeated head on one side, as the quinarius now considered; and on the reverse Castor and Pollux, or, as Sig. Olivieri calls them, two Castors, as both these figures are horsemen, such as clearly and distinctly appear on this coin. Therefore, as the letters forming the word ROMA, in the exergue, are antique enough, at least, for the time when silver was first coined at Rome, or 5 years before the commencement of the first Punic war, we may fairly suppose this quinarius to be either coeval with, or, as I rather imagine, a little anterior to the commencement of that war.

The monogram on the reverse of this quinarius, so extremely remarkable for the number of letters it contains, we shall find, on a close and attentive examination, to exhibit the word ROMANORO, the masculine genitive case plural of Romanvs, in the days of C. Duilius and L. Scipio, the son of Barbatus, towards the close of the 5th century of Rome; some time after the completion of which, the Romans converted the last syllable ro into rvm. But to analyse this extraordinary complex character a little more particularly, the first part of it perfectly answers to the word roma, as represented by a monogram on several coins of the Calpurnian family; and the latter part of it is evidently formed of the letters noro, the last of which is apparently included in the head or top of the r. As the masculine plural termination of the genitive case was ro, instead of rvm, in the year of Rome 494, when the inscription mentioning L. Scipio's conquest of Corsica, and reduction of Aleria, seems to have first appeared; it is highly probable, that the piece in question was either coeval with, or a little anterior to, that year. The inscription is as follows:

HONC. OINO. PLOIRVME. CONSENTIONT. R.	*Hunc unum plurimi consentiunt Romæ,*
DVONORO. OPTVMO. FVISE. VIRO.	*Bonorum optimum fuisse virum,*
LVCIOM. SCIPIONE. FILIOS. BARBATI.	*Lucium Scipionem. Filius Barbati,*
CONSOL. CENSOR. AIDILIS. HIC. FVET.	*Consul, Censor, Ædilis, hic fuit.*
HIC. CEPIT. CORSICA. ALERIAQVE. VRBE.	*Hic cepit Corsicam, Aleriamque Urbem.*
DEDET. TEMPESTATEBVS. AIDE. MERETO.	*Dedit Tempestatibus ædem merito.*

From what has been here laid down it seems highly probable, that this quinarius first appeared about the year of Rome 494, or rather that its first appearance was a little anterior to that year. Which if we admit, it will follow, that the Romans borrowed the monogrammatic way of writing rather from the Etruscans than the Greeks, as I asserted in one of my former papers; with the first of which nations they were perfectly well acquainted, even from the very beginning of their state; whereas they seem to have had little or no intercourse with the other, when the piece in question was coined. It remains, therefore, that what I advanced, in the paper here referred to, is clearly and indubitably true.

With regard to monograms in general, it may not be improper to remark, that they were known and used in several parts of the east, from pretty remote antiquity. They occur on some of the Hebrew, or Samaritan, and Phœnician coins, as well as on the Greek and Roman. I have an exceedingly curious Hebrew, or Samaritan coin, coeval with Simon the Just, prince and high priest of the Jews, with a monogram on it. That the Phœnicians were not unacquainted with monograms, has been admitted by the learned and ingenious M. Pellerin, and is evinced by one or two of the Phœnician inscriptions on the stones found in the ruins of Citium. That the Arabs likewise anciently used them, on certain occasions, we learn from the ligatures of the Kufic letters, and the inscriptions still remaining on several of the earlier Arabic coins. Nay, they are not disused among the modern Arabs, in their common writing, even at this very day. As for the Greeks, nothing is more common than ligatures, or monograms, on their coins. That the Palmyrenes also had several such ligatures, or complex characters, I have many years since incontestably proved.

With respect to the Romans, nothing is more certain than that combinations of 2, 3, and even 4 elements, formed into one character, not seldom occur on their coins. More extensive or complex ligatures than the monograms of 4 letters on their ancient medals very rarely appear. I have, however, an inedited semissis of the Pompeian family, with the head of Saturn, and behind it the letter S, the mark of the semissis, on one side; and the prow of a ship, over which is a monogram composed of the 5 letters, Q, P, O, M, P.

XXXIV. *Astronomical Observations made at Chislehurst, in Kent, in the Year 1773. By the Rev. F. Wollaston, LL.B., F.R.S.* p. 329.

Mr. W. having the last 2 winters communicated to the R S. what astronomical observations he had occasionally made in the course of each year, it seems to be a call on him to continue the same now. His instruments and situation are the same as before-described; and the accompanying tables are in the same form as the last year. His clock has been kept going on, without any alteration of any kind;

it is only by long and uninterrupted trials, that any judgment can be formed concerning the cause of errors.

The first is a series of observations on the going of the clock, which gradually gains by the heat of the season, and loses by the cold again. Then a register of the barometer, thermometer, and hygrometer. Next, occultations of stars by the moon. Then eclipses of Jupiter's satellites, after the manner of M. Bailly.

Since the reading of a paper, communicated last year to this society by Dr. Wilson, professor at Glasgow, on the spots of the sun; who mentions some appearances when they approach the limb, which I thought I had now and then observed, says Mr. W.; I have frequently turned my glass that way, as occasion offered, to see whether those appearances were constant, or what might be discovered to confirm the hypothesis laid down in the latter part of that paper. Dr. Wilson, I hope, will excuse me when I say, that the appearance he mentions when the spots approach the sun's limb, as if they were in a cavity on his surface, is not constant. They generally indeed have appeared so to me, I confess. But as they sometimes have not, and as I have very frequently seen them almost in contact with the limb, that is, not $\frac{1}{5}$ a second of time distant in passing a wire; I think they can scarcely be in such a hollow, below his surface, as the Doctor describes. To me indeed, by the brighter light often adjoining to them when near the limb, they have rather put on the appearance as if they were in the crater of a volcano on the top of an eminence, which then turned its side towards us; and if so, the spot would appear somewhat nearer to the limb than it actually was. I have indeed never seen any protuberance on either limb of the sun, as I have on the moon; but I have often observed, near the eastern limb, a bright facula just come on, which has the next day shown itself as a spot; though I do not recollect to have seen such a facula near the western one, after a spot's disappearance. Yet I believe both these circumstances have been observed by others, and perhaps not only near the limbs.

As to the nebulæ, they are certainly not always, though they usually are, quite round each spot, or each cluster of spots; neither are they always externally convex. Nothing therefore can be concluded from that circumstance. Besides spots are sometimes quite without any nebulæ at all; or none that I could perceive with any power of my glass. What the spots, or their nebulæ, are, I pretend not to guess. To me they appear as if they were adjoining to the surface: though that is doubted by better astronomers, who have calculated their motions. The circumstance of the faculæ being sometimes converted into spots, I think I may be sure of. That there is generally, perhaps always, a mottled appearance over the face of the sun, when carefully attended to, I think I may be as certain. It is most visible towards the limbs; but I have undoubtedly seen it in the centre: yet I do not recollect to have observed this appearance, or indeed any spots, to-

wards his poles. Once I saw, with a 12-inch reflector, a spot burst to pieces while I was looking at it. I could not expect such an event; and therefore cannot be certain of the exact particulars; but the appearance, as it struck me at the time, was like that of a piece of ice when dashed on a frozen pond, which breaks to pieces and slides on the surface in various directions. I was then a very young astronomer; but think I may be sure of the fact. Perhaps I may be thought a young astronomer still, for throwing out these rough observations and crude thoughts: but whatever they be, if my errors shall lead others into inquiries which may be productive of certainty, their end will be answered.

XXXV. *An Account of a Woman accidentally Burnt to Death at Coventry. By B. Wilmer, Surgeon, Coventry.* p. 340.

Mary Clues, of Gosford-street in this city, aged 52 years, was of an indifferent character, and much addicted to drinking. Since the death of her husband, which happened about 1½ year before, her propensity to this vice increased to such a degree, that, as Mr. W. was informed by several of her neighbours, she had drunk the quantity of 4½ pints of rum, undiluted with any other liquor, in a day. This practice was so familiar to her, that scarcely a day had passed for a year before her death, but she swallowed from ½ a pint to a quart of rum or aniseed-water. Her health gradually declined; and, from being a jolly, well-looking woman, she became thinner, her complexion altered, and her skin became dry. About the beginning of Feb. 1774, she was attacked with the jaundice, and took to her bed. Though she was then so helpless, as hardly to be able to do any thing for herself, she continued her old custom of dram-drinking, and generally smoked a pipe every night. No one lived with her in the house. Her neighbours used, in the day, frequently to come in to see after her, and in the night, commonly, though not always, a person sat up with her.

Her bed-room was next the street, on the ground-floor, the walls of which were plastered, and the floor made of bricks. The chimney was small, and there was a grate in it, which, from its size, could contain but a very small quantity of fire. Her bedstead stood parallel to, and at the distance of about 3 feet from, the chimney. The bed's head was close to the wall. On the other side the bed, opposite the chimney, was a window opening to the street. One curtain only belonged to the bed, which was hung on the side next the window, to prevent the light being troublesome. She was accustomed to lie on her side, close to the edge of the bedstead, next the fire; and on Sunday morning, March the 1st, she tumbled on the floor, where her helpless state obliged her to lie some time, till a neighbour came accidentally to see her, who with some difficulty got her into bed. The same night, though she was advised to it, she refused to have any one to sit up with her; and at ½ past 11, one Brooks, who

was an occasional attendant, left her as well as usual, locked up her door, and went home. He had placed 2 bits of coal quite backward on the fire in the grate, and put a small rush-light in a candlestick, which was set in a chair, near the head of the bed; but not on the side where the curtain was. At half after 5 the next morning, a smoke was observed to come out of the window in the street; and, on breaking open the door, some flames were perceived in the room, which, with 5 or 6 buckets of water, were easily extinguished. Between the bed and fire-place lay the remains of Mrs. Clues. The legs and one thigh were untouched. Except these parts, there were not the least remains of any skin, muscles, or viscera. The bones of the skull, thorax, spine, and the upper extremities, were completely calcined, and covered with a whitish efflorescence. The skull lay near the head of the bed, the legs toward the bottom, and the spine in a curved direction, so that she appeared to have been burnt on her right side, with her back next the grate. The right femur was separated from the acetabulum of the ischium; the left was also separated, and broken off about 3 inches below the great trochanter. The connection of the sacrum with the ossa innominata, and the inferior vertebræ of the loins were destroyed. The intervening ligaments kept the vertebræ of the loins, back, and neck together, and the skull was still resting on the atlas. When the flames were extinguished, it appeared that very little damage had been done to the furniture of the room; and that the side of the bed next the fire had suffered most. The bedstead was superficially burnt; but the feather bed, sheets, blankets, &c. were not destroyed. The curtain on the other side the bed was untouched, and a deal door, near the bed, not in the least injured. Mr. W. was in the room about 2 hours after the mischief was discovered. He observed, that the walls and every thing in the room were coloured black: there was a very disagreeable vapour; but he did not observe that any thing was much burnt, except Mrs. Clues; whose remains he saw in the state above described. He took away one of the bones (the remains of the sacrum) which he inclosed with this letter. The only way that he could account for it is, by supposing that she again tumbled out of bed on Monday morning, and that her shift was set fire to, either by the candle from the chair, or a coal falling from the grate. That her solids and fluids were rendered inflammable, by the immense quantity of spirituous liquors she had drunk; and that when she was set fire to, she was probably soon reduced to ashes, for the room suffered very little.*

* There are many other instances on record, besides the above, of the combustibility of the human body, in subjects advanced in years and addicted to the use of spirituous liquors. See a paper containing a collection of histories of this kind translated from the Journal de Physique in the 6th vol. of the Phil. Magazine.

XXXVI. Experiments on Animal Fluids in the Exhausted Receiver. By D. Darwin, M. D., of Litchfield. p. 344.

The ancient opinion, that air exists in some of the blood vessels, was exploded by the discovery of the circulation. But many of our modern theorists seem to have conceived, that an elastic vapour of some kind exists in the blood vessels, as they have ascribed the lunar and equinoctial diseases to the variations of atmospheric pressure. This opinion seems to have arisen from observing, that the skin rises, and that the vessels are distended, even to bursting, under a cupping-glass; when the pressure of the atmosphere is taken off from one part, and continues to act on all the remaining surface of the body: and would indeed, at first sight, appear to be demonstrated by the following experiments. About 4 oz. of blood were taken from the arm of one of the attendants, and immediately put under the receiver of an air-pump; and, as the air was exhausting, the blood began to swell, and to rise in bubbles, till it occupied above 10 times its original space.

As false reasoning is, in no science, of more dangerous consequence than in that of medicine, Dr. D. persuaded himself that the removal of this error might be thought worthy the attention of the R. S. In April 1772, Mr. Young, an ingenious surgeon at Shiffnal in Shropshire, and Mr. Waltire, an accurate lecturer in natural philosophy, made, at his request, the following experiments. 1. A part of the jugular vein of a sheep, with the blood in it, was included between 2 strict ligatures, during the animal's being alive, and being cut out with the ligatures, was immediately put into a glass of warm water, and placed in the receiver of an air-pump, it sunk to the bottom of the water, and would not rise when the air was diligently exhausted. It was then wiped dry, and laid on the brass floor of the receiver, and the air again exhausted, but there was not the least visible expansion of the vein, or its contents. 2. A ligature was put round the neck of the gall-bladder of the same animal, as soon as it was slaughtered; the gall-bladder, with the bile in it, was first put into water, in which it sunk, and was placed in the exhausted receiver of the air-pump; and was afterwards wiped dry, and laid on the brass plate at its bottom, as in the former experiment; but in neither case, on the greatest degree of exhaustion, did it show the least alteration of its bulk. 3. The neck of the urinary bladder of the same animal was well secured with a ligature, and contained about 2 or 3 oz. of fluid. The bladder sunk immediately on being put into warm water; but, on exhausting the receiver, many silver-like globules appeared on its surface; and it soon showed manifest signs of expansion, and rose to the top of the vessel. The same experiment was tried with it wiped dry, and laid on the floor of the receiver, and the result was, that its expansion and contraction were very perceptible to the eye.

In January 1773, by the assistance of Mr. Webster, an ingenious surgeon from Montrose, the above experiments were repeated in the following manner. A part of the vena cava inferior of a large swine, which was killed by some strokes on his head with an axe, was intercepted, when full of blood, between 2 ligatures. The part was about 1½ inch long, and held, by conjecture, near an ounce of blood; this was immersed in warm water as soon as it was cut out of the warm body, and immediately put into the receiver of an air-pump. The air was well exhausted, and again let into the receiver repeatedly, without any appearance of enlargement of the vein; which must have been easily perceivable by its ascending in the warm water. The same experiment was tried on the urinary bladder, with the same success, the urethra being tied with a ligature, while it was still in the body. The gall-bladder rose in the warm water, though the bile duct was tied before it was taken out of the body, and had air bubbles appearing on its sides, like globules of quicksilver, as happened to the urinary bladder in the experiments at Shiffnall; which, in both cases, was ascribed to some portion of cellular membrane adhering to the bladders, into the cells of which, at the time of cutting them out, some air had insinuated itself.

In these experiments the water, in which the animal parts were immersed, was warmed to about 100 degrees of Fahrenheit's scale, lest a greater degree of heat in the water might have raised an elastic vapour from these fluids, which did not naturally exist in the living animal; and all the parts were well cleared from the cellular membrane and fat; as it was imagined the atmospheric air might intrude itself into the cellular membrane, as is seen in tearing off the skin of animals recently killed, and which did indeed disappoint 2 of the above experiments, as was manifest from the silvery globules, which appeared on the surfaces of the bladders.

From the facts established by these experiments, Dr. D. thinks the following conclusions may be drawn. 1. That so great a change is produced in the blood, by its receiving, in its passage from the arm of the patient to the basin, a great admixture of atmospheric air, that the experiments afterwards made on its sensible or chemical properties are rendered very uncertain and erroneous; since the florid colour of the blood, its property of coagulation, and perhaps of putrefaction, may depend on this adscititious admixture of atmospheric air, and, at the same time, we see why so much less froth is produced in the operation of cupping, than from blood placed in the exhausted receiver of an air-pump; though perhaps as great a degree of vacuum is made in one case as in the other.

2. It is probable, from these facts, that animal bodies can bear much greater variations of the pressure of the atmosphere, than the natural ones, without any degree of inconvenience. Some who have ascended high mountains are said to have been seized with a spitting of blood; but as this never happens to animals

that are put into the exhausted receiver of an air-pump, where the diminution of pressure is many times greater than on the summit of the highest mountains, it is probable it was an accidental disease, or was owing to some violent exertions in ascending. And in the curious account Dr. Halley gives of his descending in a diving bell so low, as to have the weight of many atmospheres over him, no other complaint is recorded, but a disagreeable sensation, as he was descending, like something bursting in his ears, and which recurred at about the same depth of water in his ascent.

From the above observations of Dr. Halley on the sensation in his ears, when he descended and ascended in the diving bell, Dr. D. was led to imagine, that the air contained behind the tympanum in the vestibulum cochlea, and semicircular canals of the ear, had found or made itself a way into the Eustachian tubes, or into the external ear, by some undiscovered passage; and concluded, that a similar operation might be of service to some deaf people, where the immediate cause of their deafness might be owing to the excess or defect of this internal air. For this purpose a cupping glass, which had a syringe to exhaust it, was put over the ears of three different people, who were very hard of hearing. The inequality of the mammoid process of the temporal bone, made it necessary to put 2 or 3 circles of wash leather dipped in oil around the helix of the ear. On working the air-syringe, the external ear swelled, and became red; and at length the patients complained of pain in the internal ear, and the air was readmitted. One of these 3 patients heard considerably better immediately after the operation, and received permanent advantage; the others received neither benefit nor disservice. If this small degree of success from the use of the cupping glass, as so little pain or trouble attends the operation, should encourage other deaf persons to make use of it, it may be a means to give some light into the intricate diseases of this organ, the structure of the parts of which, and their uses are yet so little understood.

XXXVII. An Account of a Storm of Lightning observed March 1, 1774, *near Wakefield, in Yorkshire, by Mr. Nicholson, Teacher of Mathematics in Wakefield.* p. 350.

On the 1st of March, about half past 6 in the evening, as I was returning from Crofton, a village near Wakefield, I saw, says Mr. N., in the north-west, a storm approaching; the wind, which had been strong all the day, setting from the same quarter; and as in the afternoon of the same day, there had been some violent showers of hail, I made the best of my way to the turnpike at Agbridge. The air was so much darkened, before the storm began, that it was with difficulty I found my way. When I was about 300 yards from the turnpike, the storm began; when I was agreeably surprised with observing a flame of light,

dancing on each ear of the horse that I rode, and several others much brighter on the end of my stick, which was armed with a ferule of brass, but notched with using. These appearances continued till I reached the turnpike-house, where I took shelter. Presently after, there came up 5 or 6 graziers, whom I had passed on the road. They had all seen the appearance, and were much astonished. One of them, in particular, called for a candle, to examine his horse's head, saying, "It had been all on fire, and must certainly be singed."

After having continued about 20 minutes, the storm abated, and the clouds divided, leaving the northern region very clear; except that, about 10 degrees high, there was a thick cloud, which seemed to throw out large and exceedingly beautiful streams of light, resembling an aurora borealis, towards another cloud that was passing over it; and, every now and then, there appeared to fall to it such meteors as are called falling stars. These appearances continued till I came to Wakefield; but no thunder was heard. About 9 o'clock a large ball of fire passed under the zenith, towards the S. E. part of the horizon. I have been informed that a light was observed on the weathercock of Wakefield spire, which is about 240 feet high, all the time that the storm continued.

XXXVIII. Account of a Woman enjoying the Use of her right Arm after the Head of the Os Humeri was cut away. By James Bent, Surgeon, at Newcastle. p. 353.

Mr. White, of Manchester, in the history of an operation performed on the humerus, published in his treatise, entitled, Surgical Cases, with Remarks, and read before the R. S., Feb. 9, 1769; asserts, that he sawed off the upper head of that bone; and that his patient enjoyed the entire use of the joint. As the supposition of the head of the bone, with its ligaments, &c. being regenerated, must appear a little marvellous, and may prevent some from paying that attention to the operation, that it certainly merits, Mr. B. flattered himself the following case would not be unacceptable to the R. S., as it proves, that the operation is not only practicable, but adviseable; and, at the same time, points out the nature of Mr. White's mistake. In pl. 6, fig. 1, he has given a drawing of the bone he cut off; the bare inspection of which is sufficient to convince any one, that it could be only the body of the humerus that was carious, and separated from its epyphysis; as the round head, with its cartilage, is wanting; and Mr. B. believes, there are few instances where the whole head of any bone is so entirely destroyed, in 2 or 3 weeks, by a caries, as that drawing represents. Hence it appears that the joint, with its capsular ligament, remained in a sound state. He is further confirmed in this opinion, by attending to the description Mr. W. has given of his mode of performing the operation, (vide p. 58) where he says, "that he began his incisions at the orifice which was

situated just below the processus acromion." Now as the processus acromion reaches a little over the joint, his beginning his incision below that must, of course, be below the insertion of the capsular ligament.

Mary Turner, a farmer's daughter, of Ipstones, in this county, applied to Mr. B. in October, 1771, on account of an abscess in the joint of her right shoulder, with which she had been afflicted near 3 years. On examining it, he found 3 apertures; 2 near the middle and lower edge of the clavicle; and the 3d, near the insertion of the pectoral muscle into the humerus. By introducing 2 probes, from the upper and lower orifices, they easily met in the joint, the opening into which, through the ligament, seemed to be very small, and he could perceive the head of the humerus carious. As in this case, there seemed nothing to be proposed for her relief, but either to amputate the arm, or by an opening, to cut away the head of the bone. He determined on the latter; and accordingly began an incision from the upper orifice, near the clavicle, and continued it over the joint to the insertions of the pectoral muscle: but finding a single incision too small, to allow him to get at the head of the bone readily, he separated a part of the deltoid muscle from its insertion into the clavicle; and likewise a little of its insertion into the humerus; which gave him liberty to come at the joint, the capsular ligament of which, from frequent inflammation, was so thickened, and kept the head of the bone so close to its socket, that it was with difficulty he could introduce a spatula between them. This likewise, after opening the ligament, prevented the head of the bone from rising out of its socket, on pressing the elbow backward, as is common in performing the operation on a dead body, when the joint is *in* a sound state; so that he was obliged to separate it quite round, before he was able to come at the bone with the saw. He then moved the elbow backwards, and brought the head of the bone over the pectoral muscle, as he found it impossible to saw it directly across, as Mr. White directs, without leaving a considerable portion behind, that had been laid bare with the knife, and which, in all probability, must have exfoliated. By placing a card between the edge of the deltoid muscle and the bone, and the saw within the incision, with its point into the joint, he cut off all that had been deprived of the periosteum, and had no exfoliation; nor had he occasion to take up one artery. As the tendon of the biceps muscle was cut through, he kept the fore-arm suspended. The patient walked from his house to her own lodgings; her pain was not very considerable, and she recovered, by the common treatment, without any bad symptom. She left this town in 6 weeks after the operation.

By using her arm too freely when she got home, the cicatrix was torn open about $1\frac{1}{2}$ inch, which retarded its healing for 3 weeks longer; but from that time she had remained well. She had the perfect use of the fore-arm; could

raise her elbow about 5 or 6 inches from her side, put her arm back, lace her stays, put on her cap, sew, and do any business, as well as ever, that does not require the elbow to be more raised. The upper end of the humerus played about an inch below the point of the scapula; and the processus acromion and coracoides appeared on each side of the cicatrix, at nearly equal distance. He mentioned this only to point out more exactly the course of the incision.

XXXIX. Continuation of an Experimental Inquiry concerning the Nature of the Mineral Elastic Spirit, or Air, contained in the Pouhon Water, and other Acidulæ. By W. Brownrigg, M. D., F. R. S. p. 357.

Prop. 1. *The ferruginous and absorbent earths, contained in the Pouhon water, are kept dissolved in it, by means of the mephitic air* to which those earths are united.*—In an inquiry concerning the nature of the mineral and elastic spirit, or air, contained in this water, published in the Trans. of the R. S., vol. lv. [Abrid. vol. xii, p. 235] it has been shown, that when the Pouhon water is excluded from all contact with the common air, in such manner that the mephitic air which it contains has free liberty to fly from it into an empty bladder, this air does not separate from the water by any spontaneous motion, as it would from its rare texture and elastic force, were it at liberty to exert these its qualities: but, on the contrary, in this situation, it remains united to the other ingredients of the water, when exposed to the most intense heat that we usually observe, in the open air, in this our climate. It has been further shown, in the 2d experiment, that this elastic fluid, when excluded from common air, in the manner before related, is but slowly expelled from the Pouhon water by a heat of 110 degrees of Fahrenheit's thermometer, though such heat is sufficient to raise water, a much heavier body, in distillation: and so closely is air united to the other ingredients of the water, that it is not wholly expelled from them by a scalding heat of 160 or 170 degrees of the scale, when exposed to it for 2 hours.

Which experiments therefore prove, that this air is not detained in the Pouhon water by the pressure of the atmosphere, or by any other external force, as is the air with which beer, or other fermenting liquors, are often surcharged, while they are confined in bottles; but that this elastic fluid is equally mixed with the watery element, and with the other ingredients of which this mineral water is composed, and exists with them in a state of solution, or in a fixed state, being attached to the water, and to the other ingredients dissolved in it, by a force sufficient to keep them all united together in one uniform compound, while this force is not removed by some external cause.

It further appears, from the same experiments, that so long as this air continues united to the other ingredients of the Pouhon water, its martial and

* Now termed Carbonic Acid Gas.

absorbent earths do also remain suspended in it; but, so soon as any part of this air is expelled by heat, those earths begin to separate from the water, which then becomes white and turbid; and when, by continuance of the heat, more of this air is expelled, more of the earthy particles also separate from the water, in the same proportion as its air is separated from it; and while only a small portion of the air remains, some portion of the martial earth also remains dissolved in the water, as appears from its giving a slight tinge of the purple, when mixed with galls: but none of these earths are any longer detained in the water, than while it continues impregnated with some mephitic air: when this air is entirely separated from the water, it is wholly decompounded, having lost its distinguishing brisk and pungent taste, and its power of striking a purple colour with galls; its more volatile and elastic principles being exhaled, its metalline and absorbent earths then subside in a white flocculent sediment, and no other substance remains dissolved in the water, save only the small portion of alkaline and neutral salts, which enter its composition.

From this short recapitulation of the abovementioned experiments, it therefore appears, that the Pouhon water undergoes a decomposition when its air is expelled from it by means of heat. The opposite extreme of cold is also found to produce the same effect of decompounding the Pouhon water, when this its aërial principle is expelled from it by means of congelation. For having poured some of this water into open tin vessels, that were placed in the common freezing mixture of sea salt and snow, as soon as the water began to shoot into ice, at the bottom and sides of the vessels, very minute bubbles of air incessantly arose in it, and were discharged from its surface with such force, as to carry with them small particles of the water to a considerable height; and continued thus to fly off, till all the water was congealed. The ice was very white, from the minute bubbles of air, which were every where interspersed through it, and by which the frozen water considerably increased in bulk, so as to rise at its surface into a very convex form. The water, when thawed, was white and turbid, and soon let fall its metallic and absorbent earths in a white sediment: it then had almost lost all its taste; and, being mixed with tincture of galls, only gave a slight purple tinge. By a 2d congelation, it seemed almost entirely deprived of its air, and, with it, of the remaining part of its white earths; and, when decanted from its sediment, no longer struck a colour with galls. From these experiments it therefore appears, that as soon as this water is deprived of its air, whether it be by heat or by cold, it is no longer capable of keeping those earths dissolved, which, while it is impregnated with this air, continue suspended in it.

In these decompositions of the Pouhon water, by heat and by cold, no volatile spirit, either acid or sulphureous, nor any other subtile matter, has been found to fly from it, save only its mephitic air: while this air is present in the

water, its martial and absorbent earths remain dissolved in it; as soon as this air is separated from the water, in whole or in part, those earths, either in the whole or in part, do also separate from it, and are no longer suspended in it, than while they are united to a due proportion of this aërial solvent. Whence it appears, that this mephitic air is the medium by which the metalline and absorbent earths, contained in the Pouhon water, are held in solution; and, contrarywise, that those earths are the medium, by which this air is more firmly united to the watery element in this compound, in which it enters as a principal ingredient, and, by its solution in the water, and its union with these earthy substances, from a very rare volatile and elastic body, is reduced to a fixed state.

This dissolving power of mephitic air may further be proved from the recomposition of the Pouhon water, by adding to it the air expelled from it by coction. But as Mr. Cavendish has already shown, that the absorbent earths of Rathbone Place water may be redissolved by the mephitic, or fixed air, which had been extracted from that water; and as Mr. Lane has also demonstrated, that iron is rendered soluble in water, by the medium of mephitic air, Dr. B. did not think it necessary at that time to give an account of his experiments on the same subject; but as those experiments contain some phenomena that have not yet been noticed, he may perhaps offer them to the public on some future occasion.

Schol. 1.—From the foregoing experiments, it appears that the mephitic air and martial earth, contained in the Pouhon waters, strongly attract each other, and, uniting together, form a concrete soluble in water, and readily distinguished in it, by the peculiarly brisk acidulous taste, which it receives from this aërial principle, joined to a rough subastringent taste, which proceeds from the iron. This concrete, like other vitriols of iron, strikes a black colour with galls, and may well be esteemed a saline body of the neutral kind, of which the mephitic air constitutes the spirituous solvent, and the martial earth its base. It further appears, that the mephitic air is possessed of all the properties, by which some of the chemists have distinguished those pure and simple bodies, or spirits, which by them are esteemed, in their own nature, and of themselves, saline, and which, in union with other bodies, form salts that are more compound. For this aërial solvent, in like manner with the pure acid spirits, is soluble in water, and imparts to it its peculiar sharp and acidulous savour: also, in combination with various metalline and absorbent earths, this volatile elastic spirit, like those acids, forms various saline concretes of the neutral kind; inasmuch as those metalline and absorbent earths, when united to this elastic spirit, are thus rendered soluble in water; and, in union with it, acquire peculiar savours, resulting in part from this their spirituous principle, and in part also from the particular kind of earth with which it is combined. This air therefore, considered in the relation which it bears ot several earthy substances, and to water, considered also as it impresses

the organs of taste, with its peculiar brisk and acidulous savour, may justly be stiled a mineral elastic spirit of a saline nature, and is sufficiently distinguished from all other saline spirits, by its great rarity, and by its aërial nature. How far, and under what laws, this relation between mephitic air and various saline earths, and other bodies, may be extended, has not yet been fully discovered: suffice it in this place to remark, that a class of saline bodies of a neutral nature are here detected, composed of various earthy bases, united to a volatile aërial spirit, all of which agree in one common solvent, the mephitic air, but differ from each other, according to the nature of the base to which this air is united.

The agreement of these saline concretes with neutral salts in these essential properties, by which these last are distinguished from other more simple saline bodies, will further appear from their decomposition; which is effected by those various ways, and under the same laws, by which all other neutral salts are decompounded; namely, by all those different ways, by which the acid spirits, and the terrene or alkaline bases of neutral salts, can be separated from each other.

For, first, the aërial spirit of these saline concretes, is forced, by fire, from its union with the earthy base, which it holds dissolved in water, in like manner as the acid spirit of other neutral salts are expelled by fire from the more fixed principles, which enter the composition of those salts. The degree of heat required to separate the acid spirit of neutral salts, from their more fixed alkaline or earthy base, varies in the decomposition of almost every different kind of salts; and the extreme volatility and expansive force of this aëreo-saline principle renders it more easily separable, by heat, from the fixed principles to which it unites, than any other kind of saline spirit.

Secondly. The saline concretes, formed with this aërial solvent (in like manner as other neutral salts), are decompounded by the addition of stronger acids, which more powerfully attract the terrene or metallic base of these concretes, than it is attracted by their light and subtle aërial spirit, and detaches from them the aërial solvent to which those earths were before united. All acids, found in a liquid form, have this effect from the light vinous acids to the most ponderous acid of vitriol; so that the affinity between these metalline and absorbent earths, and this their aërial solvent, is less than that which exists between the same earths and all the known acid spirits. In all additions of these acids to the spirituous or acidulous waters, an effervescence has been observed, not readily accounted for, by those who suppose an acid to predominate in those waters. The conflict and discharge of air here arises from the expulsion of the aërial principle from its terrene base; in like manner as the acids of sea salt and nitre are expelled, with effervescence, from their alkaline bases, by the more powerful

acid of vitriol. And here, by the way, it may be proper to remark, that the vitriolic acid, when mixed with the acidulæ and other chalybeate waters, does not preserve those waters from decay, as Hales, and others, after him, have supposed; but, on the contrary, destroys their texture, or decompounds them, by expelling their elastic spirit, and entering into new combinations with their earthy principles; thus forming a new compound, less perishable indeed than the former, but also less efficacious in the cure of many diseases. When Rhenish wine is added to the acidulæ, the large quantity of air that flies off may, in part, proceed from the wine; but when Dr. B. mixed the vitriolic acid with Pouhon water, a considerable quantity of air was indeed discharged; but not the whole which that water holds in solution. He therefore conjectured, that some part of the air, contained in that water, might be imbibed by the superabundant acid, which he used in the experiment, and that more mephitic air might perhaps have been expelled from the water, had he only mixed with it the exact quantity of this acid, that was required to dissolve the earthy substances contained in it.

Thirdly. These saline concretes, contained in the Pouhon water, and other acidulæ, are subject to decomposition, not only from acids, as before related, but also from alkalies, whether fixed or volatile: all which more powerfully attract this subtile aërial principle than it is attracted by the martial and absorbent earths, to which it is united in those waters. And here again appears an exact agreement between these aëreo-saline concretes, and various neutral salts, in the mode of their decomposition. For the ammoniacal salts (which are all composed of the volatile alkali, united to an acid spirit, either muriatic, nitrous, or of some other kind) as soon as one of the fixed alkalies, or quicklime, is added to any of them, the acid spirit which it contains, quitting its union with the weaker volatile alkali, this last is let loose; and the stronger alkali, or quicklime, takes its place; between which and the acid spirit a new combination is formed. The same happens when any alkali, either fixed or volatile, is added to the acidulæ; their elastic spirit then quits the ferruginous and absorbent earths, to which it was joined, and forms a new combination with the alkali, by which it is more powerfully attracted than by these earthy substances. These earths therefore, being no longer suspended in the water by the aërial solvent, render it turbid and milky, until they have gradually subsided in it, in the form of a white sediment: for such is the native appearance of the martial earth, as well as of all the other earths contained in these waters, as will be shown hereafter. In these decompositions of acidulous waters, by means of alkalies, no effervescence, or discharge of air bubbles, takes place; for here the air is all absorbed by the alkali added, and not expelled from the water, as it is in the decomposition of the same waters, by means of stronger acids.

When the acidulæ are mixed with common soap, a two fold decomposition

takes place. The fixed alkali, quitting the unctuous substances, to which it was joined in the soap, unites itself to the aërial spirit, or mephitic air, of those waters, while this air, at the same time, deserts the earthy substances with which it was before combined. The same new combinations seem to take place, when soap is mixed with any of those waters which are usually called hard; many of which waters have been found to contain an earthy substance, dissolved by means of this subtile aërial principle.

The above observations and experiments show an exact agreement, in the several ways by which the various neutral salts, and those saline concretes, formed of mephitic air united to an earthy base, are decompounded. It ought however here to be remarked, that the saline concretes, which exist in the Pouhon water, in a dissolved state, though evidently of the neutral kind, have not hitherto been obtained in a solid form; owing perhaps, in some measure, to the great volatility of their spirituous principle; but chiefly to their being subject to decomposition, from the precipitation of their earthy base, by means of common air, during the evaporation of the water in which they are dissolved, as will be shown hereafter.

The mephitic air of the acidulæ, though it is soluble in water, and imparts to it its brisk and pungent taste, which has been usually stiled subacid; and though it produces effects exactly similar to those of acid spirits (by readily uniting to various earthy substances, which of themselves are not soluble in water, but, by their union with this aërial fluid, are rendered soluble in it, and communicate to the water peculiar savours, and form in it saline concretes of the neutral kind; which concretes, so formed, are again separable into their component ingredients, by all those ways by which the acid and alkaline principles of other neutral salts are separable from each other) yet it differs from all acid spirits, found in a liquid form, in its rare texture and in its elastic quality, and in not striking a red colour with syrup of violets, and other blue tinctures of vegetables; which change, in the blue colour of those tinctures, is usually esteemed a test of the presence of an acid. Besides the trials which have been made, by mixing syrup of violets with pure water, impregnated with various kinds of mephitic air, in which no change in the colour of the syrup was observed, he had for several days suspended pieces of linen, that had been dyed blue with fresh juice of violets, in the mephitic air of spa water, and also in that of chalk; and, when the linen was taken out of the said air, did not perceive its blue colour in any wise changed, though the same pieces of dyed linen were instantly turned of a green colour, when exposed to the fumes of spirit of hartshorn. Whether therefore, and under what relations, this aëreo-saline spirit may merit the title of an acid, he leaves to the determination of others. Such however it has appeared to be to many philosophers, since this mephitic air is doubtless the same with the acidum vagum fodinarum of Boerhaave and others; and with the

acidum centrale perpetuum inexhauribile of Beccher; with the spiritus sulphureus aëreo-æthereo-elasticus of Hoffman; and the sal embrionatus and sal esurinus of the sagacious Helmont, which, he says, corrodes the ore of iron, and with it forms a volatile vitriol in the Pouhon water. All these, and many other philosophers, had acquired some knowledge of this subtile aëreo-saline principle from contemplating its effects; but, not having obtained it in a palpable form, were unacquainted with several of its principal properties.

From considering the great subtilty of this aëreo-saline principle, its power of dissolving many earthy substances, with its property of uniting readily to water, and with it, of pervading the very minute vessels of the animal frame, without injuring them, as stronger acids do by their corrosive quality, we may thence form some judgment of the great efficacy of this air, as a de-obstruent and solvent, in many diseases of the human body, arising from preternatural concretions and obstructions thence ensuing. If to these we add the geat antiseptic powers of this kind of air, which it possesses in common with acids, and which were first detected by Sir John Pringle, and have since been more fully explained by Mr. Macbride and Dr. Priestley; we then, in some measure, may account for those extraordinary effects which this kind of air is found to produce, in the cure of many obstinate diseases, with which mankind are afflicted.

XL. Particulars of the Country of Labradore, extracted from the Papers of Lieut. Roger Curtis, of H. M. Sloop the Otter. p. 372.

This vast tract of land is extremely barren, and quite incapable of cultivation. The surface is every where uneven, and covered with large stones, some of which are of amazing dimensions. There are few springs; yet throughout the country there are vast chains of lakes or ponds, which are produced by the rains, and the melting of the snow. These ponds abound in trout, but they are very small. There is no such thing as level land. It is a country formed of frightful mountains, and unfruitful vallies. The mountains are almost devoid of every sort of herbage. A blighted shrub, and a little moss, is sometimes to be seen on them; but in general the bare rock is all you behold. The vallies are full of crooked low trees, such as the different pines, spruce, birch, and a species of the cedar. Up some of the deep bays, and not far from the water, it is said however there are a few sticks of no inconsiderable size. In short, the whole country is nothing but a vast heap of barren rocks.

The climate is extremely rigorous. There is but little appearance of summer before the middle of July; and in September the approach of winter is very evident. All along the coast there are many rivers, which empty themselves into the sea; yet there are but few of any consideration, and you must not imagine that the largest are any thing like what is generally understood by a river. Cus-

tom has taught us to give them this appellation, but the most of them are nothing more than broad brooks, or rivulets. As they are only drains from the ponds, in dry weather they are every where fordable; for running on a solid rock, they become broad, without having a bed any depth below the surface of the banks.

There is no variety of animals in this rocky country, nor are they at all numerous. Here are the rein-deer; the females have horns, which nature has given them to procure food, for with these they beat away the snow in winter, and by that means come at the tops of trees, which, during the inclemency of that season, is their only sustenance. There are bears black and white, wolves, the carkashew, foxes, porcupines a great many, the mountain-cat, martins, beavers, otters, hares, and a few ermine. A venomous reptile or insect, is not to be found here, except toads, and these are extremely rare. The whole country is filled with very small flies, which are exceedingly tormenting. Here are eagles, hawks, the horn-owl, and the red-game, with a smaller sort which resemble them, called the spruce-partridge: these we may call the constant inhabitants of the feathered kind. Of sea-birds, there are a great variety.

In the summer the woods are visited with many sorts of little birds, and some of them of beautiful plumage. They breed here, but towards winter they seek a happier climate. In the autumn there come a prodigious quantity of curlews. They are about the size of a woodcock, shaped like them, and nearly of the same colour; extremely fat, and most delicious eating. They continue here but a very little while, nor is it known whence they come, or whither they go. The principal fish are whales, the cod-fish, and salmon. Of shell-fish, there are but few sorts, and these in no great plenty. Lobsters there are none at all; which is very remarkable, for at a particular part in the Straits of Bellisle, not more than 5 or 6 leagues from Newfoundland, there are great abundance.

It is not surprizing that such a country as this should be thinly inhabited. The human species upon this extensive territory are but few; and such as we know of are extremely savage. The people of this country form various nations or tribes; and are at perpetual war with each other. Formerly the Esquimaux, who may be called a maritime nation, were settled at different places on the sea-coast, quite down to the river St. John's; but for many years past, whether it has been owing to their quarrels with the mountaineers, or the encroachments of the Europeans, they have taken up their residence far to the north. A good way up the country are people distinguished by the appellation of mountaineers, between whom and the Esquimaux there subsists an unconquerable aversion. Next to the mountaineers, and still farther westward, is a nation called the Escopics: and beyond them, are the Hudson Bay Indians, with whom the world is but little acquainted. There are doubtless, in such a vast tract of land, a

great number of other nations, but of whom we have not the least information.

The mountaineers are esteemed an industrious tribe; and for many years had been known to the French traders. Their chief employment is to catch fur, and procure the necessaries of life. They are extremely illiterate, but generally good-natured; and are reckoned to be less ferocious than any other of the Indians. They come every year to trade with the Canadian merchants, who have seal fisheries on the southern part of the coast, and have the character of just dealers. They are immoderately fond of spirits; for which, with blanketing, fire-arms, (in the use of which they are remarkably dexterous), and ammunition, they truck the greatest part of their furs. Their canoes are covered with the rind of birch; and though so light as to be easily carried, yet sufficiently large to contain a whole family and their traffic. By means of the multitude of large ponds throughout this country, they convey themselves a vast distance in a very little time. Whenever they find a pond in their way, they embark on it, and travel by water; when its course alters, and by following it they would lengthen their distance any thing considerable, they land, place their canoe on their head, and carry their baggage on their shoulders, till other water gives them an opportunity of reembarking. They are most excellent travellers. They bear inconceivable fatigue with astonishing patience, and will travel 2 days successively without taking any sort of nourishment. These Indians are of a deeper colour than the Esquimaux; and are low of stature. Though of a robust constitution, their limbs are small, and extremely well adapted to the rocky country they are continually traversing. They have no hair, except on the head. For many years they have dressed their food, which they boil to a jelly; whereas the other Indians eat every thing raw. It is their custom to destroy the aged and decrepid, when they become useless to the society, and burthensome to themselves. They have been questioned on this seeming inhumanity; and perhaps their reasons are not totally devoid of sound philosophy. They tell you, that as it is with difficulty they procure the necessaries of life, they can admit of none who do not contribute towards acquiring it; that having no fixed residence, and it being impossible to carry the helpless with them, as they are obliged to be continually traversing the country; they ask, if it is not better to put an end to miserable beings, than suffer them to perish with cold and hunger? The son generally does this kind office for the father; and, it having ever been a practice among them, they wonder at our considering it as an act of inhumanity.

The Esquimaux Indians, inhabiting the sea-coast of the northern part of Labradore, are doubtless from Greenland. They are a very deep tawny, or rather of a pale copper-coloured complexion. They are inferior in size to the generality of Europeans; and but a few among them are of good stature. They

bear a very near resemblance to the Laplanders, both in their persons and customs. They have beards, so have the Greenlanders, and indeed so have the inhabitants of Lapland; whereas the Iroquois, the Hurons, the Escopics, and the Mountaineers their neighbours, have hair no where except on the head. These Indians, in general, are not very disagreeably featured, though some among them are extremely ugly. They are flat-visaged, and have short noses. Their hair is black and extremely coarse. Their hands and feet are remarkably small. The women load their heads with large strings of beads, which they fasten to the hair above the ears; and they are fond of a hoop of bright brass, which they wear as a coronet. Their dress is entirely of skins, except those who have trafficked for a little blanketing. It consists of a sort of hooded close shirt, breeches, stockings, and boots. They wear the hairy side towards them, according to the seasons; and between the dress of the different sexes there is no variety, except that the women wear monstrous large boots, and their upper garment is ornamented with a tail. In the boots they occasionally place their children; but the youngest is always carried at their back, in the hood of their jacket. They have no sort of bread; but live chiefly on the flesh of seal, deer, fish, and birds. Till very lately they ate every thing raw, and putrefaction was deemed no objection.

In the winter they live in houses, or rather caverns, for they are sunk in the earth. In the summer they dwell in tents, which are made circular with poles, and covered with skins sewed together. The house consists of one room, and though not very large, yet it contains several brothers or other relations, with their wives and children. Their tents are still more crowded; because, as the whole summer they are generally rambling up and down the coast, they endeavour to diminish their baggage as much as possible. They are without any government; and no man is superior to another, but as he excels in strength or in courage, and in having the greatest number of wives and children. Being entirely without laws, general censure is the only punishment for the most detestable crimes. They have no marriage ceremony. A wife is considered as property, and a husband lends one of his wives to a friend. The wives are given very early in marriage, frequently several years before consummation; and the reason of this is, because the girl's father, by that means, has one less in family to provide for.

The Esquimeaux men are extremely indolent; and the women are the greatest drudges upon the face of the earth. They do every thing except procure food, a d even in that they are frequently assistants; so that they are at continual labour. They sew with the sinews of deer, and their needle-work is amazingly neat. Their language is the same as the Greenlanders. It is not altogether devoid of harmony, and the women have very delicate voices. These Indians

are strangers to jealousy; they do not appear to be at all quarrelsome, and they very seldom steal from one another. They do not seem very passionate; but woe be to the woman that offends her husband. If polygamy was not allowed among them, their numbers would be very few. Some of the women bear many children; but, in general, they are by no means fruitful. The wives live happily together; and, if deserving, share equally in their husband's favours. These Indians cannot reckon numerically beyond six; and their compound numbers reach no farther than twenty-one. Every thing beyond is a multitude. They navigate their shallops without a compass in the thickest fogs, and are very good coasters. They have always a vast number of dogs in their camp, which are of several uses. These animals serve as a guard; they are food; their skins are valuable for cloathing; and they draw their sledges in winter. They have not the power of barking, but their howl is hideous; they are large, and have a head like a fox, whereas the dogs of the Mountaineers are extremely small. The Samojedes and the Laplanders train the rein-deer to their sledges. The country of Labradore produces these animals; but they are only serviceable to the Esquimeaux for food and raiment. The weapons of these Indians are, the dart and the bow and arrow. They are not very expert in the use of either; though it is with these they defend themselves, and procure the necessaries of life.

As to their population, the Esquimeaux inhabitants of Labradore are far from being numerous, but little exceeding 1600; and those savages who inhabit the inland parts are still less numerous.

XLI. An Account of some New Experiments in Electricity; containing, 1. An Inquiry whether Vapour be a Conductor of Electricity. 2. Some Experiments to ascertain the Direction of the Electric Matter in the Discharge of the Leyden Bottle: with a New Analysis of the Leyden Bottle. 3. Experiments on the Lateral Explosion, in the Discharge of the Leyden Bottle. 4. The Description and Use of a New Prime Conductor. 5. Miscellaneous Experiments, made principally in the years 1771 and 1772. 6. Experiments and Observations on the Electricity of Fogs, &c. in pursuance of those made by Thos. Ronayne, Esq. By William Henley, F.R.S. p. 389.

§ 1. *An Inquiry whether Vapour be a Conductor of Electricity.*

Exper. 1. I insulated a glass funnel (pl. 11, fig. 1,) into which the streams, from a capillary tube, were directed by the electricity. From this funnel, the electrified drops were received into a large insulated earthen dish; across which lay a long wire; and from its end hung a pair of light cork balls. On working the machine, after about 90 or 100 turns of the winch, and when fifty or sixty drops had fallen into the dish, the balls separated, and presently diverged, to the

distance of half an inch. Then taking off the electricity from all the bodies concerned, I blew the column of water out of the capillary tube, replaced it in the bucket, pointing towards the funnel as before, and worked the machine again, to try whether the electricity, issuing from the syphon, and passing through the air, might not electrify all the bodies, so as to separate the balls, without the jet of water; but no such event happened. I then replaced it, with the jet falling into the funnel as before; when it succeeded. I then tried it a second time, without the jet of water; and it failed. I thus repeated the experiment alternately, with, and without the jet, taking off the electricity of the apparatus carefully between the trials; till I was perfectly satisfied that the jet of water, received into the funnel, and thence falling into the insulated dish below, was the medium by which the balls, hanging from the end of the wire placed in it, became electrified. Hence I inferred, that vapour from boiling water, &c. must also be a conductor of electricity, though probably in a less degree, as being more dissipated. Having since repeated this experiment by receiving the electrified jet immediately into a large insulated dish, I observed the effect to be much greater.

Exper. 2. Having procured a tin vessel, somewhat resembling an eolipile, or chymical retort; I placed it over a small lamp, on my prime-conductor, (fig. 2,) and filled it about half full of boiling water. The nose of it was so situated, as to throw the electrified drops into an insulated dish, furnished with balls, as in the former experiments. After the water had been some time poured in, and I imagined enough had evaporated to have produced some drops in the neck; I examined the lip, to see whether any descended, but saw none. However, on giving the machine a turn or two, I was very agreeably surprized to see the electric streams issue exactly as from a capillary tube; and a few drops having fallen into the dish, the balls became electrical, and were attracted by my finger, at the distance of a half or three quarters of an inch. In a few turns more of the globe, they separated half an inch. I then threw out the water; and, clearing the vessel of its vapour, I remounted it on its stand, pointing towards the dish as before, to try whether the sharp edge on the lip of the vessel would not electrify the air sufficiently to separate the balls, as the evaporated water had done. I turned the winch a long time for this purpose; but the balls never diverged at all. I then poured in the boiling water a second time; and, when the drops began to fall, the 4th turn separated the balls; and the 10th caused them to diverge to the distance of half an inch; and in this state of repulsion they continued a considerable time after I had ceased to work the machine. I then took off the electricity with my finger, and again cleared the vessel of its water, &c. and having replaced it with the point as before, I worked the machine again as usual. The air was now become in some measure electrical; for, at the

7th or 8th turn the balls began to separate, and in 40 turns they were about $\frac{3}{8}$ of an inch distant from each other. I then ceased to turn the winch any longer; but had no sooner stopped, than the balls began to close, and in a very few seconds they were in contact; whereas, in the former experiment, when the electrified drops were in the dish, on my ceasing to turn the globe, they showed no sign at all of converging; and I imagine would have remained separate a long time, if I had not taken off their electricity with my finger. I apprehend, therefore, from this experiment, that the vapour of hot water is a conductor of electricity.

Exper. 3. I hung on a string, as near to the ceiling of the room as I could, a pair of pith-balls, which, on working the machine a considerable time, diverged $\frac{3}{4}$ of an inch, but no wider. Then sticking into the conductor a smoking deal match, and working the machine again, they presently separated to the distance of 2 inches. The match, when placed in the same situation, and not smoking, had no such effect.

Exper. 4. Having placed an earthen half-pint mug on a stand, properly insulated; I fixed to a large ball of brass, placed in the bottom of it, the end of a wire, 6 or 8 feet in length. The other end of the wire connected with the prime conductor of a small electrical machine, fig. 3. Over this mug, and as near to the ceiling of the room as might be, I suspended a pair of light cork balls. Then filling up the vessel with boiling water, I began to work the machine; and in 50 or 60 turns of the winch, observed the balls to separate $\frac{4}{8}$, or half an inch, from each other. I then took off the electricity of the bodies, emptied the vessel, and cleared it of the vapour; and having placed the apparatus in the same manner, I again worked the machine, for a longer time, but without effect. On replacing the boiling water, I succeeded as at first. At other times, when I have been able to separate the balls by the air alone, to a small distance, yet by pouring in the hot water, the vapour has presently increased their divergence from $\frac{1}{8}$ or $\frac{3}{16}$, to half an inch distance, or in that proportion, according to the state of the atmosphere with respect to dryness or moisture. In short I have repeated these kinds of experiments so often, and many times with so much success, that there can be no doubt of vapour being a conductor of electricity.

Exper. 5. I insulated the rubber of the machine, and hung a pair of Mr. Canton's balls on the prime-conductor. On working the machine, and taking off a spark, or two, to draw off the electricity naturally inherent in the rubber, &c. I observed the divergence of the balls, which was very great, insomuch that the strings were bent: and on approaching the back of the rubber with a smoking green wax taper, just blown out, (the smoke of which was instantly attracted to it,) they diverged no wider. I then took off the balls, and

placed my own electrometer in its stand, on the prime-conductor, fig. 4; and having taken off a spark or two, as before; I again worked the machine, to observe the repellency of the index from the stem; and found it constantly to vibrate between 5 and 10 degrees of the quadrant, which was divided into 15. I then brought the smoking taper within 4 or 5 inches of the back of the rubber, as before; and observed, that on the attraction of the smoke to it, the index presently began to rise, and in a very short time got up to right angles. I repeated the experiment several times, with the same success. I then tried the experiment by bringing my finger to the same distance from the rubber, and pointing towards it; but this, in many trials, had not the least effect. The taper likewise, when held at the same distance, and not smoking, had no effect at all. I am convinced therefore, that the smoke was the medium which conveyed the electricity from my hand to the insulated rubber.

Exper. 6. I placed on a stand, on the prime-conductor, a piece of smoking wax taper, fig. 5, when immediately on working the machine, the smoke, from a large and diffused volume, was much contracted, and its motion upwards greatly accelerated. I then took off the electricity of the conductor, and held a pair of cork balls a quarter of an inch diameter, hung on threads 2¼ inches long, perpendicularly over the rising smoke; and as high as I could possibly reach, standing on a chair; this might raise the balls about 5½ feet above th prime-conductor; when, working the machine, in a few seconds the balls separated to half an inch distance. I then removed the taper, but could not perceive that the balls were at all affected without it; but on replacing it, they separated as before. I repeated the experiment several times, with and without the taper, and the different effect was constantly as above recited. I then set a tin saucer on the stand, and placed on the saucer a half pint mug of boiling water, fig. 6; and over this water I presented the balls in the rising vapour; as had before been done in the smoke. On working the machine a few seconds, the balls diverged to the distance of the 12th part of an inch. On removing the water, and presenting the balls as before, they never separated at all, though I worked the machine for a longer time; but on replacing the water, in a few seconds the balls diverged as at first. These experiments I repeated several times, and always with the same success. The smoke therefore, in the first experiment, and the vapour of the hot water in this last, was certainly the medium which conveyed the electricity from the prime conductor to the balls: and I think I may now very safely pronounce, that smoke, and the vapour of hot water, are absolutely conductors of electricity; though smoke is a far better one than the vapour of hot water, and both of them are exceedingly bad ones.

§ 2. *Of the Direction of the Electric Matter, in the Discharge of the Leyden Bottle.*

Exper. 1. Light a small wax taper, and place it, with the flame exactly be-

tween 2 brass balls, A and B, about 2 inches asunder; properly introduced into the circuit, fig. 8. Then having given a small phial 2 or 3 turns of the globe, charging it positively, connect the coating of it, by a chain, with the wire of the ball A; and on applying the knob of the phial, to the wire of the ball B, you will observe the flame to be plainly driven from it; being often blown upon the ball A, so as to blacken it with the smoke. Then charge the phial negatively, and, the apparatus remaining as before, apply the knob of the phial as at first; and you will then perceive the flame to be blown quite in the contrary direction, viz. from A towards, and often upon B, as on Dr. Franklin's principles of the Leyden bottle, it ought to be.

Exper. 2. Charge a large jar positively, and insulate it; then take a long curved wire, pointed at both ends, and hold it by a glass handle, so as to bring one end of the wire half an inch from the knob, and the other end of it to the same distance from the coating of the jar. You will then observe a small luminous spark on the point opposed to the knob of the jar, and a fine pencil, diverging from the lower point, spreading on the coating of the jar, which will presently discharge it silently. Then charge the jar negatively; insulate it, and apply the wire as before; and the appearances at the points of the wire will be directly reversed; plainly demonstrating the direction of the electricity in the discharge of the bottle.

Another very convenient and easy method, of exhibiting the phenomena of the positive and negative electricity of the inside and outside surfaces, of a charged Leyden bottle, is by slipping a cap of metal, furnished with a ball and wire, on the outside coating; and mounting it on an electric stand, in a horizontal position, as fig. 12; or if the bottom of the glass be turned much upward into the body of it, a piece of wood may be worked to its shape, and cemented to it; then through the middle of this wood, a short tube of metal may be inserted, so as to admit the wire which is connected with the ball to pass through it; and be brought into contact with the coating of the jar, at pleasure. By this means, experiments may be made, at either end of the bottle, with great facility; and other charged or exhausted bottles, excited ribbons, or other electrics: the curved pointed wire, &c. &c. may be readily applied; and give or receive a spark; be attracted or repelled; according to the kind of electricity in the two bodies so applied towards each other. By hanging a chain round either of the wires, and connecting it with one end of the discharging rod; and bringing the other end of the rod so as to leave a proper space between that and the ball on the wire, at the opposite end of the bottle; the flame of a taper, &c. may be interposed, and show the direction of the electricity in the discharge: or a cork-ball, hung by silk, may play between them, in the manner described by Dr. Franklin. If the balls are taken off from the wires of the bottle, the

wires being pointed, and one of them placed before the globe, or a prime conductor, electrified positively, the phenomena of charging the Leyden bottle will be discovered by the different appearances at the ends of the wires, as at fig. 13. If the bottle be thus placed before a conductor electrified negatively; or the insulated rubber to a machine; the appearances at the ends of the wires will be reversed, as on Dr. Franklin's principles they ought to be; and thus explain his theory of the Leyden phial.

But a more simple, and beautiful analysis of the Leyden phial, hath not perhaps been exhibited, than the following. Let a bottle that will hold near a pint, having a long neck, about an inch in diameter, be furnished with a small plate at the top; with a valve properly secured, after the bottle is exhausted: from which plate, a wire about the 8th of an inch in diameter, is to project a little below the neck, and terminate with a blunt end. The top is to be covered with a round brass cap, firmly fixed on, and made air-tight. The bottom of the bottle should be coated with tin foil, which should be continued 3 inches up the side. This bottle will charge and discharge several times in a minute; and the tin foil coating will prevent the shock from affecting the hand of the operator. The phenomena of charging the Leyden bottle is elegantly explained by this contrivance, and is made visible by the end of the wire, on which the appearances vary, according as the bottle is charged, viz. positively, or negatively; or as the conductor from which it is charged, is electrified. Fig. 14, letter A, shows such a bottle, charging negatively, at a conductor loaded with positive electricity. Letter B shows the same bottle charging positively, at the same conductor. Fig. 15, letter C, shows the bottle charging positively at a conductor electrified negatively; or at the insulated rubber. Letter D shows the same bottle, charging negatively at the same conductor.

§ 3. *Of the Lateral Explosion in the Discharge of the Leyden Bottle.*

Exper. 1. Having made a double circuit, the first by an iron bar, $1\frac{1}{2}$ inches in diameter, and half an inch thick; the 2d by $4\frac{1}{2}$ feet of small chain; on discharging a jar, containing 500 square inches of coated surface; the electricity passed in both circuits, sparks being visible on the small chain in many places. On making the discharge of 3 jars, containing together 16 square feet of coated surface, through 3 different chains at the same time, fig. 16, bright sparks were visible in them all; and I have not the least doubt but it would have been visible in as many more. The chains were of iron and brass, of very different lengths, the shortest 10 or 12 inches, the longest many feet in length. When those jars were discharged through the iron bar before mentioned, together with a small chain, $\frac{3}{4}$ of a yard in length; the whole chain was illumined, and covered throughout with beautiful rays, like bristles, or golden hair. Having placed a large jar in contact with the prime conductor, I affixed to the coating of it an

iron chain, which I also connected with a plate of metal, on which I intended to make the discharge by the discharging rod, fig. 18. This done, I hooked another chain, much longer, and of brass, to the opposite side of the jar, and brought the end of it within $8\frac{1}{2}$ inches of the metal plate. In contact with this end, I laid a small oak stick, 8 inches long, which I covered with saw-dust of fir-wood. On making the discharge on the plate both the chains were luminous through their whole lengths; as was also the saw-dust, which was covered by a streak of light, making a very pleasing appearance. From this experiment may, I think, be inferred, the necessity of making the conductors, erected as a security to buildings, &c. from the damage of lightning, both of the best materials, and of a very sufficient substance; and for this purpose, perhaps nothing will be found so proper as lead, which will remain in the earth many centuries without any considerable decay; and the tops of chimnies being covered with it,* and furnished with a long, sharp pointed rod of copper, or iron pointed with copper, which I think should extend at least 5 or 6 feet above the top of the chimney, or highest part of the building; a communication should be made from it by plates of lead, 8 or 10 inches broad, with the lead on the ridges and gutters, and with the pipes which carry down the rain water; which pipes should be continued to the bottom of the building, and there made to communicate, by means of another leaden pipe, or a plate of it, as before mentioned, with the water in a well, or the moist earth, or the main pipe which serves the house with water.

§ 4. *Description and Use of a new Prime Conductor.*

A, fig. 16, is a glass tube, 18 inches long, and near 2 inches in diameter. B, C, balls of brass, with a ferule, 2 inches long, to each of them; which ferules are to be cemented to the ends of the tube, and made air-tight. One of the brass plates, which are soldered to the ferules, has a small hole drilled through it, by which the air is to be exhausted. It is covered by a strong valve, properly secured, and concealed by the brass ball B or C. D, E, balls of brass, about $\frac{5}{8}$ of an inch in diameter, fixed on wires, which project $2\frac{1}{2}$ inches from the brass plates, at each end of the glass tube. F, a fine pointed wire, to collect the electricity from the excited glass globe, &c. G, supporters of sealing wax, on which the luminous conductor is to be mounted.

N. B. The dots in the tube are intended to represent the appearance of the electricity in it, described in the experiments. But when a bottle or a large jar is discharged through the glass conductor, it is uniformly filled with light.

* I mention covering the tops of chimnies with lead, as a protection to the upper courses of bricks, from the effects of wind; and not as being of any essential service to the conductor, any farther than as it may assist in fixing the pointed rod, which is to be elevated above it, more securely.—Orig.

The use of the Glass Conductor.—The glass tube, thus furnished and mounted, being properly exhausted, and perfectly dry, will act in all respects like one of metal; and the electrometer, being placed on the brass ball B, will answer exactly to the charge of a jar or battery. But the principal use of this instrument, is to ascertain the direction of the electric matter, as it passes through it. And this end it completely answers in the manner following, viz. set it with the collecting point F, before the globe, and place the knob of an uncharged bottle nearly in contact with the brass ball B, or hang a chain, &c. from it to the table; and, on working the machine, the ball D in the tube becomes entirely enveloped in a dense white atmosphere of electricity. If the point F be brought nearly into contact with an insulated rubber, and a communication be made from the ball B to the table; the atmosphere will be on the ball E in the tube. If a bottle, positively charged, be presented as in the drawing, fig. 18, the appearances in the tube will be as there delineated. But if a bottle charged negatively, be thus applied, the atmosphere will surround the ball E in the tube, as in fig. 19.

If, instead of the brass balls in the tube, points are used; or if a point be fixed at one end of the tube, and a ball at the other, the effect will be precisely the same.—Note also, that the glass conductor, for the purpose of making Dr. Franklin's curious experiments, with a pointed and blunted wire, is far superior to one of metal, the electric atmosphere being so much better retained by it. By this easy and simple process, may an ocular demonstration, at all times, be given, in a dark room and dry air, of the truth and propriety of Dr. Franklin's hypothesis of the Leyden bottle.

§ 5. *Miscellaneous Experiments, made principally in the Years* 1771 *and* 1772.

Exper. 1. If a black silk ribband, or a piece of black silk, be laid on a quire of paper, &c. on a table, and excited by drawing over its surface sealing-wax, sulphur, amber, or a tube of glass with the polish taken off by emery, its electricity will be positive: whereas, if it be excited singly, or together with a white ribband, by drawing them briskly between the fingers it is always negative. Laying it on the paper, and drawing over its surface a rod, or tube of smooth glass, its electricity will also be negative.

Exper. 2. If a plate of glass, 10 or 12 inches in diameter, be excited, and placed on the top of a box, from which a pair of light pith or cork balls are suspended, being mounted on a stand of sealing-wax; the balls will separate, and stand repelled from each other, being electrified positively, in a dry air, upwards of 4 hours. When they come into contact, on removing the glass, they diverge again, and are negatively electrified; but on replacing it they close. On removing it again they separate; and thus alternately as long as any electricity remains in it.

If the plate of glass be placed in a frame of wood, and a light pith or cork-ball be laid on its surface, on presenting towards it the end of a finger, or the point of a pin, &c. the ball will recede from them, with a very brisk motion, and may thus be driven about on the surface of the glass, like a feather in the air, by an excited tube, or the wire of a charged bottle. The cork-ball, being deprived of its electricity by the pin, &c. instantly flies to that part of the glass to which it is attracted the most forcibly.

Exper. 3. I hung on the prime conductor a small phial, 2 inches in diameter, coated $3\frac{1}{4}$ inches from the bottom. From the coating of this phial I suspended 2 chains: the first in contact with a heavy weight, placed on a card, across which I had ruled lines at equal distances, fig. 10, the 2d chain formed a circuit, with leaden pipe, small brass wire, small chain, &c. of 120 feet in length From the ball of the discharging rod, which rested on another weight, I also hung a chain, in contact with, and completing the circuit of 120 feet before-mentioned; and observed, that if the bottle was charged quite full, the electricity would, in the discharge, pass through the long circuit, rather than over the surface of the card, when the weights were placed at $\frac{9}{10}$ of an inch asunder; but if I charged the bottle only about half full, the electricity would, in the discharge, pass through the long circuit, rather than over the surface of the card, though the weights were placed at the distance of only $\frac{3}{10}$ of an inch. Query, Can there be a greater proof of the small resistance made by metal to the passing of the electric matter, compared with card, wood, &c. and consequently of the utility of metallic conductors to buildings, ships, &c.? The same observation has been repeatedly made, on the effects of the natural electricity.

Exper. 4. Having prepared a phial, in the manner directed by Mr. Lane, for making his curious experiment, by passing a wire through the bottom, and another through the cork, so as to bring the ends of the 2 within half an inch of each other, about the middle of the bottle, which was filled with water, I found, as that gentleman observed, that a slight shock of electricity discharged through it, would break the bottle. But having put a very small wire from the top to the bottom of it, through the water; I discharged through it 3 large jars, containing 16 square feet of coated surface, when the whole of the small wire was exploded; but the bottle remained unhurt. If therefore a metallic conductor, being too small, should happen to be destroyed by a stroke of lightning, yet the building, &c. to which it is affixed, will probably escape uninjured.

Exper. 5. When I strongly electrify a large prime conductor, 3 feet long, and 12 inches in diameter, if a person hold in his hand a brass rod terminated by a ball, 2 inches in diameter, at the distance of 2 inches from the side of the conductor, fig. 11, he will continue to draw such strong sparks as will give him a sensible shock in both his legs; but if another person at the same time present

the point of a lancet, or a wire 5 or 6 inches long, nicely tapered to a point, tipped with steel, towards the conductor; though at the distance of 2 feet, or somewhat more, this will draw off all its electricity silently; and not suffer a spark to pass from it to the brass ball: it is also observable, that if the point of the wire, or lancet, be brought nearly into contact with the prime conductor, yet no sensation is felt in the arm, &c. of the operator. Hence appears clearly the preference due to points, rather than round balls, or blunted ends, for the termination of the conductors erected as a security to buildings, &c. from damage by lightning; for it seems probable, that the sharp point of the conductor will act on the electric atmosphere of the cloud, and perhaps gradually and silently continue to diminish the contents, before the cloud can approach near enough to strike; and thus contribute to lessen, if not actually prevent, a stroke. But should the point be struck, the consequence I suppose will not be great, and a curious instance I have now before me, which I shall beg leave to quote as follows. "About 9 o'clock we had a dreadful storm of thunder, lightning, and rain, during which the main-mast of one of the Dutch East Indiamen was split, and carried away by the deck; the maintop mast and top gallant-mast, were shivered all to pieces; she had an iron spindle at the maintop gallant-mast head, which probably directed the stroke. This ship lay not more than the distance of 2 cables length from ours, and in all probability we should have shared the same fate, but for the electrical chain which we had but just got up, and which conducted the lightning over the side of the ship; but though we escaped the lightning, the explosion shook us like an earthquake, the chain at the same time appearing like a line of fire; a centinel was in the action of charging his piece, and the shock forced the musket out of his hand, and broke the ram rod. On this occasion I cannot but earnestly recommend chains of the same kind to every ship, whatever be her destination; and I hope that the fate of the Dutchman will be a warning to all who shall read this narrative, against having an iron spindle at the mast head." See Capt. Cook's voyage. This conductor was of copper wire, $\frac{3}{16}$ of an inch in diameter; which I am inclined to think is rather too small for the purpose; I am of opinion it ought to be a quarter of an inch at least; as I have been informed by Dr. Solander, that the point originally belonging to the conductor, had been stolen; and that this, on which the lightning fell, was of inferior workmanship, and not so sharp; which was another great disadvantage: perhaps if the wire of the chain had been larger, and the point more acute, the stroke would have been much lessened, if not absolutely prevented. If, instead of those chains, plates of copper, $\frac{3}{16}$ of an inch thick, and 2 inches broad, with the edges neatly rounded off, were inserted in a groove, and continued down the maintop-gallant-mast, the maintop-mast, and part of the main-mast, into the well-hole: a communication from the mast, to the underside of one of the decks,

might be made with a plate, or rod of metal, flattened at each end; and from that rod the conductor might be continued by plates of lead, or copper, on the underside of the deck, and down both the outersides of the ship, as low as the keel, if it be thought necessary: and this method I should apprehend would be preferable to the chains, which are now in use. Particular care should be taken, to have all the plates, which form the conductor, as nearly as possible in contact with each other, and to fix a sharp pointed slender rod of copper at its summit. And for the purpose of connecting the plates, inserted in the maintop-gallant-mast, the maintop-mast, and the main-mast; if a hoop of copper were fixed in a groove of its own thickness, at the top of the main-mast; and another such hoop at the upper end of the maintop-mast; perhaps they might answer this end very conveniently. Dr. Watson has collected from ancient history, the accounts of electrical appearances, on pointed bodies; as the spears of soldiers, &c. &c. which have been very judiciously introduced by Dr. Priestley into his History of Electricity; and I cannot but think those accounts furnish a very strong argument in favour of pointed conductors; for had the bodies here spoken of been terminated by blunted ends, or round knobs, it is probable that many of them, instead of drawing off the lightning silently, would have been struck with it; and this, being deemed a common occurrence, would have passed unnoticed, and consequently never have been recorded in history.

If pointed bodies had really the property of drawing down strokes of lightning on themselves, I think the pillar on Fish-street Hill, commonly called the Monument, could not long have escaped. This pillar is terminated by a basin of metal, $4\frac{1}{2}$ feet in diameter. The basin is surrounded by a great number of bended plates of metal, sharply pointed, to represent flames of fire. From the basin, to the floor of the gallery, are fixed perpendicularly in a circular order 4 thick bars of iron; and in these bars are inserted 28 strong hoops, and 4 segments of circles, of the same metal, which serve as steps from the gallery to the basin. One of these bars, being 1 inch thick and 5 inches broad, is connected with the iron rails of the stair-case, which reaches to the bottom of the building, and forms a substantial, regular conductor of metal the whole length. The monument was erected by Sir Christopher Wren in remembrance of the fire of London, which happened in the year 1666. It was completed by that great architect in the year 1677; is, including the blazing urn at its summit, about 202 feet in height, from the pavement; and has never, as far as I have been able to learn, been struck by lightning. The antennæ and legs of the grashopper on the Royal Exchange in Cornhill; and the tongue and tail of the dragon on the spire of Bow church in Cheapside, London, are also remarkable instances: indeed I have often thought it rather a favourable circumstance, that most of the lofty public buildings in this metropolis which have metallic termi-

nations, have generally been furnished with weather-fanes, which commonly end in sharp points; for had they been terminated with large round balls of metal, perhaps many more of them might long since have been demolished. Here therefore I cannot but express my earnest wishes, that on all future occasions, where lofty public edifices are to be erected, a good pointed conductor for the lightning, may be considered by every architect, or surveyor, as an essential part of the edifice itself.

Exper. 6. I attemped to ascertain the conducting power of different metals, in the following manner. I took a thick piece of paste-board, across which I ruled lines, exactly an inch asunder. On these lines crosswise I placed the wires, which were confined by heavy weights: the edges of which weights just touched the ruled lines; leaving exactly an inch of wire between them (see fig. 10). The kinds tried were, pure gold, silver, brass, copper silvered, and iron. They were all drawn through the same hole, except the iron, which was somewhat larger than the others. They were proved by 2 jars, containing 11 square feet of coated surface; and the charges were adjusted by an electrometer graduated in divisions of a 10th of an inch each, the diameter of the scale being 2 inches. The result was as follows:

Pure gold	was melted at	4	divisions.
Brass		6	
Copper silvered		8	
Pure silver		10	
Iron		10	

When I gave either of the wires a division less than the number above specified, it was not melted; when I gave either of them a division more, it was exploded; the greater part vanishing in smoke; whereas these charges just burst them into balls.

Having lately been presented, by Dr. Lewis, with 6 specimens of his platina, in as many different states, I selected the largest grains, from one of the parcels which he informed me had been repeatedly exposed to long-continued vehement fires; the most intense he had been able to excite, or any vessels he could procure would support: and after a few small globules, consisting doubtless in great part of heterogeneous metal, had melted out, repetitions of the operation produced no further change. It was afterwards boiled successively in oil of vitriol, aquafortis, and spirit of salt, in order to its further purification; and which indeed reduced it to a state the most pure of any that excellent chemist had been able to produce. Having ruled a line with a blunt ended wire, over the surface of a plate of white wax;

Exper. 7. I pressed in the grains of platina lightly, and in contact with each other, so as to form a regular line, half an inch long. At each end of the line of platina, and in contact with it, I placed a thick wire, with its ends nicely

rounded off, and made perfectly smooth. I covered the platina with a piece of thick plate-glass; and then discharged through it, 3 jars containing 16 square feet of coated surface: when I obtained many beautiful spherules of the platina. Several of them stuck to the wax and glass; and others imperfectly formed, on the edges, &c. of the grains; which proved that the fusion had been complete.

Exper. 8. I made a long cork perfectly dry, and held one end of it very near the fire, till it began to burn. At the same time I held a small fine toothed file in the clear part of the fire, till that also had become very dry, and rather hot. Then, having filed off the end of the cork, I applied it to a pair of neat light pith balls; when it attracted them both, and raised them perpendicularly, as high as the strings would permit. Having electrified the balls by excited amber, the cork would increase their divergence from 1 to near 2 inches? or it would repel them at an inch distance, so as to drive them 1½ inch out of the perpendicular. Electrifying the balls by excited glass, these appearances were directly reversed. The cork therefore had parted with its electricity to the file, and plainly acted as a negative electric.

Exper. 9. Having neatly rounded off the corners of a piece of thin talc, about 3 inches square; I coated both its sides within ¼ of an inch of the edges, with tin-foil, which I also rounded off at the corners. The talc, thus prepared, I observed would readily charge, without wiping, or drying the uncoated part, and the force of the shock, in the discharge, was really astonishing.

Having been shown, by my late truly ingenious friend Mr. Canton, an electric spark, of a very beautiful crimson colour, which always appeared as it was drawn over, or through, a piece of smooth wood, at the top of the conductor-stand, and which was supposed by some gentlemen to be the light of electricity, very thinly spread on the surface of the wood; I was exceedingly desirous to know from what cause this phenomenon really proceeded; and for that purpose made the following experiment.

Exper. 10. I fixed between 2 balls, introduced into the circuit of an electric discharge, a piece of smooth wainscot, about 2 inches in diameter, and a quarter of an inch thick; when, on making the discharge of a pretty large jar, I observed the wainscot to be nearly covered with the electric light, the outer parts, or edges of the light, were exceedingly thin, but the colour very white, as it was also in several other experiments, made with the same intent. I then procured a circular piece of coloured box, which was glued to the top of the stand to my prime conductor; when, drawing strong sparks through this wood, of whatever colour it was, I became clearly of opinion, that the colour of the spark varied according to its depth in the wood, viz. if it passed on the surface, it was white, a little below it, yellow, or orange: still lower, scarlet: and deeper in the wood, crimson.

It having been mentioned by some gentlemen, as their opinion, that the matter of light, and the electric matter, were the same thing; I made the following experiment, in order to determine whether there was any foundation for such an opinion.

Exper. 11. I insulated the rubber of my machine, and placed it in such a situation, that the rays of the sun, passing through the open window of my room, might fall immediately on it; but this I observed produced no electricity: I then collected the rays into a focus, by means of a good convex glass, and threw them on the back of the rubber, till it was burned quite black; but this method was attended with no better success. I then mounted one of Mr. Canton's electrometers, furnished with very light balls, on a stand of sealing-wax; and having electrified them negatively, by excited amber, so as to diverge a full inch, I again collected the rays of the sun by the convex glass, and held it at such a distance as to bring the focus exactly on the end of the box, which was burnt very black, and the glue in the joints melted; but the balls were not in the least affected.

Exper. 12. Hold a piece of amber near the flame of a candle, till it becomes hot: then apply it to a suspended thread, and it will not attract it, neither will it become electrical in cooling; but press it ever so lightly on your hand, in order to try its heat, though without the least friction, and, if it be not too hot, it will be electrical, and attract it violently. Heat it again at the candle, and its electricity shall be taken quite away. Press it again gently on your finger, or hand, and the power will be restored. Apply it again to the candle, it is lost. And thus alternately. Other electrics may probably act in the same manner; as the flame of a candle, or hot air, will conduct away the electricity of glass, almost instantaneously.

Exper. 13. Showing Mr. Nairne the above-mentioned experiments: when the amber had been well heated, and being presented to a suspended thread, having shown no sign at all of electricity; I held it, between my thumb and fore-finger, very near the table, but not so as to touch it, that we might entirely avoid friction. He then blew against it 30 blasts, with a pair of kitchen bellows; when presenting it to the thread it attracted it, at the distance of $\frac{1}{8}$ of an inch. He then blew against it 30 blasts more, as above described; when applying it again to the thread, we saw it attracted at half an inch distance; and on drawing back the amber, it drew the thread after it 6 or 8 inches. We repeated the experiment 3 times with the like success; and are satisfied, that the amber was made electrical by the friction of the particles of air against its surface: and not in the least by heating only. We afterwards excited the amber, when it must have been perfectly cold, but dry, by only blowing against it as before. The same process succeeds with glass.

§ 6. *Experiments and Observations on the Electricity of Fogs, &c.*—1771, Nov. 14, half past 8, A. M. I find a fog, not very thick, pretty strongly electrified. The balls separate full half an inch. They keep stationary, there being little or no wind.

Dec. 2, half past 8, A. M., a fog, moderately thick, is strongly electrified. The balls diverge half an inch; but when they are brought near the building, they close, and open again on removing them. The mercury in the thermometer is 15 degrees above the freezing point.

Dec. 18, half past 4, P. M., a moderately thick fog is strongly electrified soon after its appearance. The balls diverge full half an inch, and regularly close at the approach of excited wax. The wind is troublesome, but the balls keep their distance, and at intervals very well admit trying the experiment.

1772, Jan. 5, a fog is strongly electrified positively. The balls diverge full half an inch. The air is sharp, and frosty.

Jan. 13, 9 o'clock, A. M., a fog, not very thick, is strongly electrified positively. The mercury in the thermometer is $7\frac{1}{2}$ degrees above the freezing point. There is little or no wind.

Jan. 18, 10 o'clock, A. M. The air is pretty strongly electrified by a fall of snow.

From the small number of experiments I have been able to make on the electricity of the atmosphere, I cannot help being of opinion, that fogs are much more strongly electrified in, or immediately after, a frost, than at other times; and that the electricity in the fogs is often the strongest, soon after their appearance. I also now hold it for a certain rule, that whenever there appears a thick fog, and the air is at the same time sharp and frosty, that fog is strongly electrified positively. Though rain may not be an immediate, yet I am inclined to think it is by no means a very remote consequence of electricity in the atmosphere; and, from the trifling observations I have had an opportunity to make on that subject, I have not failed to find that in 2 or 3 days after I have discovered the air to be strongly electrified, especially if that electricity continued for as long a time, we have had rain, or other falling weather, and I incline to believe, more plentifully in proportion to the strength and continuance of the electricity; if not rain, snow, &c. according to the state of the atmosphere, with respect to heat and cold. If electricity be not a cause, I think it at least a prognostic, of falling weather.

XLII. A Letter from David Macbride, accompanying a Letter from Mr. Simon to Dr. Macbride, concerning the Reviviscence of some Snails preserved many Years in Mr. Simon's Cabinet. p. 432.

In Mr. Simon's letter of the 26th of November, he mentions a particular

shell, whose snail had come out 4 several times, in the presence of different people, each of whom have assured me that they saw it. A day or two after the date of that letter, the above gentleman brought the identical shell, as he declared, into the presence of several other persons, that they might try if the snail would again make its appearance. The company were not disappointed: for after the shell had lain 10 minutes in a glass of water that had the cold barely taken off, the snail began to appear: and in 5 minutes more we perceived half the body fairly pushed out from the cavity of the shell. We then removed it into a basin, that the snail might have more scope than it had in the glass: and here in a very short time, we saw it get above the surface of the water, and crawl up towards the edge of the basin. While it was thus moving about, with its horns erect, a fly chanced to be hovering near, and perceiving the snail darted down on it. The little animal instantly withdrew itself within the shell, but as quickly came forth again, when it found the enemy had gone off. We allowed it to wander about the basin for upwards of an hour; when we returned it into a wide mouthed phial, where Mr. Simon had lately been used to keep it. He presented me with this remarkable shell; and I observed, at 12 o'clock, as I was going to bed, that the snail was still in motion; but next morning I found it in a torpid state, sticking to the side of the glass.

In a few weeks after the time abovementioned, I took an opportunity of sending this shell to Sir John Pringle, who showed it at a meeting of the society; but as he has been pleased to inform me, some of the members could not bring themselves to believe but that Mr. Simon must have suffered himself to be imposed on by his son, who, as they imagined, substituted fresh shells, for those which he had got out of the cabinet. On this, I wrote to Mr. Simon, which produced his letter of the 4th of February. I afterwards also examined the boy myself; and could find no reason to believe that he either did or could impose on his father.

Mr. Simon is a merchant of this place, of a very reputable character, and undoubted veracity. He lives in the heart of the city, a circumstance which rendered it almost impossible for the son (if he had been so disposed) to collect fresh shells. The father of Mr. Stuckey Simon was Mr. James Simon, F. R. S., who, being a lover of natural history, as well as an antiquarian, made a little collection of fossils, which is still in the son's possession, and contains some articles that are rather uncommon.

Mr. Stuckey Simon to Dr. Machbride, dated Dublin, Nov. 26, 1772.

Sir,—An accident having brought to light what some naturalists have not had an opportunity to examine into, and which has been a subject of some conversation among gentlemen to whom I have mentioned it, has made me commit to wr ng the simple facts, in order to put others on making further experiments on the sub ect. About 3 months since, I was settling some shells in a drawer;

among which were some snail shells. I took them out, and gave them to my son, a child about 10 years old, who was then in the room with me. The Saturday following, the child diverting himself with the shells, put them into a flower-pot which he filled with water, and next morning put them into a basin. Having occasion to use it, I observed the snails had come out of the shells. I examined the child. He assured me they were the same I gave him some days before; and said he had a few more, which he brought me. I put one of them in water; and, in half an hour after, I observed it put out its horns and body, which it moved with a slow motion, I suppose from weakness. I then informed Major Vallancy and Dr. Span of this surprizing discovery. They did me the favour to come to my house the Saturday following, to examine the snails; and, on putting them in water, found that only one had life, which was that I put in water, for it came out of its shell, and carried it on its back about the basin. The rest I suppose died by being kept too long in water; for, on the first discovery, I let them remain in the water till the Monday following, when I poured off the water, the snails being still out of their shells, and seemingly dead. They lay in that state till Tuesday night, when I found they had all withdrawn into their shells; and, though I several times since put them into water, they showed no signs of life. Dr. Quin and Dr. Rutty did me the favour, at different times, to examine the snail that is living; and were greatly pleased to see it come out of its solitary habitation, in which he has been confined upwards of 15 years, for so long I can with truth declare it has been in my possession; as my father died in January 1758, in whose collection of fossils those snails were, and for what I know they might have been many years in his possession before they came into my hands. The shells are small, and of one kind; white, striped with brown.—Since this discovery, I have kept this snail in a small phial, with a cover with holes, to let in air; and it seems at present very strong, and in health.

XLIII. The Bill of Mortality of the Town of Warrington, for the Year 1773. *By the Rev. J. Aikin.* p. 438.

The town of Warrington, contains between 1600 and 1700 houses. At 5 persons to a house, which is supposed a sufficient allowance, as but few are occupied by more than one family, this will give above 8000 for the number of inhabitants. The average of yearly marriages, christenings, and burials, registered in the parish church,

	Marriages.	Christenings.	Burials.
From 1750 to 1769 inclusive, is	73	237	199
For the years 1770, 1771, 1772, is	95	331	258

This will serve to show the increase of the place, and its comparative healthi-

ness; especially if we consider that the deaths are much more exactly registered than the births. In the present bill, the number of children, who died after receiving only private baptism, in consequence of which their deaths were registered, but not their births, amounts to 17; which might therefore be added to the average of christenings for the last 3 years, and will form an extraordinary instance of healthiness and increase. The present bill also takes in the separate registers kept by different societies, in which the births much exceed the burials, as many of the latter are entered at the parish church.

The melancholy overbalance of burials, which now appears, plainly arises from the dreadful ravages of a single disease, the small-pox; which perhaps has seldom raged with greater malignity than in its late visitation of this town. Its victims were chiefly young children; whom it attacked with such instant fury, that the best-directed means for relief were of little avail. The state of the air went through all possible variations in the course of it, but with no perceptible difference in the state of the disease. In general, the sick were kept sufficiently cool, and were properly supplied with diluting and acidulous drinks; yet where they recovered, it seemed rather owing to a less degree of malignity in the disease, or greater strength to struggle with it, than any peculiar management. Where it ended fatally, it was usually before the pustules came to maturation; and indeed in many they showed no disposition to advance after the complete eruption, but remained quite flat and pale. In one neighbourhood, out of 29 who had the disease, 12 died, or about 2 in 5; in others the mortality was still greater, and there is reason to believe it was not less on the whole. It may perhaps be worthy of observation that the proportion of females who died, to males, was nearly as 3 to 2. While we lament the severity of the scourge with which we have been afflicted, we cannot but highly regret, that a practice, which experience has established as so effectual a security against it, was so little followed. Not 10 were inoculated in the whole town and neighbourhood: these all did well, yet their example was not sufficient to overcome some accidental prejudices taken against it.

General Bill for 1773.

Marriages.	Births.			Burials.		
93	Males	175	356.	Males	223	473.
	Females	181		Females	250	

Of these 473 deaths, 211 were by the small-pox.

XLIV. Of the Stilling of Waves by Means of Oil. Extracted from sundry Letters between Benj. Franklin, LL. D., F. R. S., Wm. Brownrigg, M. D., F. R. S., and the Rev. Mr. Farish. p. 445.

This paper may be consulted in Dr. Franklin's works, collected and published in 1806, in 3 vols. 8vo. see p. 144, vol. 2.

XLV. On a New Map of the Northern Archipelago, and a Specimen of Native Iron. By M. de Stehlin, Couns. of State to her Imperial Majesty of Russia. p. 461.

As a testimony of his attachment to the R. S., and as the first tribute he owed to that learned body, he had the honour to transmit herewith 2 novelties, which he thought worthy of their notice. The first was a new map, and his preliminary description of a new Archipelago in the North, discovered a few years before by the Russians, in the N. E. beyond Kamtshatka. The second was a piece of raw and native iron; of which Mr. Pallas, one of the R. S. of Petersburgh academicians, who had 5 years been employed in making researches in natural history, in the provinces of the Russian empire, had discovered in 1773 a hillock or mass, weighing 50 puds, the pud consisting of 40 Russian pounds, in Siberia, in the mountains called Nemir, between the rivulets Ubec and Sisim, which fall into the river Jenisei, scarcely 100 fathoms from a rich mine of loadstone or iron.

The existence of raw or native iron has hitherto been doubted; but M. de S. almost thinks that this discovery determined the question; especially when it is considered, that in the whole district where this mass was found, there is not the least trace extant of any ancient forge, nor any place that might leave room to suspect that there had been, in former times, any works of iron ore, which had been melted, and afterwards abandoned to that mass. Should any doubt remain concerning the existence of the native iron, and the authenticity of this discovery, he should rather suppose that, many ages ago, there might have been a volcano, which by melting the iron ore had formed the above mass, to which might afterwards have been joined the little hyacinthine spars and other stones now mixed with it.

Translation of an Article in the Petersburg Gazette of Sept. 6, 1773.

"The academy expects from Siberia a black mass weighing about 40 puds,* of raw or native, soft and flexible iron, which the academician Pallas has discovered during his residence in the neighbourhood of the river Jenisei. This very remarkable and huge lump is of a spongy texture, of the most perfect and malleable iron, whose cavities are closely filled with small polished pieces of hyacinthine spar, some round, some with flat surfaces, and all of the colour of transparent amber. The mass is rusty only on the surface; but the interior has been preserved by a kind of black varnish spread all over the iron, which is of an irregular form blunted at the corners. This iron may be bent and hammered when cold, and, when moderately heated, may be shaped into nails and other tools; but, in a violent heat, and especially if, in order to separate it

* The mass in its present state, weighs 152 Russian pounds.

from the sparry particles, it is thrown into smelting ovens, it becomes brittle, granulated, and will not join again in the forge.

This mass was found lying on the surface, at the top of a high woody eminence, not far from the mountains called, by the Tartars, Nemir, between the two rivulets Ubei and Sisim, which fall from the right into the Jenisei, a little below Abakanskoi Ostrog, and scarcely 100 fathoms from a rich mine of hard ore of loadstone. The appearance and nature of this mass, and the qualities of the iron of which it chiefly consists, are so decisive, that it cannot be doubted but that it has been thus produced by nature; and if so, the existence of native iron, which has hitherto been questioned, is established beyond all contradiction; especially if it be considered, that no trace of any old iron work, of which there are many in the Siberian mountains, is to be met with in the desart where the mass was found; and the mine abovementioned was not opened before the year 1752, when the miners, who were there employed, first discovered this mass of iron: since which time no further notice had been taken of it."

XLVI. Of Torpedos found on the Coast of England. By John Walsh, Esq. F.R.S. p. 464.

"It has lately been found, that the torpedo, or electric ray, frequents the shores of this island, contrary to a received opinion among naturalists, who have in general considered it as an inhabitant only of warmer climates. In consequence of inquiries Mr. W. had set on foot in some of our southern fishing ports, 2 torpedos, taken in Torbay, one in the beginning of August, and the other in the beginning of Nov., last year, (1773), have been actually sent up to this metropolis. The first, procured by the good offices of Mr. Amyatt, apothecary, in Berkeley-square, was examined, and the electrical organs were successfully injected, by Mr. John Hunter. The second, forwarded by Mr. Grant, a principal fishmonger in the land carriage branch, then at Brixham, came up very fresh and perfect, in one of his fish machines. This was weighed and measured before it was touched by the dissecting knife, and found to weigh 53 lb. avoirdupois, and to measure 4 feet in length, $2\frac{1}{2}$ feet in its extreme breadth, and $4\frac{1}{2}$ inches in its extreme thickness.

The largest torpedo Mr. W. met with in the neighbourhood of Rochelle, where upwards of 70 passed through his hands, weighed little more than 10 lb. and measured not quite 2 feet in length, nor quite 16 inches in breadth: and the largest he had read of was that mentioned by Rhedi to Lorenzini, weighing 24 lb. doubtless of Leghorn, which make about 18 avoirdupois. Though this Mediterranean torpedo has been ever considered as of an extraordinary size, it is exceeded in weight nearly 3 to 1 by our enormous British torpedo.

Its back was of a dark ash colour, with somewhat of a purple cast, but not at all mottled like those of the Atlantic coast of France, nor regularly marked with eyes, as they have been called, like some found in the Mediterranean. Its under part was white, skirted however with the same ash colour, which towards the tail became almost universal. The side fins, being a little contracted and curled up, prevented the precise measurement of its breadth, but it appeared to hold the general proportion observed in those of Rochelle; that is, the breadth was $\frac{4}{5}$ of the length. Its electric organs likewise were proportionate with theirs, each organ measuring 15 inches in extreme length, and 8 in extreme breadth. In short, the torpedo of Torbay no way differed from those seen in the Bay of Biscay, but in size and colour: and perhaps this difference may be thought rather casual than denoting a specific distinction.

It was a female, without any signs of pregnancy. The intestines contained, with some black slime, 2 vertebras of a fish, seemingly of the cod kind. The electric organs of this torpedo were likewise injected by Mr. Hunter, though not with his first success, from the bursting of the artery in the operation; he determined however the number of columns, in one organ, to amount to 1182, and fully confirmed the observation he formerly made, that their numerous horizontal partitions were very vascular.

The frequent, and perhaps favourite situation of the torpedo, is to lie in concealment under sand. If it be placed by design, as it is sometimes left by accident, in any hollow of a sandy beach, whence the tide has just retired, it swims to that brink where the water is still draining away, and on finding itself unable, after repeated attempts, to push itself over the shallow, and follow the course of the tide, it begins with admirable address to bury itself in the sand, and by a gentle but quick flapping of its extremities all round, soon sinks itself a bed, and in the action throws the sand in a light shower over its back. Neither the animal nor the spot it is in can now be distinguished; save only that, on a nice search, its two small inspiratory foramina, and their membranes at play, may be perceived. It is in this situation that the torpedo gives his most forcible shock, which throws down the astonished passenger who inadvertently steps on him.

Mr. W. has thus shown that Great-Britain too claims the torpedo, or electric ray; that ours is the broad marine sort, which Socrates, as Meno thought, resembled; and that it is the black torpedo, whose influence subdues obstinate head-achs, and the gout itself.* In announcing to our naturalists and electricians the presence of this wonderful guest, Mr. W. says, he should certainly felici-

* Scribonius Largus, cap. 1, and 41. See also several of the early physicians, Roman and Arabian, for different cures attributed by them to the effect of the torpedo.—Orig.

tate our individuals on their acquisition, but that the Leyden Phial contains all his magic power.

XLVII. Description of a Double Uterus and Vagina. By John Purcell, M. D. Professor of Anatomy in the College of Dublin. p. 474.

The body of a woman, who had died in labour in the 9th month of her pregnancy, was dissected at the anatomical theatre of Trinity College. In the summer of 1773, on opening the abdomen, a uterus appeared of such a size and form, as are generally observed at that period. It contained a full grown fœtus; but was furnished with only one ovarium and one Fallopian tube, which were situated on the right side. On the left was placed a 2d uterus unimpregnated, and of the usual size, to which the other ovarium and tube were annexed. But these 2 uteri were totally distinct and separated from each other, except at the lower extremity of their necks, where their union extended ¼ of an inch, and an acute angle was formed between them. There was nothing extraordinary in the formation of the external parts of generation; but from each side of the meatus urinarius a membrane ran downwards; and the two, having comprehended this orifice between them, were joined together a little below it, so as to form, by their union, a septum or mediastinum, which taking the remainder of its origin from all that prominent ridge called the superior columna, and descending perpendicularly, was inserted into the inferior columna, so as to extend from the entrance of the vagina as far backward as its posterior extremity, and thus to divide it into two tubes of nearly equal dimensions. But each of these did not lead solely to the womb of its own side; for the right vagina became gradually wider as it ran backward, and at last was so far dilated as to comprehend, within its circumference, the orifices of both uteri; while that on the left side, having taken an oblique direction, ended in a cul de sac, or cæcum. Such a confirmation might have rendered it totally useless: to prevent which, nature, fertile in expedients, seems to have had recourse to a very extraordinary contrivance. This was a fissure in the septum, an inch in length, and about an inch distant from the womb of that side. Though its circumference was perfectly smooth, we must acknowledge that it might have arisen from an accidental rupture of the septum; the lips of the wound not uniting, and, in process of time, becoming callous; and yet, he imagined, that the parts were originally formed in this manner, in order to preserve a communication between the 2 vaginæ.

Thus it appears, that both uteri might be impregnated through either vagina, as that on the right side led directly to both; and as, by means of the fissure in the septum, the semen could easily be thrown from the left vagina into the right, where the apertures of the 2 wombs were placed. Through the latter

passage both uteri would seem to have an equal chance for impregnation; for, notwithstanding that which contained the fœtus was placed almost directly in a line with the axis of the right vagina: yet this probably was not its original position; but by degrees its bulk increased so much as necessarily to occupy the middle space, and push the unimpregnated one aside. But however surprizing it may seem at first view, yet there was reason to imagine, that the right womb, though at a greater distance, would be much more apt to conceive than the other, if the left vagina only had been made use of. For when this was distended, it appeared that the posterior part of the septum, by its protuberance, closed up and covered the left os tincæ; and as such would probably be the case in copulation, the semen not finding a ready admission into it, would pass over to the right orifice, where its entrance could not be so much obstructed. So that, if he may hazard a conjecture, he thought it more likely, since the right uterus alone conceived, that the left vagina had generally been employed.

It was a prevailing opinion among the ancients, that male children were conceived in the right side of the womb, and females in the left. Having so few opportunities of dissecting human subjects, they depended too much on the analogy of the structure of brutes, which has been the principal source of the many erroneous descriptions met with in their works. It is well known that the uterus of many quadrupeds is divided into 2 cornua, in which the fœtuses are lodged; and it was not very absurd to conclude, that nature might have formed them for the distinct repositories of the 2 sexes. Accordingly this was supposed to take place in the human uterus, which has been described and delineated as if distinguished into 2 chambers. Hence arose the opinion, which is received in some places to this day, that a very sure prognostic, with regard to the sex of the child, may be drawn from the side of the belly on which the tumour is more sensibly felt. Dissections, being now more frequent, have proved, that the human womb generally has only one undivided cavity; so that the fœtus, let it come from which tube it may, will, when arrived to a certain size, occupy it entirely. This observation however is not sufficient to refute the supposition that each sex might have its peculiar ovarium; and some authors pretend, that they are able to determine how many males or females any animal has brought forth, by examining the number of cicatrices on its ovaria. For, when females only had been produced, the right ovarium was found still full of vesicles, but the left quite exhausted. That this is not always the case in brutes, appears from the observation of Dr. Harvey, who frequently found male fœtuses in the left cornu, and females in the right. In the human subject, opportunities of ascertaining this matter must occur very seldom. We have an instance, recorded by Cyprian, where both a boy and a girl were conceived, though the right tube was wanting. But the present case affords another example, which is

decisive; for here the impregnated uterus had not the smallest communication with the left ovarium or tube, and yet it contained a female fœtus.

The septum was not merely membranous, but fleshy, and of a considerable thickness; and, like most other mediastina in the human body, consisted of 2 laminæ combined. Of these each vagina furnished one; for each had its own constrictor, and being completely surrounded by muscular fibres, had a power of contraction independent of the other, which could not be effected if both vaginæ were comprehended within the same muscular rings, and separated by a membrane incapable of action.

It has been the opinion of many modern authors of the first reputation, that the fundus is that part of the womb, whose extent increases, in the greatest proportion during pregnancy; and on this supposition they have founded various theories. One of the principal arguments which they propose, in support of their opinion, is, that the insertion of the Fallopian tubes is removed from the angles of the uterus, and gradually descends towards its neck; so that a short time before delivery they are at a very great distance from their former position. Haller does not attempt to deny these facts; but mentions 3 instances where the tubes did not change their place. But Petit, in his Memoire on the cause and mechanism of child-birth, is clearly of opinion, that the whole doctrine is destitute of foundation. He asserts, that the fundus increases less than any other part, and that the surprizing growth of the womb is effected by fresh supplies of fibres, successively furnished by the neck and parts adjoining. As a decisive proof, he insists that the insertion of the tubes continues nearly in the same place, and accounts for the error of the abovementioned authors by observing, that as the fundus is pushed upwards by the growth of the other parts, a greater portion of the tubes will adhere to the surface of the womb, and thus the apparent place of insertion be very far distant from the real one. This remark is verified in the present instance; for the tube at first sight appeared to penetrate into the middle of the uterus; but on a closer inspection, and by introducing a bristle, it was found to run for a considerable space between it and the coat which it receives from the peritonæum, and at length to enter into its cavity, not very far from the spot which it may be supposed to have occupied before impregnation.

With regard to superfœtation, it is evident how easily it might have been effected in the present subject; and the supposition of a double uterus can readily account for it on many other occasions. But this is a matter on which it would be needless to dwell any longer, as it has been very fully treated in Gravel's Dissertation, published in Haller's Collection; where we meet with a similar instance of 2 uteri and a vagina, the anterior part of which was divided by a septum, but whose posterior portion was single, where the septum was discontinued.

Haller, in his Opuscula Pathologica, gives the history of a young lady of quality who had 2 wombs, each of an oval shape, and furnished with its own peculiar vagina. One of these vagina was anterior, and communicated with the right womb; the other was posterior, and led to the left. And it is worth observing, that in these two cases, and in most others of the same kind, which have been hitherto observed, each uterus had only 1 ovarium, and 1 tube.

A double uterus is described by O. Acrel, in a treatise printed at Stockholm, in 1762; and in the 7th vol. of Haller's Elementa Physiologiæ, various authors are referred to, who deserve to be consulted on this subject. In some of these we find examples of 2 wombs, or 1 uterus divided into two cornua. In other instances the uterus retained its proper external appearance, though it was really double, its cavity being divided by a septum.

Since therefore it is certain that, in the structure of the parts of generation, Nature frequently deviates from her ordinary course, practitioners in midwifery ought to consider how many difficulties they may perhaps be exposed to, by not attending to the possibility of sometimes meeting with those organs formed in the same manner as in the subject of this essay. An attention of this kind would probably have been of the utmost consequence in the present case; for the orifice of the unimpregnated uterus was so far dilated, as easily to admit 2 fingers, which might have arisen from the attempts of the midwife to bring on delivery: nor can we conceive any thing more vexatious than such a case would prove, were it to fall into the hands of an inexperienced person; as the orifices of the different wombs presenting themselves alternately to his touch, he might entertain doubts of the pregnancy of his patient, even when her labour was approaching; and, by endeavouring to dilate the left vagina, all his efforts to promote delivery, would only serve to render it more difficult, or perhaps impracticable.

XLVIII. On some Specimens of Native Salts, collected by Dr. Brownrigg, and shewn at a Meeting of the R. S., June 23, 1774. p. 481.

This paper contains a description of some specimens of native salts, mentioning at the same time the places where they were found.

END OF THE SIXTY-FOURTH VOLUME OF THE ORIGINAL.

I. Experiments on the Torpedo, made at Leghorn, January 1, 1773. *By Dr. John Ingenhousz,* F. R. S. Anno* 1775. *Vol. LXV.* p. 1.

As I could get no torpedos alive to my lodgings at Leghorn, I hired a fishing

* Dr. Ingenhousz, was a native of Breda, and for some time practised physic in his native country. About the year 1767 he came to England, to learn the Suttonian method of inoculating the

vessel, called a tartana, with 18 men, and went out 20 miles to sea, where the bottom is muddy, and where those fish are chiefly to be found. We caught 5; of which 4 were about a foot in length, and the other of a smaller size. Before the nets were taken up, I charged a coated jar by a glass tube, and gave a shock to some of the sailors, who all said they felt the same sensation as when they touched the torpedo. They also said, that this animal has but very little force in winter, and cannot live a long time out of the water. I put the torpedos immediately into a tub, filled with sea water, together with 2 or 3 other fishes, which I found not at all hurt by their company. I took one of the torpedos in my hand, so that my thumbs pressed gently the upper side of those two soft bodies at the side of the head, called (perhaps very improperly) musculi falcati by Redi and Lorenzini, while my forefingers pressed the opposite side. About a minute or 2 after, I felt a sudden trembling in my thumbs, which extended no farther than my hands: this lasted about 2 or 3 seconds. After some seconds more, the same trembling was felt again. Sometimes it did not return in several minutes, and then came again, at very different intervals. Sometimes I felt the trembling both in my fingers and thumb. These tremors gave me the same sensation as if a great number of very small electrical bottles were discharged through my hand very quickly one after the other. The fish occasioned the shock, or trembling, as well out of the water as in it. The shock lasted sometimes scarcely a second; sometimes 2 or 3 seconds. Sometimes it was very weak; at other times so strong, that I was very near being obliged to quit my hold of the animal. The torpedo having given one shock, did not seem to lose the power of giving another of the same force soon after; for I observed several times, that the shocks, when they followed one another very fast, were stronger at last than in the beginning; and this was the same when the fish was under water as when kept out of it. The pressure of my fingers, more or less strong,

small-pox; and in 1768, on the recommendation of Sir John Pringle, he was engaged to go to Vienna to inoculate the Archduchess Teresa-Elizabeth, daughter of the emperor Joseph II., and his majesty's two brothers, the archdukes Ferdinand and Maximilian; and the next year he went to Italy and inoculated the grand duke of Tuscany. The rewards of these services were the rank of body physician and counsellor of state to their Imperial Majesties, with a pension for life of about 600l. sterling per annum. For many years afterwards he resided chiefly in England, almost unceasingly employed in scientific pursuits, till the time of his death, which happened at Bowood Park, the seat of the marquis of Lansdowne, Sept. 7, 1799, at a very advanced age. Dr. I. was a man of great simplicity of manners, and benevolence of disposition; to whom the public are indebted for several curious and useful discoveries; particularly in the application of pneumatic chemistry and natural philosophy, to the purposes of medical and agricultural improvements. Besides several ingenious papers in the Philos. Trans. from vol. 65 to vol. 70, Dr. I. published in 1779, "Experiments on Vegetables, discovering their great power of purifying the common air in sunshine, and of injuring it in the shade and at night;" which have since been extended and improved, and republished on the Continent, in collections of his works in French and German editions, which include also his papers in the Phil. Trans. and others which were published in the Journal de Physique.

did not seem to make any alteration in the powers of the torpedo. Applying a brass chain to the back of the fish, where I had put my thumb before, I found no sensation at all in my hand, though I repeated the experiment often, and applied the chain for a space of time in which I always perceived a stroke.* This was probably owing to the weakness of the fish in winter; or perhaps because I neglected to put my finger to its opposite side. Having insulated myself on an electrical stand, and keeping the torpedo in my hand, in the manner abovementioned, I gave not the least sign of being electrified, whether I received a stroke from the fish or not. The torpedo being suspended by a clean and dry silk ribband, it attracted no light bodies, such as pith-balls, or others, put near it. A coated bottle applied to the fish, thus suspended, did not at all become charged. When the fish gave the shock in the dark, I heard no crackling noise, nor perceived any spark. When pinched with my nails, it did not give more or fewer strokes than when not pinched. But by folding his body, or bending his right side to his left side, I felt more frequent shocks. Dr. Drummond made these experiments with me.

We dissected some of the torpedos, and found, if I remember well, 4 very large bundles of nerves, passing sidewards from the head into the 2 soft bodies, called musculi falcati, and distributed by dense ramifications through their whole substance. These nerves seem to terminate in round threads, which surround certain cylinders of a transparent gelatinous substance, which seems to constitute the material part of these singular bodies that appear to be the reservoirs of the electric power: these cylinders are parallel to each other, and have their direction from the under to the upper side of the fish. I did not observe whether these soft bodies changed in size when the torpedo gives a shock, but I suspect they do.

II. Of Two Giants Causeways, or Groups of Prismatic Basaltine Columns, and other curious Vulcanic Concretions, in the Venetian State in Italy; with some Remarks on the Characters of these and other Similar Bodies, and on the Physical Geography of the Countries in which they are found. By John Strange, Esq. F. R. S. p. 5.

Mr. S. first gives a topographical view of a part of the south-east side of a hill, called Monte Rosso, about 7 miles nearly south of Padua, in the Venetian State in Italy, and a mile to the west of Abano, a village well known, from the celebrated hot baths of that name, and which are situated at half a mile distance

* Dr. Ingenhousz means, that he felt no shock, though he saw the animal, by the contortion of its body, give one to the chain. At that time he did not seem to know, that though the shock would be communicated by a rod of any metal, it could not be so by a chain, or where there was the least interruption of continuity.—Orig.

to the south of it. This view particularly represents a natural range of prismatic columns, of different shapes and sizes, placed in a direction nearly perpendicular to the horizon, and parallel to each other, much resembling that part of the famous Giant's Causeway in Ireland, called The Organs. The next is a similar representation of the west side of another basaltine hill, called Il Monte Del Diavolo, or the Devil's Hill, near San Giovanni Illarione, also in the Venetian State, and Veronese district, about 10 miles nearly north west of Vicenza. The prismatic columns appear to be ranged in an oblique position, along the side of the hill. This drawing however represents only a part of the Causeway of San Giovanni, which continues along the side of a valley, nearly in the same manner, to a considerable distance. Though the columns of both these hills are of the simple, or unjointed species, yet they differ very remarkably from each other in many respects, but principally in their forms, and the texture and quality of their parts. Those of San Giovanni commonly approach a circular form, as nearly as their angles will permit; which is also observable in the columns of the Giants Causeway, and of most other basaltine groups. On the contrary, those of Monte Rosso rather affect an oblong or oval figure. The columns of San Giovanni measure, one with the other, near a foot in diameter; nor do they vary much in their size; though this is often the case in similar groups, and is particularly observable in that of Monte Rosso, whose columns sometimes equal nearly a foot in diameter, while others scarcely exceed 3 inches: their common width is about 6 or 8 inches. They differ therefore very considerably, in size, from those of the Giants Causeway; some of which, it is well known, measure 2 feet in width. Nothing certain can be said concerning the length of the columns of San Giovanni, as they present only their tops to view; the remaining parts of them being deeply buried in the hill, and in some places entirely covered. The columns of Monte Rosso, as far as they are visible, measure only from 6 to 8 or 10 feet in height; which is also a small size, when compared with the height of those of the Giants Causeway, some of which measure near 40 feet. The columns of the Venetian groups manifest however all the varieties of prismatic forms, that are observable in those of the Giants Causeway, and other similar groups. But they are commonly either of 5, 6, or 7 sides; but the hexagonal form seems mostly to prevail, which is also remarkable in the Giants Causeway, and probably in most others. Nor is there less difference in the texture and qualities of these columns, than in their forms. Those of San Giovanni present a smooth surface, and, when broken, appear within of a dark iron grey colour, manifesting also a very solid and uniform texture; in which characters they correspond with the columns of the Giants Causeway, and those of most other basaltine groups. But the columns of Monte Rosso are very different in all these respects. For they have not only a

very rough, and sometimes knotty surface, but, when broken, show a variegated colour and unequal texture of parts. They are commonly speckled, as it were, more or less distinctly, and resemble an inferior sort of granite, of which Monte Rosso itself is formed, and which serves as a base to the range of columns in question. It is, in general, not quite so hard as the Alpine and Oriental granites, and is sometimes even friable. Linnæus justly observes, that this species of granite abounds in France; for I have lately seen large tracts of it in the neighbouring provinces of Auvergne, Velay, and Lionnois; and apprehend, that it likewise abounds in the Vivarey, Gevaudan, and Sevennes mountains; from the affinity observable in the physical geography of those countries. But it is equally common in Italy; for besides Monte Rosso, the bulk of the Euganean hills in general, of which that is a part, principally consists of it; and these hills occupy a considerable tract in the plains of Lombardy. It is also common in the Tuscan and Roman States: the mountain close to Viterbo, on the road to Rome, is entirely composed of it. The columns of Monte Rosso appear therefore of a different character from any hitherto described by mineralogists, who only mention those of a uniform colour and texture. But the great singularity here is, that such a range of prismatic columns should be found bedded, as it were, in a mass of granite, and composed nearly of the same substance; of which I never yet saw or heard any other instance. This circumstance seems therefore to render the causeway of Monte Rosso more curious and singular than the famous one in Ireland is known to be, from the regular articulation of its columns; the same phenomenon having lately been discovered at Staffa, one of the western islands of Scotland. Different groups of articulated basaltine columns have likewise been observed in the province of Auvergne in France; particularly by M. Beost de Varennes, at Blaud near Langeac; and by M. Desmarests, near le Mont d'Or. M. Sage also mentions another near St. Alcon, in the same province. The Monte Rosso group is, however, not only curious in itself, but very interesting, on account of the great light it seems to throw on the origin of granites in general.

It is remarkable, that the columns in the two different groups of Monte Rosso and San Giovanni, preserve respectively the same position, nearly parallel to each other; which is not commonly the case in other basaltine groups. For though the principal aggregate, which forms the Giants Causeway, stands in a direction perpendicular to the horizon; yet other small detached groups of columns also appear in the hill above, that affect by their position, different degrees of obliquity. Among the numerous basaltine hills of Auvergne and Velay, in France, which seem to abound in those provinces more than in any other part of Europe, and perhaps of the known globe, nothing is more common than to see the columns of the same group lying in all possible directions, as

irregularly almost, as the prisms in a mass of common crystal. Nor is this variety of position so observable in single columns, as in whole masses or ranges of them, which often present themselves in the same hill, disposed in different strata or stages, as it were, one above the other, many of which affect very different, and even opposite directions. The columns of San Giovanni seem bedded in a kind of vulcanic sand, which, in many parts of the hill, entirely covers them; these however probably rest at bottom on a base of basaltine rock of the same nature. Nothing is more common in the provinces of France just mentioned, than to see isolated basaltine hills almost exclusively composed of different layers of columns, which present themselves in stages, one above the other, often without any other stratum between them, resembling, in some measure, si magna licet componere parvis, a huge pile or stack of cleft wood. Though the columnar crystallization of Monte Rosso is the only one I have yet seen, or heard of, in a mass of granite, yet other groups of columns have occurred to me in other parts, that are equally of a heterogeneous substance or texture, though different from those of Monte Rosso, as well as from the common basaltes.

These systematic mineralogists, in general, assign the same common origin to most lapideous solids, which they suppose to be generated by deposition from an aqueous fluid. In whatever manner therefore the prismatic bodies in question are classed, on such a principle, no adequate idea can thence be ascertained concerning their origin, which seems manifestly different. For surely the structure, and other phenomena of these bodies, sufficiently prove them to be crystallizations or concretions of a particular kind, and generated immediately from an igneous fluid: for they are not only peculiar to vulcanic tracts of country; but differ, in every respect, from common crystals produced from an aqueous fluid. Every one knows, that the latter are formed stratum super stratum, by a slow and successive deposition and juxta-position of parts, as hath been proved satisfactorily by Cappeler, Linnæus, and other writers on this subject. The same mode of generation is more particularly explained by Steno, in his excellent treatise, De Solido intra Solidum Naturaliter Contento. But this mode does not seem at all reconcileable with the basaltine crystallizations in question. For however these bodies may vary in their texture, yet none of them afford the least indication of an origin common to other crystals; but seem rather the effects of some intrinsic principle of organization, by which they appear to have been produced simultaneously, in a manner, on the consolidation of the whole mass of matter, in which they lie, and with which they constantly bear the greatest analogy, as before observed. It is further remarkable, that common crystals are parasitical bodies; whereas basaltine crystallizations, notwithstanding the peculiarities of their figures, rather seem to form integral parts of the masses

to which they adhere; and seem to acknowledge, with them, one common and simultaneous origin; like the rhomboidal and other crystallizations in granites, and other similar vitrifiable compound stones. The common slow and limited principle of crystallization, seems not at all adequate to so great an effect, which seems exclusively attributable to an igneous fluid, on the general concretion of which, the organic principle may be supposed to have operated simultaneously in a large mass, and produced these bodies in the same manner as a linget of metal concretes at once in the mould. No other mode of generation seems reconcileable with the phenomena of basaltine aggregates. It seems also further evident from the phenomena, that prismatic basaltine crystallizations, and other regularly figured vulcanic groups, have been generated locally, and not in the midst of those violent convulsions of Nature which are commonly assigned for the origin of vulcanic mountains in general. That the principle of organization, whatever it be, operates locally in the formation of these bodies, appears sufficiently evident from the regular disposition and other particular characters of their groups. For notwithstanding the various directions of the columns, and masses composed of them, in the different groups, yet in other respects the greatest regularity of disposition is commonly observable. They form strata, which are uniformly organized, disposed in particular directions, and often constant in the same to a great extent. These strata not only manifest a parallelism between their regularly figured parts, but in their whole aggregates; which often form extensive horizontal beds, and of an equal thickness throughout. This parallelism is also equally remarkable in groups that are composed of many strata; as I have particularly observed in those of Murat, and the Castle Hill of Achon, in Upper Auvergne; in which the columnar strata are not only parallel in themselves, but preserve in their position, a parallelism with the other strata of the respective groups, which lie in regular stages, one above the other: and since these groups commonly form, in a manner, integral parts of the masses, or mountains in which they are found, and these manifest also some affinity in their structure; it seems most reasonable to assign to both one common origin.

The Euganean hills form an irregular group in the plain of Lombardy, about 7 miles nearly south by west from Padua, and extend from north to south as far as Este. The most considerable part of them composes an irregular sort of chain, which extends in the above direction; while other parts are severally detached, and form isolated mountains about the skirts of this chain, particularly on the north-east side, towards Abano. The outer skirt of the entire group may measure from 30 to 40 English miles. The external characters of this group exactly correspond with the forms commonly ascribed by naturalists to vulcanic mountains in general; since the points of the chain before mentioned, as well as the isolated members of it, are of various conical, orbicular, and elliptical shapes.

As this group, therefore, rests on a perfect plain, it makes a very singular appearance. The vulcanic hills immediately round Isenchaux in Velay affect also the same forms; but as they are mixed with other hills of a different form, and the country about them is broken and irregular, they do not produce so singular an effect as the Euganean hills, which suddenly rise from a perfect level. I am informed, that there is a similar, though smaller group of isolated vulcanic hills in a plain of Dalmatia, near Cossovo; and another group of hills, nearly of the same forms, in the county of Down, in Ireland, and called the Mourn hills; which, like those near Padua, consist mostly of granite and lava. The Euganean hills have, moreover, a superficial and partial covering of slaty and calcarious strata, of posterior origin, and that manifest no marks of having suffered by fire. Such strata slightly cap mount Venda, which is the highest among these hills; though of no very considerable elevation, measuring only about 252 French toises above the Venetian Lagunes, according to Abbé Toaldo, professor of astronomy at Padua. From the lava and granite mixed together in the Euganean hills, they bear an affinity with those of Auvergne and Velay; but differ from them by the superincumbent unburnt strata of lime-stone.

III. An Inquiry, to show what was the Ancient English Weight and Measure according to the Laws or Statutes, prior to the Reign of Henry the Seventh. By Henry Norris, Esq. p. 48.

William the Conqueror, by his charter, confirmed to the English all their ancient laws, with such additions or alterations as he made to their advantage. The 57th clause of that charter is, "De mensuris et ponderibus. Et quod habeant per universum regnum, mensuras fidelissimas et signatas, et pondera fidelissima et signata sicut boni prædecessores statuerunt." From this clause it seems clear, that king William ordained sealed standards, both of weights and measures, to be made, such as his predecessor king Edward had ordained. Neither weights nor measures are here described particularly; but the subsequent statutes define them more plainly. And the Chronicon Pretiosum tell us, that from historians it appears the Conqueror determined what the weight of the sterling penny, or penny-weight should be, to weigh 32 grains dry wheat. Consequently the standard penny-weight was made equal to the weight of 32 grains of wheat. Succeeding kings confirmed William's charter; and even the great charter granted by king John is only to explain and restore the ancient laws, which had been infringed. The statutes of 51st of Henry III, and 31st of Edward I, explain the ancient weights and measures; that is to say, the English penny called a sterling, round without clipping, was to weight 32 grains dry wheat, taken from midst of the ear, and 20 of those penny-weights were to make an ounce, and 12 ounces a pound; and 8 of those pounds were to be a gallon

of wine, and 8 of those gallons to make a London bushel, which is the 8th part of a quarter. The definition of the penny-weight, in these statutes, agrees with the determination of William the Conqueror, and shows that the legal weight continued the same. What the weight of that pound was, so raised from a penny-weight, equal to the weight of 32 grains of wheat, we may clearly learn from that declaration in the 18th of Henry VIII, when he abolished that old pound, and established the Troy weight; which says, that the Troy pound exceedeth the old Tower pound by $\frac{3}{4}$ of the ounce. As the Troy pound established by Henry VIII, is the same as is now in use, consisting of 5760 Troy grains, and 480 grains to the ounce, and 12 ounces to the pound; so 360 grains is $\frac{3}{4}$ of the ounce, which, deducted from 5760, leaves 5400 Troy grains, equal to the weight of that old Saxon pound which he abolished. But to trace out experimentally the weight of that penny-weight, raised from 32 grains of wheat, I got a small sample of dry wheat of last year 1773 (the weight of that year but ordinary); and, from a little handful of it, I told out just 96 round plump grains, dividing them into parcels of 32 grains each, and all 3 weighed exactly $22\frac{1}{2}$ Troy grains; consequently, 240 such penny-weights, which the old pound consisted of, were equal only to 5400 of our present Troy grains, conformable to the declaration of Henry VIII. Thus the weight of that old pound is clearly ascertained to be lighter than the present Troy pound by $\frac{3}{4}$ of an ounce; and it clearly shows that they were 2 different weights.

By those statutes of Henry III, and Edward I, it is said, that 8 pounds were to make a wine gallon, and 8 of those gallons to be a bushel, and 8 bushels a quarter; consequently the wine and corn gallon were one and the same measure. The statute of the 12th of Henry VII says, the gallon measure was to be 8 pounds of wheat, which ascertains what was to be understood by former statutes, and is consonant to reason, to fix the measure of wheat by its own weight, not by that of wine, as wheat was an article of greater importance to the community to ascertain its measure, than wine; and a gallon measure to contain 8 pounds of wheat, must be $\frac{1}{4}$ part larger in cubical contents than a measure to contain 8 pounds of wine. As it appears by the charter of William the Conqueror, that there were sealed standards made of weights and measures, we cannot doubt, but they were preserved and kept in the king's exchequer, for legal standards; and as several statutes direct their being made of metal, they were permanent and certain, by which to make more: which Henry VII expressly tells us he practised, by making new according to the old: so that there could be no need to recur to 32 grains of wheat, much less to 7680, every time new standards were to be made, unless we suppose our ancestors defective in common sense. Whenever, by new statutes, fresh standards were directed to be made, we may observe that the assize of weight and measure continued uniformly fixed and de-

scribed to be one and the same, to show there was no alteration made or intended. And thus, by the laws of assize, from William the Conqueror to the reign of Henry VII, the legal pound weight continued a pound of 12 ounces, raised from 32 grains of wheat, and the legal gallon measure invariably to contain 8 of those pounds of wheat, 8 gallons to make a bushel, and 8 bushels a quarter; the bushel therefore contained 64 of those pounds of wheat, and the quarter 512 pounds.

These were the legal weights and measures for common use, during that period. The first alteration really made therein, was in the 12th year of Henry VII. That the laws of assize were often infringed, is very evident from the frequent complaints, mentioned in Cotton's Abridgment of the Tower Records, against the king's purveyors; particularly in the 14th of Edward III, for remedy against outrageous takings of purveyors; and in the 45th of Edward III, that the king should be served by common measure; and in the 3d of Henry 5, that the king's purveyors do take 8 bushels of corn only, to the quarter striked. The general answers to which were, that the statutes should be observed. It appears also, that others infringed the laws of assize. For the statute of 27th of Edward III says, some merchants bought Avoirdupois merchandises by one weight, and sold by another; which plainly implies, they bought by some weight heavier than the legal, and sold by the legal weight which was lighter. The statute therefore, to enforce observance of the laws of assize, only wills and establishes, that there be one weight, one measure, and one yard, through all the land. This can be understood to mean no other than the legal assize, which preceding statutes had enacted. And further, in the reign of Henry VI, we see that buyers of corn, bought by a vessel, called a fat, of 9 bushels, which contained 72 gallons; and like those merchants before mentioned in the statute of Edward III, we may presume they sold by another measure, the legal quarter of 8 bushels, containing but 64 gallons: for the statute of 9th Henry VI forbids the buying by that vessel, called a fat. The prohibition implies the illegality of the vessel and its use, and implies also the enforcement of the laws of assize. Taking therefore all the several statutes together, in one connected view, those that fix the laws of assize, with those to reform abuses committed against them, we are led to conclude, that those laws of assize continued uniformly one and the same, till Henry VII altered them. Having thus shown by those laws, that the old pound weight was a Saxon pound of 12 ounces, raised from 32 grains of wheat, and was equal only to 5400 of our present Troy grains; and that the measure of capacity was a gallon, to contain 8 of those pounds of wheat, and 8 of those gallons made a bushel: I shall now endeavour, by help of figures, to demonstrate what was the cubical contents both of the gallon and bushel measures.

We know that the present Troy pound consists of 5760 Troy grains, and that

7000 of those Troy grains are equal to the present Avoirdupois pound of 16 ounces, and that 5400 of those Troy grains are equal to the old Saxon pound of 12 ounces; consequently, the old Saxon pound was $\frac{540}{576}$ of the present Troy pound, and the old Saxon pound was $\frac{54}{70}$ of the present Avoirdupois pound. We know that modern experiment has proved the weight of 1728 cubic inches of wheat, common sort, to be $47\frac{1}{2}$ pounds Avoirdupois; and of a better sort, to weigh from $48\frac{1}{4}$ to $48\frac{1}{8}$ pounds Avoirdupois; the difference in their weight is not very great; however I shall take the lowest weight to compute by, the $47\frac{1}{2}$ pounds Avoirdupois, which, in Saxon weight, is $61\frac{31}{54}$ pounds Saxon. And then say, as $61\frac{31}{54}$ pounds Saxon : 8 pounds Saxon :: 1728 cubic inches : $224\frac{1}{2}$ cubic inches, for the contents of the old Saxon gallon for wine and wheat. But as the old standard wine gallon kept at Guildhall, and found there in 1688, proves to be 224 cubic inches contents, there is reason to conclude it to be of the same standard assize, as was the ancient Saxon gallon for wine and wheat: for, as 1728 cubic inches : 224 cubic inches : $61\frac{31}{54}$ pounds Saxon : $7\frac{53}{54}$ pounds Saxon, which is about $4\frac{1}{2}$ penny-weights short of the 8 pounds, mentioned in the statutes for the gallon to contain, and is such a small difference, as may arise in different years, in the weight of such a quantity of wheat. The very near agreement of these computations, gives us sufficient reason to conclude, that the old standard wine gallon, of 224 cubic inches contents, found at Guildhall in 1688, was of the same standard assize, as was the ancient gallon measure ordained to hold 8 Saxon pounds of wheat; and of course then the bushel measure must have been 1792 cubic inches contents, which will appear to hold nearly 64 Saxon pounds of wheat, as by those old statutes it ought to do. For, as 1728 cubic inches : 1792 cubic inches :: $61\frac{31}{54}$ pounds Saxon : $63\frac{1476}{1728}$ pounds Saxon, which is only about an ounce and three quarters short of 64 pounds; and in so large a quantity of wheat, is a trifling difference, naturally arising in weight of wheat of different years. These demonstrations, by figures, sufficiently prove what the cubical contents of those ancient English measures must have been, according to the old statutes of assize, viz.

The gallon measure, 224 cubic inches contents, to hold 8 pounds Saxon.
The bushel........ 1792 ditto .... 64 ditto.

And as 8 bushels made a quarter, the quarter contained 512 Saxon pounds of wheat. These were the ancient legal measures, according to the old laws of assize.

It now remains to mention the particular statute of the 12th of Henry VII, under which an alteration was brought about in those ancient weights and measures, without seeming to intend it; as the statute itself differs not in substance from the other old laws of assize, except calling the pound by a new name, Troy. But previously it may be observed, that very probably the unsettled state

of the kingdom for many years preceding, might pave a way to that alteration. There had been several contests about the crown, between the two houses of York and Lancaster, till Henry VII by conquest mounted the throne; and in such times of public disturbance, the laws of assize were more likely to be infringed than well kept. For, after Henry VII was well settled on his throne, we find complaint was made in the 11th year of his reign, that the laws of assize had not been observed and kept. On which he made fresh standards of weights and measures, and sent them to the several shires and towns in the kingdom. But in the very next year, the 12th of his reign, there came out that particular statute, under which the weights and measures were altered; reciting that the king, in the former year, had made weights and measures of brass, according to the old standards remaining in his treasury, which weights and measures are said, on a more diligent examination, to have been approved defective. It is not said, whether they were the old standard weights and measures, or the new ones, made in the former year, that had been approved defective; nor how much they were so; all this is left to conjecture. Therefore we may with great probability conjecture, that they were not defective in respect to their old original standard; but only in respect to the heavier new Troy pound, intended to be then introduced. And what warrants such conjecture is, the express declaration of his son Henry VIII, when he abolished the old pound, in the 18th of his reign, and established the Troy; for he then declares, that the Troy pound exceeds the old pound by $\frac{3}{4}$ of an ounce. Hence then, there can be no doubt, but Henry VII altered the old English weight, and introduced a heavier Troy pound, that exceeded the old one by $\frac{3}{4}$ of an ounce; and though none of his standard weights have come down to us, yet his brass bushel measure, with his name on it, was found in the exchequer in 1688, and proves to be 2145 cubic inches contents; from which we may form conclusions, both on his weights and measures, sufficient to convince us that he altered both. That his bushel was a measure of 9 gallons instead of 8, and that his Troy pound was $\frac{1}{16}$ part heavier than the old English pound, which was raised from 32 grains of wheat. Experiment has proved, that a measure of 1728 cubic inches of wheat, will weigh from $47\frac{1}{2}$ to about $48\frac{1}{4}$ pound Avoirdupois; but suppose it be only $47\frac{3}{4}$ pounds Avoirdupois, that, in Troy weight, will be $58\frac{17}{576}$ pounds Troy. Hence we may easily find the weight of wheat that 2145 cubic inches will contain. For, as 1728 cubic inches : 2145 cubic inches :: $58\frac{17}{576}$ pounds Troy : 72 pounds Troy, the weight of wheat that Henry VIIth's bushel would contain. And dividing the 72 by 8, the number of pounds limited by the statute to a gallon, it proves Henry VIIth's bushel was a measure of 9 gallons instead of 8; and as 8 bushels made a quarter, then the quarter contained 72 gallons; which seems to correspond with the number of gallons contained in the vessel, called a fat, the use of which was

prohibited by statute in Henry VIth's time, about 60 years before Henry VII, as before remarked. If we divide the 2145 cubic inches contents of the bushel, by 9, the number of gallons it contained, it shows the gallon measure to be 238$\frac{1}{3}$ cubic inches contents, which is $\frac{1}{16}$ part larger than the old Saxon gallon of 224 cubic inches, just in the proportion as the Troy pound is $\frac{1}{16}$ part heavier than the old Saxon pound. The statute limits the gallon to hold 8 pounds Troy of wheat; and so we find the gallon of 238$\frac{1}{3}$ cubic inches will do; for as 2145 cubic inches : 238$\frac{1}{3}$ cubic inches :: 72 pounds Troy : 8 pounds Troy. But if it be said, that the statute limits the bushel to 8 gallons, not 9, then the gallon measure must have been 268$\frac{1}{8}$ cubic inches contents, and would hold 9 pounds Troy of wheat, though the statute says it was to hold only 8 pounds Troy. Take it either way, it shows that the bushel was not made according to the statute; it held 72 pounds instead of 64 pounds. And on the whole it clearly proves, that Henry VII altered both the weights and the measures; that he introduced the Troy pound, which was heavier by $\frac{3}{4}$ of an ounce than the Saxon or old English pound; and that his bushel measure was about $\frac{1}{6}$ part larger than the ancient Saxon or old English bushel measure. The first statute that directs the use of the Avoirdupois weight, is that of the 24th of Henry VIII; which plainly implies it was no legal weight, till that statute gave it a legal sanction, and the particular use to which the said weight is there directed, is simply for weighing butchers meat in the market. And it is note-worthy, that in all the old statutes of assize prior to Henry VII, the legal gallon measure of capacity is founded on 8 pounds, raised from the weight of 32 grains of wheat, and by that statute of 12th Henry VII, the gallon is to contain 8 pounds Troy: therefore these 2 sorts of weight were the only ones established as legal by the statutes; and both are a lighter weight than Avoirdupois. How, or when, the Avoirdupois weight came first into private use, is not clearly known to us; but this seems clear, that no statute before the 24th Henry VIII has given it any legal sanction.

IV. Of an Apparatus for Impregnating Water with Fixed Air; and of the Manner of Conducting that Process. By John Mervin Nooth, M.D., F.R.S. p. 59.

The possibility, says Dr. N., of impregnating water with fixed air was no sooner ascertained, by experiment, than various methods were contrived to effect the impregnation. Dr. Priestley, however, is the only one that has published any description of an apparatus, calculated entirely for this purpose. This apparatus was communicated to the public, with the view of promoting the discovery of the medical effects of fixed air united with water; and, in consequence of this communication, some very successful attempts have been made in the cure of diseases. The experiments however have not been so numerous as one could

have wished; perhaps the difficulty in conducting the process, in the manner proposed, has been, in some measure, the reason why so few experiments, on this subject, have been made public. For though, in the hands of Dr. P., the apparatus was sufficiently convenient, it must be confessed, that the conduct of the process required more address, than generally falls to the share of those that are unaccustomed to such experiments. Independent too of the inconveniencies attending the process, there was another objection to the apparatus, which, with most people, might have considerable weight. The bladder, which formed part of it, was thought to render the water offensive; and when the solvent power of fixed air is considered, it will not appear improbable, that the water would be always more or less tainted by the bladder. In some trials which Dr. N. made with Dr. Priestley's apparatus, it always happened, that the water acquired a urinous flavour; and this taste in the water was, in general, so predominant, that it could not be swallowed, without some degree of reluctance. The difficulty therefore in the conduct of the process, and the offensiveness of part of the apparatus, made some less exceptionable method of producing the impregnation desirable. This Dr. N. variously attempted, keeping convenience and cleanliness constantly in view; and he flattered himself, that he had at last contrived an apparatus that would perfectly answer the intended purpose. Twelve months had elapsed since this contrivance had been in constant use; and to that time there was no reason to wish for the least alteration. Presuming therefore on the possibility of its becoming, when known, extensively useful, and convinced of the favourable reception which every attempt of this nature meets with from the R. S., he begged leave to communicate a description of the apparatus that he had invented, and of the manner of conducting the process.

The apparatus is of glass, and consists of 3 vessels as A, B, C, fig. 1, 2, 3, pl. 12. The glasses are accurately fitted to each other, and at the joints are impervious both to air and water. The glass A is designed for the effervescing substances. The vessel B is to contain the water to be impregnated with air. In the lower part of the glass B is placed an ivory valve, surrounded with cork, as in fig. 4. The cork a is fitted to the bottom of the glass B, and has through it a hole, to receive the part b of the ivory valve. On the broader part of this piece b, is placed a moveable piece c. The surfaces of these pieces are so accurately ground, that, when applied to each other, no fluid whatever can pass between them. The moveable part c is secured on the part b by the cover d, which is so constructed, as to allow the piece c some motion, and this cover has likewise holes to give passage to the air that shall raise the moveable piece c. The glass C serves 2 purposes; it confines the air on the surface of the water in B, and at the same time prevents all danger of explosion by allowing the water to give place to the ascending air.

The Process.—As chalk and oil of vitriol are capable of producing the desired effervescence, and are the most eligible on account of their cheapness, he has, in describing the process, mentioned only these 2 ingredients. Various other substances may however be employed for the same purpose; but none perhaps are so unexceptionable as those named. In the other acids a proper degree of fixity is wanting, during the effervescence; the nitrous and marine have so much volatility, that there is always a risk of some of the acid fumes passing the valve, and thus rendering the water acid, which it was intended to impregnate only with fixed air. To begin the process, it is necessary to fill the vessel A up to the dotted lines, with diluted oil of vitriol. By confining the height of the surface of the effervescing mixture to the dotted lines in the glass A, none of the acid will be driven through the valve, during the intumescence that attends the escape of the fixed air. The glass B is to be totally filled with water, and the vessel C is to be put on it. Some powdered chalk is then to be thrown into the glass A, and the vessels are to be immediately placed as in fig. 5, except that the stopper belonging to C is to be left out. When the acid in the lowermost vessel acts on the chalk, the extricated air passes the valve in the middle glass; and as the construction of this valve allows the fixed air from the effervescing substances to pass, but denies a passage to the water in a contrary direction, the separated air ascends to the upper part of the middle glass, and at the same time a portion of water, equal in bulk to the intruding air, passes up the bent tube into the uppermost vessel. As the effervescence goes on, the fixed air continues to accumulate in the middle vessel, and the uppermost one to be filled with the water that has given place to the air. The quantity of chalk to be thrown into the acid at one time, must be determined by the capacity of the uppermost vessel. Should more air be extricated than is sufficient, in the conduct of the process, to fill that vessel, the water will run over the top of it, and will continue to run as long as any air ascends in the middle vessel, or till the surface of the water is below the extremity of the bent tube. Both these accidents are to be carefully avoided; as in one case the whole would be wet and disagreeable; and in the other, a quantity of fixed air would be unnecessarily lost. Half a drachm of chalk will, in general, produce air enough to fill the uppermost vessel with water; and it must be remembered, that the chalk employed to produce the effervescence, should be finely powdered, as a selenetic crust will otherwise form around it, and thus prevent the action of the acid on the interior part. To keep the neck of the glass clean, through which the chalk is put, it will be necessary to include the chalk loosely in paper; and this circumstance is by no means to be neglected, as the accurate junction of the glasses depends on it, and consequently the whole of the process. When the uppermost vessel is filled with water, and there is therefore a considerable quan-

tity of fixed air in the middle one, these two vessels are to be separated from the lowermost, and the air and water are to be agitated together, to promote their union. If, during the agitation, a stopper be put into the uppermost glass, the descent of the water in it will not show the absorption of the fixed air by the water, as the external atmospherical air will enter below, at the valve, to fill the space which the absorbed fixed air would otherwise leave void. But, on the contrary, if the uppermost vessel be open, during the agitation, the pressure of the atmosphere on the surface of the water in that vessel, will force the water down into the middle one, as fast as the absorption of the fixed air below will allow it room. This latter method may be pursued, when a person wishes to know the quantity of fixed air that the water can absorb; but in common use, it will be better to stop the uppermost vessel, as the air and water may be then more forcibly agitated without inconvenience, and of course the impregnation more expeditiously effected. During the effervescence, the uppermost glass is to remain open, and it is only to be stopped when the agitation is performed. It is not to be expected, that the impregnation will be considerable at first; it will indeed be necessary to repeat the process, with the same water, 4 or 5 times, before it will be highly impregnated. After an agitation therefore, when a stronger impregnation is wished for, the uppermost vessel is to be opened, and raised from the middle one, to allow the water to descend, that was before driven up. When the middle glass is again full, a fresh quantity of chalk is to be put in the lowermost vessel, and the agitation to be repeated, as soon as the effervescence ceases. It is seldom necessary to repeat the process more than 4 times, to produce a very strong impregnation; but should it be thought proper to have the water as highly saturated with fixed air as it admits of, nothing more than a repetition of the same process is requisite. In this account of the apparatus, he had purposely confined himself to the method of uniting fixed air with water; but it is to be observed, that many curious experiments may be made with it, both in chemistry and pharmacy. By its assistance, Dr. N. had been enabled to imitate very perfectly, the common mineral waters, and to make aqueous solutions of substances that were before deemed insoluble in water. These circumstances however he had reserved for a future paper, which he should have the honour to present to the society, as he had not then been able to arrange the several facts, which this apparatus had made him acquainted with, in the manner he could wish.

P. S. Since the foregoing paper was read, Dr. N. had contrived a glass valve, which seems preferable in some respects to the ivory one. The following is a description of it. It consists of 3 pieces, as in fig. 7. The superior and inferior pieces are perforated, but the middle one is without perforation, having only its upper part convex; and its under part plane. In fig. 8 is a perpendicular

section of the 3 pieces composing the valve, at the distance at which they ought to be placed, with respect to each other, in the tabular part of the vessel B. This vessel having the glass valve in it, and filled with water, is to be put on the glass A containing substances in the act of effervescence. In that case, the extricated air will ascend through the perforations in the superior and inferior pieces, the middle one proving no obstacle to the air, having sufficient room to yield to the current of air rushing upwards; but when the air ceases to ascend, and the pressure of the water above takes place, the middle piece will prevent the water from descending, its plane surface being then applied to the plane surface of the piece below it. Thus this glass valve will answer in every case where the ivory one can be employed; and for a variety of purposes it will undoubtedly prove preferable, particularly when corrosive substances are subjected to experiment.

V. Of a Musical Instrument, brought by Captain Fourneaux from the Isle of Amsterdam in the South Seas, to London, in 1774, and given to the R. S. By Joshua Steele, Esq. p. 67.

This instrument consisted of a system of 9 musical pipes, of various lengths, and conected together in a parallel position. The manner of blowing them, in making the experiments, was the same as people use to whistle in the pipe hole of a drawer key. The upper series of tones, which are exact 5ths to the lower, are easiest produced by an unexperienced person; and the lowest series, which we shall call fundamentals, with somewhat more address and a weaker blast. Besides the abovementioned tones, if the velocity of the breath be increased a little, the first 5 pipes will give octaves to the fundamentals, and if further increased, sharp 3ds, or tierces, above these octaves. In the pipes 6, 7, 8, 9, Mr. S. could neither make the octaves to the fundamentals, nor the sharp tierces; but in their stead, the minor, or flat 3d, above the octave came, when the breath was urged beyond the degree requisite to produce the 5th. This minor 3d, is an accident out of the natural order of tones produced from simple tubes, which he does not pretend to account for. Mr. S. then adds the notes of the several tones which he produced from each pipe.

VI. Remarks on a Larger System of Reed Pipes from the Isle of Amsterdam, with some Observations on the Nose Flute of Otaheite. By Joshua Steele, Esq. p. 72.

The nose flute of Otaheite, gives only 4 sounds, with the first degree of breath, which are, in an ascending series, by a semitone, a tone and a semitone. If urged with a stronger breath, it will give octaves above these; but it then becomes ill in tune: and it seems, the natives of Otaheite use no more than those first 4

sounds. Notwithstanding the small extent of this series, yet, by the aid of varying the measure, it is capable of several different melodies, though the general cast of them will be melancholy.

The specific difference between this system of pipes, and the smaller, described before, will be understood from the following observations. It consists of 10 pipes, joined together in the same manner as those of the smaller system. The first 9 pipes exhibit to the eye the same figure as the system before described; and the 10th pipe is a little longer than N° 4. For in this larger system, N° 8 is 13 inches long; N° 4 $13\frac{1}{2}$, nearly; and N° 10 is 14 inches. The sounds which each pipe exhibits easily, are marked in minims. As the upper minims are 6ths to those next under them, it follows, from the law of harmonic sounds, that the lower minims are 5th to the fundamental sounds of these pipes, which are written in quavers, to show that they are very difficult to be produced. The upper minims of N° 1, 2, 3, 4, 5, and also of 10, are sharp 3ds, or rather, major 10ths, to the fundamental sound of each pipe. And the upper minims of N° 6, 7, 8, 9, are nearly minor tenths to their fundamentals; which circumstance seems to agree with what was remarked in the smaller system, as an extraordinary property, touching the minor 3ds. But Mr. S. will not yet assert, that this property is altogether natural, because he found some of the latter pipes were partly obstructed by accidental rubbish, which was drawn out with difficulty: so that he pretends not to decide, whether the cause of their being, not quite, in the same proportion of tune, as he found in the first system, arises from some casual injury, or from original intention, or original inaccuracy. The interval between N° 1 and 2 in these pipes, is only of 2 semitones; whereas that between the N° 1 and 2 of the former system, was of 3 semitones. The series N° 2, 3, 4, 5, and the series N° 6, 7, 8, 9, have similar intervals in both systems. Therefore he imagines these to have been the original extent of the whole modulating series, like the double tetrachórd of the Greeks, and that the N° 1 and N° 10 are additionals at pleasure; as, in the smaller system, the interval between N° 1 and 2 was a semitone greater than that between N° 1 and 2 in the larger system; and N° 10 in the smaller system was totally omitted, though he had seen 2 others which had it. The sounds in this larger system are 7 tones lower than those of the smaller, which corresponds with the difference of their dimensions; the pipe N° 4 in this system measuring nearly $13\frac{1}{2}$ inches in length, with diameter seemingly proportional; whereas the N° 4 in the smaller system measured only $7\frac{1}{4}$ inches. By increasing the velocity of the blast, these pipes gave sounds still higher, which were 4ths above the upper minims, or octave and 6ths above the fundamentals; and with a little more force, tritones, or sharp 4ths, above the upper minims, which were octave and flat 7ths above the fundamentals.

VII. Description of a New Dipping-Needle. By Mr. J. Lorimer, of Pensacola. p. 79.

Whenever any one meets with a terrella, or spherical loadstone, the first thing he does is to find out its poles; and having once discovered them, he knows immediately how any small bit of needle will be affected, when placed on any part of its surface. The poles are most readily discovered by trying where the filings of iron, or a small bit of needle, will stand erect on the terrella; and this is generally found to be on 2 points diametrically opposite to each other. But the magnetic poles of the earth seem to be situated obliquely to one another (see the Berlin Memoirs, 1757); but where they are actually situated, is hitherto unknown; whether they are on land or water. Yet be these things as they may, it appears evident, that accurate observations, made as near to these magnetic poles as possible, with a good dipping-needle, are the surest way to complete the magnetic theory of this globe, analogous to the method we pursue in examining the terrella. But as all the dipping-needles appeared to be very ill calculated, for the sea service at least, Mr. L. contrived one on a different plan in 1764, and had it executed by Mr. Sisson. He called it a universal magnetic needle, or observation compass; because he could by it take the dip and amplitude, and even the azimuth, with only one assistant, to take the altitude. The needle was of the same shape and size nearly as those used for the compasses of the royal navy, and played vertically on its own axis, which had 2 conical points, slightly supported in 2 corresponding hemispherical sockets, inserted into the opposite sides of a small upright brass parallelogram, about $1\frac{1}{2}$ inch broad, and 6 inches high. Into this parallelogram is fixed, at right angles, a slender brass circle, about 6 inches diameter, silvered and graduated to every half degree, on which the needle shows the dip; and this, for the sake of distinction, he calls the circle of magnetic inclination. This brass parallelogram, and consequently the circle of inclination, also turns horizontally on 2 other pivots, the one above and the other below, with corresponding sockets in the parallelogram. These pivots are fixed in a vertical brass circle, of the breadth and thickness of $\frac{2}{10}$ of an inch, and of such a diameter, as to allow the circle of inclination and the parallelogram to move freely round within it. This 2d he calls the general meridian. It is not graduated, but has a small brass weight fixed to the lower part of it, to keep it upright; and the circle itself is screwed, at right angles, into another circle, of equal internal diameter, of the same thickness, and twice the breadth, which is silvered and graduated on the upper side to every half degree. It represents the horizon, as it swings freely on gimbols, and is always nearly parallel to it. The whole is contained in a neat mahogany box, of an octagon figure, with a glass plate at top and one on each side, for about $\frac{2}{3}$ down. That part of the frame which contains the

glass lifts off occasionally. The whole box turns round on a strong brass centre, fixed in a double plate of mahogany, glewed together cross-ways, to prevent its warping or splitting; and this again is supported by 3 brass feet, such as are used for the cases of table knives, frosted that they may not easily slip, if the vessel should have any considerable motion. It has another square deal box to lock it up in, to preserve the glass, &c. when it is not wanted for use.

The use of this instrument is very plain, as the inclination or dip is at any time apparent from inspection only, and also the variation, when the frame is turned round till the great vertical circle lies exactly in the plane of the true meridian: for the circle of inclination being always in the needle's vertical plane, its edge will evidently point out on the horizon, the variation E. or W. But at sea, when there is not too much motion, the frame is turned round, till the vertical circle be in the plane of the sun's rays; that is, till the shadow of the one side of it just covers the other, and the edge of the circle of inclination will then give the magnetic amplitude, when the sun is rising or setting; but the azimuth at all other times of the day; and the true amplitude or azimuth being found in the usual way, the difference is the variation. When the motion is considerable, observe the extremes of the vibration, and take the mean for the magnetic amplitude or azimuth. When the sun does not shine so bright as to give a shadow, set the brass circle in a line with his body, if he be at all visible by the eye. The principal advantage at first aimed at in this compass, was to contrive a dipping-needle, which should be sufficient for making observations at sea. As those needles, to be of use, must be placed, by some means or other, in such a manner, as that all their vibrations shall be made in the true magnetic meridian, north and south, otherwise they are good for nothing. For if one of them be placed at right angles, across the magnetic line, it will stand perpendicularly up and down in any part of the world; the least dip therefore is always in this magnetic line. But the only method of setting a dipping-needle at sea, was to place it in a line with the common compass needle; and this must be very inaccurate, if they be at any considerable distance from each other; or if they be near, the 2 needles would influence each other, and neither of them could be true; nay, supposing them for once to be properly placed in this line, the least motion of the ship throws them out again. But this instrument has a constant power in itself, not only of setting itself in the proper position, but also of keeping itself so; or of restoring itself to the same situation, if at any time it has lost it; and it is curious to see how, by its double motion, it counteracts, as it were, the rolling motion of the vessel.

VIII. Bill of Mortality for Chester for the Year 1773. *By J. Haygarth, M. D., F. R. S.* p. 85.

That Chester is healthy to a very remarkable degree, is still more clearly evinced from the register of this year, than in that of the last. In 1772, one half of the inhabitants appeared to arrive at 20 years of age; a fact which seemed very surprizing when compared with the proportional mortality in other towns, both of a larger and less size. But, according to this year's register, one half have lived to be 36 years old. In 1772, 1 in 15¾ had lived to above 80, and this year 1 in 13. These are very uncommon instances of longevity for so large a proportion of the inhabitants. The inhabitants of St. Michael's parish were numbered to be 618, of whom this year 10 only have died; that is, a less proportion than 1 in 61. If the inhabitants of the whole city were numbered with the same accuracy as those of St. Michael's, many important conclusions, both medical and political, might with certainty be deduced from the bill of mortality. The register of burials in the 9 parishes are kept separate; hence, by comparing the number of inhabitants in each parish with the burials in each, for a period of years, we may, on the most evident foundation, discern which part of the town is most healthy. In a political view, such an account would furnish the best means of demonstrating the accuracy of a table of the probabilities of life, formed from the register, and supply unerring data for calculating annuities, the value of reversionary payments, and assurances on lives. Such an old town as Chester, where the number of inhabitants has for many years suffered little variation, and where the births and burials are nearly equal, is peculiarly well fitted to furnish this important information. The registers confirm the observation, that women live longer than men. Of those who have lived to above 80, only 10 are males, and 17 females; the number of widowers this year is 17, of widows 44. The table of diseases of different ages confirms in general the observations of last year. It is evident that no epidemic visited this place in 1773; not one died of the measles, or miliary fever, and the 10 who sunk under the chincough had probably lingered under the disease since the former year, towards the end of which it ceased to be epidemic. Only one died of the natural small-pox; 12 were inoculated in Chester, during this year, and all recovered.

IX. *Experiments on a New Colouring Substance from the Island of Amsterdam, in the South Sea. Made by Mr. Peter Woulfe, F. R. S.* p. 91.

This substance is of a light bright orange colour; has a peculiar, though not a strong smell; and, when handled, gives a yellow stain to the skin, which does not readily wash out with soap and water. Put on a red hot iron, it smokes, melts, and catches fire, leaving a caput mortuum. When boiled with water, it gives the liquor only a slight yellow tinge, which is but little heightened by the addition of a fixed alkali; therefore the colouring part of this substance is

insoluble in water. Oil of vitriol put to it becomes of a red orange colour; but, when the acid is drained off, the residuum appears purple. Annotto, treated in the same manner, gives a blue colour. Spirit of wine, æther, fixed and volatile alkalies, as also soap, dissolve the colouring part of this substance. To determine the quantity of colouring matter which it contains, 2 drs. were digested in a mattrass, with 4 oz. of rectified spirit of wine; the solution, being filtered, assumed a rich deep yellow colour, like a strong solution of saffron or gamboge with the same spirit; what remained in the filter was digested a 2d time, with 4 oz. of fresh spirit of wine, and the liquor filtered; this solution was much weaker than the first. The undissolved part remaining in the filter after this 2d solution was digested, a 3d time, with 4 oz. of fresh spirit; but the solution was now quite weak, and of a very pale yellow colour. The residuum being now deprived of its colouring portion, was slowly dried, when it appeared of a very pale yellow colour, felt as soft as starch between the fingers, and weighed 42 grs.; so that $\frac{4}{5}$ nearly of this colouring substance are soluble in spirit of wine; the undissolved part is not soluble in water, acids, or alkalies. Put on a red hot iron, it smokes and catches fire without melting, leaving a caput mortuum, and gives a smell similar to that arising from common vegetable matter. The first solution in spirit of wine, after standing 24 hours, deposits some of its colour in the form of minute spiculine crystals, of an orange colour. The 2d and 3d solutions let fall none of their colour. The 1st solution, dropped on paper, tinges it of a bright orange colour, the 2d gives a lively yellow colour, and the 3d a pale yellow. The 1st solution, sufficiently diluted with spirit of wine, makes a bright yellow stain on paper, no way inclining to an orange, but exactly resembling that made by the 2d solution; hence it seems probable, that an orange colour is only a deep yellow. Vitriolic æther readily dissolves the colouring part of this substance, and affords solutions of nearly the same colour as those made with spirit of wine. Oil of turpentine dissolves but a small portion of it, and acquires only a pale yellow colour. A solution of fixed alkali in water, digested with this substance, dissolves a large portion of its colouring part, and the solution is of a brownish yellow colour. Volatile spirit of sal ammoniac, seems to dissolve a larger portion of it than the fixed alkali, and the solution is of a reddish orange colour. A solution of soap in water, boiled with this substance, likewise dissolves its colouring part. All the foregoing solutions, except that in oil of turpentine, which was not tried, dye silk, cloth, and linen, of various shades of yellow and orange; but these colours are discharged, by boiling the dyed substances for some time in soap and water. This colour can therefore be of use only in dying silk and wool, for which purpose we are already furnished with good dyes. Few colours go so far in dying as this new substance, and none dye so speedily, especially when soap and water are used as the solvent; for a dip or two will dye cloth or silk of a lively yellow colour,

when put into the mixture while hot. Soap and water may be perhaps used with advantage, as the solvent for several other colours.

From the foregoing experiments it appears, that this colouring substance, on which they have been made, is of the resinous kind, and has a good deal of affinity with annotta.

X. Experiments and Observations on the Gymnotus Electricus, or Electrical Eel. By Hugh Williamson, M. D., of Philadelphia. p. 94.

A sea-faring man brought to this city a large eel, that had been caught in the province of Guiana, a little to the westward of Surinam. It had the extraordinary power of communicating a painful sensation, like that of an electrical shock, to people who touched it, and of killing its prey at a distance. The eel was 3 feet 7 inches long, and about 2 inches thick near the head. On a transient view it resembled one of our common eels both in shape and colour; but its head was flat, and its mouth wide, like that of a cat-fish, without teeth. A fin, which was above 2 inches broad, extended along its belly, from the point of its tail to within 6 inches of its head. This fin was almost an inch thick where it adhered to the body; the upper part of it was muscular, but of a very different texture from the muscular part of the body; the difference was obvious to the touch, but Dr. W. had no opportunity of making any observations by dissecting the subject. It was a native of fresh water, and breathed at the interval of 3 or 4 minutes, by lifting its head to the surface.

Exper. 1. On touching the eel with one hand, Dr. W. perceived such a sensation in the joints of his fingers as he received on touching a prime conductor or charged phial, when no circle was formed; or such as he had received, when a few sparks of the electric fluid have been conveyed through his fingers only. 2. On touching the eel more roughly, he perceived a similar effect in his wrist and elbow. 3. Touching the eel with an iron rod, 12 inches long, he perceived the like sensation in the joints of the thumb and fingers with which he held the metal. 4. While another person provoked the eel by touching it, Dr. W. put his hand into the water at the distance of 3 feet, and felt such a sensation in the joints of his fingers as when he had touched the eel, but not so painful. 5. Some small fishes were thrown into the water where he was swimming; he killed them immediately, and swallowed them. 6. A cat-fish,* that was at least 1½ inch thick, was thrown into the water where the eel was swimming; he killed it also, and attempted to swallow it, but could not. 7. To discover whether the eel killed those fish by an emission of the same fluid with which he affected the hand when touched, Dr. W. put his hand into the water, at some distance from the eel; another cat-fish was thrown into the water; the eel swam up to it, but

* The Bayre de Rio of Marcgrave.—Orig.

presently turned away, without offering any violence. After some time he returned; when, seeming to view it for a few seconds, he gave it a shock, by which it instantly turned up its belly, and continued motionless; at that very instant Dr. W. felt such a sensation in the joints of his fingers as in experiment 4. 8. A third cat-fish was thrown into the water, to which the eel gave such a shock, that it turned on its side, but continued to give signs of life. The eel seeming to observe this, as it was turning away, immediately returned, and struck it quite motionless. It could easily be perceived that the last shock was more severe than the former. The eel never attempted to swallow any of those fish after the first, though he killed many of them; and whenever he was going to kill one, he swam directly up to it, as if he was going to bite it; when he came up, he sometimes paused before he gave the shock, at other times he gave the shock immediately. On removing any of those cat-fish, though apparently dead, into water in another vessel, they presently recovered. Fish that are stunned by a small electrical shock were found to recover in the same manner. 9. Touching the eel, so as to provoke it, with one hand, and at the same time holding the other hand in the water, at a small distance, a shock passed through both arms, as in the case of the Leyden experiment. 10. Dr. W. put the end of a wet stick into the water, and holding it with one hand, he touched the eel with the other; a shock passed through both arms as before. 11. Taking another gentleman in company by the hand, he touched the eel, while Dr. W. held one of his hands in the water; the shock passed through them both. 12. Instead of putting his hand into the water, at a distance from the eel, as in the last experiment, he touched its tail, so as not to offend it, while an assistant touched its head more roughly; they both received a severe shock. 13. Eight or 10 persons, taking hands, stood in a circular form; the first in the series touched the eel, while the last put his hand into the water, at some distance from it; they all received a gentle shock. 14. The above experiment was repeated with no other variation than that the last person touched the eel's tail, while the first touched its head; they all received a severe shock. 15. Another gentleman and Dr. W. holding the extremities of a brass chain, the one put his hand into the water while the other touched the eel, so as to offend it; the shock passed through both. 16. Dr. W. wrapped a silk handkerchief round his hand, and touched the eel with it, but received no shock; though another gentleman felt the shock, who, at the same time, put his hand into the water, at some distance from the eel. 17. A great variety of other experiments were made by 2 persons, one touching the eel near its head, the other putting his hand into the water, or touching it near the tail, forming a communication at the same time between their hands, which were out of the water, by pieces of charcoal, rods of iron or brass, a piece of dry wood, glass, silk, &c. The uniform result

of all these experiments was, that whatever usually conveys the electrical fluid, would also convey the fluid discharged by the eel; and vice versâ, a brass chain, that had very many links in it, would not convey it, unless when the shock was severe, or the chain tense. 18. One of the company being insulated on glass bottles, received several shocks from the eel; but he exhibited no marks of a plus state of electricity, nor would cork balls, suspended by silken threads, give any marks of it, either when they were suspended over the eel's back, or touched by the insulated person at the instant he received the shock. 19. A person, holding a phial in one hand properly lined and coated for electrical experiments, put his hand to the tail of the fish, while an assistant, holding a short wire in one hand, that communicated with the inside of the phial, grasped the fish near its head, so as to receive a severe shock in his hand and arm, but it passed no farther. 20. Two pieces of brass wire, about the thickness of a crow's quill, were screwed in opposite directions, into a frame of wood, so as to come within less than the 100th part of an inch of contact; they were rounded at the point. Dr. W. held the remote end of one of those wires, while an assistant held the other; in the mean while, one of them putting his hand into the water near the eel, the other touched it so as to receive a shock. They repeated this experiment 15 or 20 times with different success: when the points of the wires were even screwed asunder, to the 50th part of an inch, the shock never passed in the circle; but when they were screwed up within the thickness of double-post paper, the shocks, such of them as were severe, would pass through them both; in which case, they doubtless leaped from the point of one wire to the other, though he was not so fortunate as to render the spark generally visible. But it should be observed, that the eel, on which he made these experiments, was not easily provoked, and appeared to be in bad health. He frequently passed his hand along its back and sides from head to tail, and lifted part of its body above the water, without tempting it to make any defence. Dr. Bancroft says, that such eels in Guiana have shocked his hand at the distance of some inches from the surface of the water. Perhaps fire emitted by eels lately taken, might be rendered visible.

From the above experiments it appears: 1. That the Guiana eel has the power of communicating a painful sensation to animals that touch or come near it. 2. That this effect depends entirely on the will of the eel; that it has the power of giving a small shock, a severe one, or none at all, just as circumstances may require. 3. That the shock given, or the painful sensation communicated, depends not on the muscular action of the eel, since it shocks bodies in certain situations at a great distance; and since particular substances only will convey the shock, while others, equally elastic or hard, refuse to convey it. 4. That the shock must therefore depend on some fluid, which the eel discharges from

its body. 5. That as the fluid discharged by the eel affects the same parts of the human body that are affected by the electric fluid; as it excites sensations perfectly similar; as it kills or stuns animals in the same manner; as it is conveyed by the same bodies that convey the electric fluid, and refuses to be conveyed by other bodies that refuse to convey the electric fluid; it must also be the true electrical fluid; and the shock given by this eel, must be the true electrical shock.

While Dr. W. made these experiments, the eel was kept in a large vessel, supported by pieces of dry timber, about 3 feet above the floor. Perhaps it may deserve notice, that a small hole being bored in the vessel in which the eel was swimming, one person provoked the eel so as to receive a shock; another person at the same time, not in contact with him, but holding his finger in the stream that spouted from the vessel, received a shock also in that finger. From this and sundry other experiments, Dr. W. believes that the gymnotus has powers greatly superior to, or rather different from those of the torpedo.

XI. Of the Gymnotus Electricus, or Electrical Eel. In a Letter from Alexander Garden, M. D., F. R. S. Dated Charles-Town, South Carolina, Aug. 14, 1774. p. 102.

There are 5 of these fishes now here, of different sizes, from 2 feet in length to three feet 8 inches. The following description was made out from the longest and largest. It might have been much more accurate, if there had been a possibility of handling the fish, and examining it leisurely; or if there had been a dead specimen, as many things relating to the internal and external structure could in that case have been more exactly ascertained. But this fish has the amazing power of giving so sudden and so violent a shock to any person that touches it, that there seems an absolute impossibility of ever examining accurately a living specimen; and the person who owns them rates them at too high a price, not less than 50 guineas for the smallest, for me to get a dead specimen, unless one should die by accident.

The largest of these fish was, as before said, 3 feet 8 inches in length, when extending itself most, and from 10 to 14 inches in circumference about the thickest part of his body. The head is large, broad, flat, smooth, and impressed here and there with holes, as if perforated with a blunt needle, especially towards the sides, where they are more regularly ranged in a line on each side. The rostrum is obtuse and rounded. The upper and lower jaws are of an equal length, and the gape is large. The nostrils are two on each side; the first large, tubular, and elevated above the surface; and the others small, and level with the skin, placed immediately behind the verge of the rostrum, at the distance of an inch asunder. The eyes are small, flattish, and of a bluish colour, placed

about $\frac{3}{4}$ of an inch behind the nostrils, and more towards the sides of the head. The whole head seems to be well supported; but whether with bones or cartilages, is uncertain. The body is large, thick, and roundish, for a considerable distance from the head, and then gradually grows smaller, but at the same time deeper, or becomes of an acinaciform shape, to the point of the tail, which is rather blunt. There are many light-coloured spots on the back and sides of the body, placed at considerable distances in irregular lines, but more numerous and distinct towards the tail. When the fish was swimming, it measured 6 inches in depth, near the middle, from the upper part of the back to the lower edge of the fin, and it could not be more than 2 inches broad on the back at that place. The whole body, from about 4 inches below the head, seems to be clearly distinguished into 4 different longitudinal parts or divisions. The upper part or back is roundish, of a dark colour, and separated from the other parts on each side by the lateral lines; which, taking their rise at the base of the head, just above the pectoral fins, run down the sides, gradually converging, as the fish becomes smaller, to the tail, and make so visible a depression or furrow in their course, as to distinguish this from the 2d part or division, which may be properly called the body, or at least, appears to be the strong muscular part of the fish. This second division is of a lighter and more clear bluish colour than the upper or back part, and seems to swell out somewhat on each side, from the depression of the lateral lines; but, towards the lower or under part, is again contracted, or sharpened into the 3d part, or carina. This carina, or heel, is very distinguishable from the other two divisions, by its thinness, its apparent laxness, and by the reticulated skin of a more grey and light colour, with which it is covered. When the animal swims gently in pretty deep water, the rhomboidal reticulations of the skin of this carina are very discernible; but when the water is shallow, or the depth of the carina is contracted, these reticulations appear like many irregular longitudinal plicæ. The carina begins about 6 or 7 inches below the base of the head, and gradually widening or deepening as it goes along, reaches down to the tail, where it is thinnest. It seems to be of a strong muscular nature. Where it first takes its rise from the body of the fish, it seems to be about 1 inch or $1\frac{1}{2}$ inches thick, and is gradually sharpened to a thin edge, where the 4th and last part is situated; videlicet, a long, deep, soft, wavy fin, which takes its rise about 3 or 4 inches at most below the head, and runs down along the sharp edge of the carina to the extremity of the tail. Where it first rises it is not deep, but gradually deepens or widens as it approaches to the tail. It is of a very pliable soft consistence, and seems rather longer than the body. The situation of the anus in this fish is very singular, being placed underneath, and being about an inch more forward than the pectoral fins, and consequently considerably nearer the rostrum. It is a pretty long rima

in appearance; but the aperture must be very small, as the formed excrements are only about the size of a quill of a common dunghill fowl. There are two pectoral fins, placed one on each side, just behind the head, over the foramina spiratoria, which are small, and generally covered with a lax skin, situated in the axillæ of these fins. These fins are small for the size of the fish, being scarcely an inch in length, of a very thin, delicate consistence, and orbicular shape. They seem to be chiefly useful in supporting and raising the head of the fish when he wants to breathe, which he does every 4 or 5 minutes, by raising his mouth out of the water. This shows that he has lungs and is amphibious, and the foramina spiratoria seem to indicate his having branchiæ likewise. Dr. G. mentions the appearances of a number of small cross bands, annular divisions, or rather rugæ of the skin of the body. They reach across the body down to the base of the carina on each side; but those that cross the back seem to terminate at the lateral lines, where new rings take their rise, not exactly in the same line, and run down to the carina. This gives the fish somewhat of a worm-like appearance; and indeed it seems to have some of the properties of this tribe, for it has a power of lengthening or shortening its body to a certain degree, for its own conveniency, or agreeable to its own inclination: and besides this power of lengthening or shortening his body, he can swim forwards or backwards with apparently equal ease to himself, which is another property of the vermicular tribe. When he swims forward, the undulation or wavy motion of the fin and carina begin from the upper part, and move downwards; but when he swims backward, and the tail goes foremost, the undulations of the fin begin at the extremity of the tail or fin, and proceed in succession from that backwards to the upper part of the body; in either case he swims equally swift. Every now and then the fish lays himself on one side, as it were, to rest himself, and then the 4 several divisions of his body abovementioned are very distinctly seen; videlicet, the vermiform appearance of the 2 upper divisions; the retiform appearance of the carina; and the last, or dark-coloured fin, whose rays seem to be exceedingly soft and flexible, and entirely at the command of the strong muscular carina. When he is taken out of the water, and laid on his belly, the carina and fin lie to one side, in the same manner as the ventral fin of the tetraodon does, when he creeps on the ground.

The person to whom these animals belong, calls them electrical fish; and indeed the power they have of giving an electrical shock to any person, or to any number of persons who join hands together, the extreme person on each side touching the fish, is their most singular and astonishing property. All the 5 are possessed of this power in a very great degree, and communicate the shock to one person, or to any number of persons, either by the immediate touch of the fish with the hand, or by the mediation of any metalline rod. The keeper says,

that when they were first caught, they could give a much stronger shock by a metalline conductor than they can do at present. The person who is to receive the shock must take the fish with both hands, at some considerable distance asunder, so as to form the communication, otherwise he will not receive it; at least he never saw any one shocked from taking hold of it with one hand only: though some have assured him that they were shocked by laying one hand on him. When it is taken hold of with one hand, and the other hand is put into the water over its body, without touching it, the person receives a smart shock; and the same effect follows, when a number join hands, and the person at one extremity of the circle takes hold of, or touches the fish, and the person at the other extremity puts his hand into the water, over the body of the fish. The shock was communicated through the whole circle, as smartly as if both the extreme persons had touched the fish. In this it seems to differ widely from the torpedo, or else we are much misinformed of the manner in which the benumbing effect of that fish is communicated. The shock which our Surinam fish gives, seems to be wholly electrical; and all the phenomena or properties of it exactly resemble those of the electric aura of our atmosphere when collected, as far as they are discoverable from the several trials made on this fish. This stroke is communicated by the same conductors, and intercepted by the interposition of the same original electrics, or electrics per se, as they used to be called. The keeper of this fish says, that he catched them in Surinam river, a great way up, beyond where the salt water reaches; and that they are a fresh water fish only. He says, that they are eaten, and by some people esteemed a great delicacy. They live on fish, worms, or any animal food, if it is cut small, so that they can swallow it. When small live fishes are thrown into the water, they first give them a shock, which kills or so stupifies them, that they can swallow them easily, and without any trouble. If one of these small fishes, after it is shocked, and to all appearance dead, be taken out of the vessel where the electrical fish is, and put into fresh water, it will soon revive again. If a larger fish than they can swallow be thrown into the water, at a time that they are hungry, they give him some smart shocks, till he is apparently dead, and then they try to swallow or suck him in; but, after several attempts, finding he is too large, they quit him. On the most careful inspection of such fish, Dr. G. could never see any mark of teeth, or the least wound or scratch on them. When the electrical fish are hungry, they are pretty keen after their food; but they are soon satisfied, not being able to contain much at one time. An electrical fish of 3 feet and upwards in length cannot swallow a small fish above 3, or at most $3\frac{1}{2}$ inches long. I have had Mr. Bancroft's Essay on the Natural History of Guiana put into my hands, in which I find an account of this animal; but, as I think that he has not been very particular in the description of it, I

resolved still to send you the above account, that you might judge for yourself. I observe, that his account or description and mine differ in several things; and among others, where he says, that those fish were usually about 3 feet in length; but the one, of which I have sent a slight description, was 3 feet 8 inches. He was told, that some of these fish have been seen in Surinam river, upwards of 20 feet long, whose stroke or shock proved instant death to any person that unluckily received it.

XII. *Experiments and Observations in a Heated Room. By Charles Blagden, M.D., F.R.S.* p. 111.

About the middle of January, several gentlemen, with Dr. B., received an invitation from Dr. George Fordyce, to observe the effects of air heated to a much higher degree than it was formerly thought any living creature could bear. They all rejoiced at the opportunity of being convinced, by their own experience, of the wonderful power with which the animal body is endued, of resisting a heat vastly greater than its own temperature; and their curiosity was not a little excited to observe the circumstances attending this remarkable power. They knew indeed, that of late several convincing arguments had been adduced, and observations made, to show the error of the common opinions on this subject; and that Dr. Fordyce had himself proved the mistake of Dr. Boerhaave* and most other authors, by supporting many times very high degrees of heat, in the course of a long train of important experiments; with which he hoped Dr. F. himself would favour the public. In the mean time, Dr. B. was happy in an opportunity of laying before the R. S. the following short account of some of these experiments, and of the views with which they were undertaken; for the particulars of which he was obliged to Dr. Fordyce himself.

Dr. Cullen long ago suggested many arguments to show, that life itself had a power of generating heat, independent of any common chemical or mechanical means; for, before his time, the received opinions were, that the heat of animals arose either from friction or fermentation.† Governor Ellis, in the year 1758, observed,‡ that a man can live in air of a greater heat than that of his

* Elem. Chemiæ, tom. I, p. 277, 278.—Orig.

† To do further justice to the philosophy of this most ingenious and respectable professor, Dr. B. declares, that during his stay in Edinburgh, from the year 1765 to 1769, the idea of a power in animals of generating cold (that was the expression) when the heat of the atmosphere exceeded the proper temperature of their bodies, was pretty generally received among the students of physic, from Dr. Cullen's arguments; in consequence of which he applied a thermometer, in a hot summer day, to the belly of a frog, and found the quicksilver sink several degrees: a rude experiment indeed, but serving to confirm the general fact, that the living body possesses a power of resisting the communication of heat.—Orig.

‡ Phil. Trans., vol. l. p. 755.—Orig.

body, and that the body in this situation, continues its own cold. The Abbé Chappe d'Auteroche informs us, that the Russians use their baths heated to 60°* of Reaumur's thermometer, about 160 of Fahrenheit's, without taking notice however of the heat of their bodies when bathing. With a view to add further evidence to these extraordinary facts, and to ascertain the real effects of such great degrees of heat on the human body, Dr. Fordyce tried the following experiments.

He procured a suite of rooms, of which the hottest was heated by flues in the floor, and by pouring on it boiling water; and the 2d was heated by the same flues, which passed through its floor to the 3d. The first room was nearly circular, about 10 or 12 feet in diameter and height, and covered with a dome, in the top of which was a small window. The 2d and 3d rooms were square, and both furnished with a sky-light. There was no chimney in these rooms, nor any vent for the air, excepting through crevices at the door. In the first room were placed 3 thermometers: one in the hottest part of it, another in the coolest part, and a 3d on the table, to be used occasionally in the course of the experiment: the frame of this last was made to turn back by a joint, so as to leave the ball and about 2 inches of the stem quite bare, that it might be more conveniently applied for ascertaining the heat of the body, and several other purposes.

Exper. 1. In the first room the highest thermometer stood at 120°, the lowest at 110°; in the 2d room the heat was from 90° to 85°; the 3d room felt moderately warm, while the external air was below the freezing point. About 3 hours after breakfast, Dr. Fordyce having taken off all his clothes, except his shirt, in the 3d room, and being furnished with wooden shoes, or rather sandals tied on with list, entered into the 2d room, and staid 5 minutes in a heat of 90°, when he began to sweat gently. He then entered the 1st room, and stood in the part heated to 110°; in about ½ a minute his shirt became so wet that he was obliged to throw it aside, and then the water poured down in streams over his whole body. Having remained 10 minutes in this heat of 110°, he removed to the part of the room heated to 120°; and after staying there 20 minutes, he found that the thermometer placed under his tongue, and held in his hand, stood just at 100°, and that his urine was of the same temperature. His pulse had gradually risen till it made 145 pulsations in a minute. The external circulation was greatly increased; the veins had become very large, and a universal redness had diffused itself over the body, attended with a strong feeling of heat. His respiration however was but little affected. Here Dr. Fordyce remarks, that the moisture of his skin most probably proceeded chiefly from the condensation of the vapour in the room on his body. He concluded this experiment in the

* Voy. en Siberie, tom. i. p. 51.—Orig.

2d room, by plunging into water heated to 100°; and after having been wiped dry, was carried home in a chair; but the circulation did not subside for 2 hours, after which he walked out in the open air, and scarcely felt the cold.

Exper. 2. In the first room the highest thermometer varied from 132° to 130°; the lowest stood at 119°. Dr. Fordyce having undressed in an adjoining cold chamber, went into the heat of 119°; in ½ a minute the water poured down in streams over his whole body, so as to keep that part of the floor where he stood constantly wet. Having remained here 15 minutes, he went into the heat of 130°; at this time the heat of his body was 100°, and his pulse beat 126 times in a minute. While Dr. Fordyce stood in this situation, he had a Florence flask brought in, filled with water heated to 100°, and a dry cloth, with which he wiped the surface of the flask quite dry; but it immediately became wet again, and streams of water poured down its sides; which continued till the heat of the water within had risen to 122°, when Dr. Fordyce went out of the room, after having remained 15 minutes in a heat of 130°; just before he left the room his pulse made 139 beats in a minute, but the heat under his tongue, in his hand, and of his urine, did not exceed 100°. Here Dr. Fordyce observes, that as there was no evaporation, but constantly a condensation of vapour on his body, no cold was generated but by the animal powers. At the conclusion of this experiment, Dr. Fordyce went into a room where the thermometer stood at 43°, dressed himself there, and immediately went out into the cold air, without feeling the least inconvenience; on which he remarks, that the transition from very great heat to cold is not so hurtful as might be expected, because the external circulation is so excited, as not to be readily overcome by the cold. Dr. Fordyce has since had occasion, in making other experiments, to go frequently into a much greater heat, where the air was dry, and to stay there a much longer time, without being affected nearly so much; for which he assigns 2 reasons; that dry air does not communicate its heat like air saturated with moisture; and that the evaporation from the body, which takes place when the air is dry, assists its living powers in producing cold. It must be immediately perceived that, besides the principal object, these curious experiments throw great light on many other important subjects of natural philosophy.

January 23. The Hon. Captain Phipps, Mr. Banks, Dr. Solander, and Dr. Blagden, attended Dr. Fordyce to the heated chamber, which had served for many of his experiments with dry air. They went in without taking off any of their clothes. It was an oblong-square room, 14 feet by 12 in length and width, and 11 in height, heated by a round stove, or cockle, of cast iron, which stood in the middle, with a tube for the smoke carried from it through one of the side walls. When they first entered the room, about 2 o'clock in the afternoon, the quicksilver in a thermometer, which had been suspended there, stood

above the 150th degree. By placing several thermometers in different parts of the room they afterwards found, that the heat was a little greater in some places than in others; but that the whole difference never exceeded 20°. They continued in the room above 20 minutes, in which time the heat had risen about 12°, chiefly during the first part of their stay. Within an hour afterwards, they went into this room again, without feeling any material difference, though the heat was considerably increased. On entering the room a 3d time, between 5 and 6 o'clock after dinner, they observed the quicksilver in their only remaining thermometer at 198°*: this great heat had so warped the ivory frames of the other thermometers, that every one of them was broken. They now staid in the room, all together, about 10 minutes; but finding that the thermometer sunk very fast, it was agreed, that for the future only one person should go in at a time, and orders were given to raise the fire as much as possible. Soon afterwards Dr. Solander entered the room alone, and saw the thermometer at 210°; but, during 3 minutes that he staid there, it sunk to 196°. Another time, he found it almost 5 minutes before the heat was lessened from 210° to 196°. Mr. Banks closed the whole, by going in when the thermometer stood above 211°; he remained 7 minutes, in which time the quicksilver had sunk to 198°; but cold air had been let into the room, by a person who went in and came out again during Mr. Banks's stay. The air heated to these high degrees felt unpleasantly hot, but was very bearable. Their most uneasy feeling was a sense of scorching on the face and legs; their legs particularly suffered very much, by being exposed more fully than any other part to the body of the stove, heated red hot by the fire within. Their respiration was not at all affected; it became neither quick nor laborious; the only difference was a want of that refreshing sensation which accompanies a full inspiration of cool air. Their time was so taken up with other observations that they did not count their pulses by the watch: Dr. Blagden's, to the best of his judgment by feeling it, beat at the rate of 100 pulsations in a minute, near the end of the first experiment; and Dr. Solander's made 92 pulsations in a minute soon after they had gone out of the heated room. Mr. Banks sweated profusely, but no one else; Dr. Blagden's shirt was only damp at the end of the experiment. But the most striking effects proceeded from their power of preserving their natural temperature. Being now in a situation in which their bodies bore a very different relation to the surrounding atmosphere from that to which they had been accustomed, every moment presented new phenomena. Whenever they breathed on a thermometer, the quicksilver sunk several degrees. Every expiration, particularly if made with any degree of violence, gave a very pleasant

* This thermometer stands, near the boiling point, about 1 degree too high.—Orig.

impression of coolness to their nostrils, scorched just before by the hot air rushing against them when they inspired. In the same manner their now cold breath agreeably cooled their fingers whenever it reached them. On touching his side, Dr. B. says, it felt cold like a corpse; and yet the actual heat of his body, tried under his tongue, and by applying closely the thermometer to his skin, was 98°, about a degree higher than its ordinary temperature. When the heat of the air began to approach the highest degree which this apparatus was capable of producing, their bodies in the room prevented it from rising any higher; and when it had been previously raised above that point, inevitably sunk it. Every experiment furnished proofs of this: toward the end of the first, the thermometer was stationary: in the 2d, it sunk a little during the short time they staid in the room: in the 3d, it sunk so fast as to oblige them to determine that only one person should go in at a time: and Mr. Banks and Dr. Solander each found, that his single body was sufficient to sink the quicksilver very fast, when the room was brought nearly to its maximum of heat.

These experiments therefore prove, in the clearest manner, that the body has a power of destroying heat. To speak justly on this subject, it must be called a power of destroying a certain degree of heat communicated with a certain quickness. Therefore in estimating the heat which we are capable of resisting, it is necessary to take into consideration not only what degree of heat would be communicated to our bodies, if they possessed no resisting power, by the heated body, before the equilibrium of heat was effected; but also what time the heat would take in passing from the heated body into our bodies. In consequence of this compound limitation of our resisting power, we bear very different degrees of heat in different mediums. The same person who felt no inconvenience from air heated to 211°, could not bear quicksilver at 120°, and could just bear rectified spirit of wine at 130°; that is, quicksilver heated to 120° furnished, in a given time, more heat for the living powers to destroy, than spirits heated to 130°, or air to 211°.* And they had, in the heated room where their experiments were made, a striking though familiar instance of the same. All the pieces of metal there, even their watch chains, felt so hot, that they could scarcely bear to touch them for a moment, while the air, from which the metal had derived all its heat, was only unpleasant. The slowness with which air communicates its heat was further shown, in a remarkable manner, by

* These numbers are the result of some experiments which were made on the first of February, in a room where the heat of the air was 65°. Mr. Banks and Dr. Blagden found that they could bear spirits which had been considerably heated and were then cooling, when the thermometer came to the 130th degree; cooling oil at 129°; cooling water at 123°; cooling quicksilver at 117°. And these points were pretty nicely determined; so that though they could bear water very well at 123°, they could not bear it at 125°, an experiment in which Dr. Solander joined them. And their feelings with respect to all these points, seemed pretty exactly the same.—Orig.

the thermometers they brought with them into the room, none of which at the end of 20 minutes, in the first experiment, had acquired the real heat of the air by several degrees. It might be supposed, that by an action so very different from that to which the human body is accustomed, as destroying a large quantity of heat, instead of generating it, they must have been greatly disordered. And indeed they experienced some inconvenience; their hands shook very much, and they felt a considerable degree of languor and debility; Dr. B. had also a noise and giddiness in his head. But it was only a small part of their bodies that exerted the power of destroying heat with such a violent effort as seems necessary at first sight. Their clothes, contrived to guard them from cold, guarded them from the heat on the same principles. Underneath they were surrounded with an atmosphere of air, cooled on one side to 98°, by being in contact with their bodies, and on the other side heated very slowly; because woollen is such a bad conductor of heat. Accordingly Dr. B. found, toward the end of the first experiment, that a thermometer put under his clothes, but not in contact with his skin, sunk down to 110°. On this principle it was that the animals, subjected by M. Tillet to the interesting experiments related in the Memoirs of the Academy of Sciences for the year 1764, bore the oven so much better when they were clothed, than when they were put in bare: the heat actually applied to the greatest part of their bodies was considerably less in the first case than in the last. As animals can destroy only a certain quantity of heat in a given time, so the time they can continue the full exertion of this destroying power seems to be also limited; which may be one reason why we can bear for a certain time, and much longer than can be necessary to fully heat the cuticle, a degree of heat which will at length prove intolerable. Probably both the power of destroying heat, and the time for which it can be exerted, may be increased, like most other faculties of the body, by frequent exercise. It might be partly on this principle that, in M. Tillet's experiments, the girls who had been used to attend the oven bore, for 10 minutes, a heat which would raise Fahrenheit's thermometer to 270°: in the above experiments however, not one of them thought he suffered the greatest degree of heat that he was able to support.

A principal use of all these facts is, to explode the common theories of the generation of heat in animals. No attrition, no fermentation, or whatever else the mechanical and chemical physicians have devised, can explain a power capable of producing or destroying heat, just as the circumstances of the situation require. A power of such a nature, that it can only be referred to the principle of life itself, and probably exercised only in those parts of our bodies in which life seems peculiarly to reside. From these, with which no considerable portion of the animal body is left unprovided, the generated heat may be readily communicated to every particle of inanimate matter that enters into our composition.

This power of generating heat seems to attend life very universally. Not to mention other well known experiments, Mr. Hunter found a carp preserve a coat of fluid water round him, long after all the rest of the water in the vessel had been congealed by a very strong freezing mixture. And as for insects, Dr. Martine* observed, that his thermometer, buried in the midst of a swarm of bees, rose to 97°. It seems extremely probable, that vegetables, together with the many other vital powers which they possess in common with animals, have something of this property of generating heat. Dr. B. doubts if the sudden melting of snow which falls upon grass, while that on the adjoining gravel walk continues so many hours unthawed, can be adequately explained on any other supposition. Moist dead sticks are often found frozen quite hard, when in the same garden the tender growing twigs are not at all affected. And many herbaceous vegetables, of no great size, resist every winter degrees of cold which are found sufficient to freeze large bodies of water. It may be proper to add, that after each of the abovementioned experiments of bearing high degrees of heat, they went out immediately into the open air, without any precaution, and experienced from it no bad effect. The languor and shaking of their hands soon went off, and they did not afterwards suffer the least inconvenience.

XIII. The supposed Effect of Boiling on Water, in disposing it to Freeze more readily, ascertained by Experiments. By Joseph Black, M. D.,† Professor of Chemistry at Edinburgh. p. 124.

"We had lately, says Dr. Black, one day of a calm and clear frost; and I immediately seized the opportunity, which I missed before, to make some

* Essays Medical and Philosophical, p. 331.—Orig.

† This celebrated chemical philosopher was born in 1728, at Bourdeaux, in France, of British parents. He was sent for education first to Belfast, and afterwards to Glasgow, where he studied physic and took the degree of M. D. He was afterwards appointed to read lectures on chemistry and medicine in that university; and in 1766 Dr. Cullen having exchanged the professorship of chemistry for that of the practice of physic in the university of Edinburgh; Dr. Black was appointed to succeed him; and the duties of this office he continued to discharge with increasing reputation for upwards of 30 years. In this situation, says Professor Robison, he soon became one of the principal ornaments of the university of Edinburgh, and his lectures were attended by a crowded audience. It could not be otherwise. His personal appearance and manners were those of a gentleman, and peculiarly pleasing. His voice in lecturing was low but fine; and his articulation so distinct, that he was perfectly well heard by an audience consisting of several hundreds. His discourse was so plain and perspicuous, his illustration by experiment so apposite, that his sentiments on any subject never could be mistaken; and his instructions were so clear of all hypothesis or conjecture, that the hearer rested on his conclusions with a confidence scarcely exceeded in matters of his own experience.

Dr. B.'s constitution was never strong, and for many years preceding his death, he had been subject to a spitting of blood, which he had prevented from proceeding to an alarming length by a very abstemious diet and remarkable serenity of mind. His bodily strength, however, declined very visibly

experiments relative to the freezing of boiled water, in comparison with that of water not boiled. I ordered some water to be boiled in the tea kettle 4 hours. I then filled with it a Florentine flask, and immediately applied snow to the flask, till I cooled it to 48° of Fahrenheit, the temperature of some unboiled water which stood in my study in a bottle; then putting 4 oz. of boiled, and 4 of the unboiled water, separately, into 2 equal tea cups, I exposed them on the outside of a north window, where a thermometer pointed to 29°. The consequence was, that ice appeared first on the boiled water; and this, in several repetitions of the experiment, with the same boiled water, some of which were made 9 hours after it was poured out of the tea kettle. The length of time which intervened between the first appearance of ice on the 2 waters, was different in the different experiments. One cause of this variety was plainly a variation of the temperature of the air, which became colder in the afternoon, and made the thermometer descend gradually to 25°. Another cause was the disturbance of the water; when the unboiled water was disturbed now and then by stirring it gently with a quill tooth-pick, the ice was formed on it as soon, or very nearly as soon, as on the other; and from what I saw, I have reason to think, that were it to be stirred incessantly, provided at the same time the experiment were made with quantities of water, not much larger or deeper than these, it would begin to freeze full as soon. In one of these trials, having inspected my tea cups

during 1798 and 1799; and on the 26th of Nov. of the last-mentioned year he expired suddenly, while at table, with his usual fare, some bread, a few prunes, and a measured quantity of milk diluted with water. He had the cup in his hand when the last stroke of his pulse was to be given, and had set it down on his knees, which were joined together, and had kept it steady with his hand, in the manner of a person perfectly at his ease: and in this attitude he expired, without spilling a drop, and without a writhe on his countenance. His servant thought he had been asleep. This euthanasia happened when he was in his 71st year.

When we take a view of Dr. B.'s experiments on magnesia and quicklime proving that the causticity in burnt lime and alkalies, is owing to their being deprived of fixed air (carbonic acid) with which they are combined in their mild state; and of the experiments which he made on the conversion of water into steam, showing the difference between sensible and latent heat, (in which originated the great improvements made by his pupil Mr. Watt in that admirable and most useful mechanical apparatus, the steam engine) when we take a view of these experiments, from whence as, from a centre, have radiated the brilliant discoveries in pneumatic chemistry of several contemporary philosophers, we shall be fully satisfied that they who have pronounced Dr. B. to have been one of the greatest chemists of the 18th century, have by no means over-rated his scientific character.

Besides his inaugural dissertation De Acido a Cibis Orto, and his Experiments on Quicklime above-mentioned, and the present paper in the Phil. Trans., Dr. B. published an Analysis of the Waters of some Boiling Springs in Iceland, (see the Trans. of the R. S. of Edinburgh). And after his death the world was favoured with the publication of his Lectures on Chemistry, in 2 vols. 4to., 1803, by his intimate friend Mr. Robison, Professor of Natural and Experimental Philosophy in Edinburgh; from whose and Dr. Ferguson's account of the author, the above particulars have been taken.

when they had been an hour exposed, and finding ice on the boiled water, and none on the other, I gently stirred the unboiled water with my tooth-pick, and saw immediately fine feathers of ice formed on its surface, which quickly increased in size and number, till there was as much ice in this cup as in the other, and all of it formed in one minute of time, or 2 at most. And in the rest of the trials, though the congelation began in general later in the unboiled water than in the other; when it did begin in the former, the ice quickly increased so as, in a very short time, to equal, or nearly equal in quantity, that which had been formed more gradually in the boiled water. The opinion, therefore, which I have formed from what I have hitherto seen is, that the boiled and common water differ from one another in this respect; that whereas the common water, when exposed in a state of tranquillity to air that is a few degrees colder than the freezing point, may easily be cooled to the degree of such air, and still continue perfectly fluid, provided it still remain undisturbed: the boiled water, on the contrary, cannot be preserved fluid in these circumstances; but when cooled down to the freezing point, if we attempt to make it in the least colder, a part of it is immediately changed into ice; after which, by the continued action of the cold air on it, more ice is formed in it every moment, till the whole of it be gradually congealed before it can become as cold as the air that surrounds it. From this discovery it is easy to understand, why they find it necessary to boil the water in India, in order to obtain ice. The utmost intensity of the cold which they can obtain by all the means they employ, is probably not greater than 31° or 30° of Fahrenheit's thermometer. Common water, left undisturbed, will easily descend to this degree without freezing; and, if they have not the means of making it colder, may continue fluid for any time, provided it be not disturbed: the refrigerating causes of that part of the world when they have done so much, have done their utmost, and can act no further on the water. But this cannot happen to the boiled water; when the refrigerating causes have cooled it to 32°, the next effect they produce, is to occasion in it the beginning of congelation, while the water is afterwards gradually assuming the form of ice, we know, by experience, that its temperature must remain at 32°; it cannot be made colder, so long as any considerable part of it remains unfrozen.* The refrigerating causes continue therefore to have power over it, and to act upon it, and will gradually change the whole into ice, if their action be continued sufficiently long.

The next object of investigation may be the cause of this difference between the boiled and the common water. In considering this point, the following idea

* Common water, when cooled in a state of tranquillity to several degrees below the freezing point, will suddenly rise up to it again, if disturbed in such a manner as to occasion in it a beginning of congelation.—Orig.

was suggested. As we know from experience, that by disturbing common water, we hasten the beginning of its congelation, or render it incapable of being cooled below 32°, without being congealed; may not the only difference between it and boiling water, when they are exposed together to a calm frosty air, consist in this circumstance; that the boiled water is necessarily subjected to the action of a disturbing cause, during the whole time of its exposure, which the other is not? One effect of boiling water long, is to expel the air which it naturally contains; as soon as it cools, it begins to attract and absorb air again, till it has recovered its former quantity; but this probably requires a considerable time. During the whole of this time, the air entering into it must occasion an agitation or disturbance in the water, which, though not sensible to the eye, may be very effectual in preventing it to become, in the least, colder than the freezing point, without beginning to freeze, in consequence of which, its congelation must begin immediately after it is cooled to that point. When I reflect on this idea, I remember a fact which appears to me to support it strongly. Fahrenheit was the first person who discovered that water, when preserved in tranquillity, may be cooled some degrees below the freezing point without freezing. He made the discovery while he was endeavouring to obtain ice from water that had been purged of its air: with this intention he had put some water into little glass globes, and having purged it of air, by boiling and the air-pump, he suddenly sealed up the globes, and then exposed them to the frosty air. He was surprized to find the water remain unfrozen much longer than he expected, when at last he opened some of his globes, in order to apply a thermometer to the water, or otherwise examine what state it was in. The immediate consequence of the admission of the air was a sudden congelation, which happened in the water; and in the rest of his globes, a similar production of ice was occasioned by shaking them. The inference that may be drawn from these experiments of Fahrenheit's, is sufficiently obvious; it appears to remove all doubt with regard to the above supposition. Before these experiments of Fahrenheit occurred to my memory, I had planned a few, suggested by the above supposition, that might have led to the same conclusion; but the short duration of the frost, for one day only, did not give me time to put them in execution.

XIV. Experiments on the Dipping-Needle, made by Desire of the R. S. By Thomas Hutchins. p. 129.

In these experiments the instrument was placed in 4 several positions, viz. with the index placed east, and then placed west, with the poles of the needle placed one way, and then the same with the poles changed or reversed. In each,

of these 4 positions, the dip was taken and noted down 3, 4, or 5 times. And the mediums of all these, for the several places, are as follow.

1. At Stromness in the isles of Orkney, lat. 58° 59′ N., long. 3° 30′ W. from London, June 9, 1774. The mean dip was 75° 51′.

In these observations the needle was placed horizontal, and the vibration continued between 9 and 10 minutes. The instrument was set in the middle of a room up one pair of stairs; but being apprehensive that the iron grate, fender, poker, and tongs, might, in some measure, affect the needle, trial was made in the open air, and in a place free from such obstacles.

2. On the Holms in the entrance of Stromness Harbour, June 23, 1774. Variation per azimuth 24° westerly. Long. from London 3° 30′ W. lat. 58° 59′ N. Dip. 75° 40′.

The needle in all these observations was left to vibrate from an horizontal position. The instrument was set on the top of the case in which it was packed, and stood in the open air, in a fine sunny day.

3. In Hudson's Straits, July 23, 1774, lat 62° 3′ N., long. 69° W. from London, variation 43° westerly. Mean dip, 82° 42′.

The needle vibrated from an horizontal situation. These observations were made on a large piece of ice, to which the 3 ships were grappled.

4. In Hudson's Straits, July 27, 1774, lat 62° 23′ N., long. 71° 30′ W. from London, variation 42° 50′ westerly per azimuth. Dip 83° 11′.

5. In Hudson's Straits, July 28, 1774, lat. 62° 25 N, long. 71° 30′ W. from London, variation per azimuth 44° W. Dip 82° 46′.

6. In Hudson's Bay, August 14, 1774, lat. 56° 53′ N., long. 85° 22′ W. from London, variation per azimuth 24° W. Dip. 82° 41′.

These experiments were made in the cabin of the Prince Rupert, while she lay among ice. The ship frequently varied the position of her head a point of the compass; but by replacing the instrument as often as was found necessary, there was the greatest reason to think these observations, which took up above 3 hours, are pretty accurate.

7. At Moose Fort in Hudson's Bay, September 8, 1774, lat. 51° 20′ N., long. 82° 30′ W. from London, variation 17° W. Dip 80° 13′.

The observations were made on shore. So remarkable a difference between them, when Mr. H. was expecting quite the reverse, surprized him as much as the increased inclination of the needle from observations made nearly in the same parallel of latitude in London. He endeavoured, by drawing a magnetical meridional line with chalk, and paying the greatest attention to keeping the instrument perfectly steady and horizontal, to render these experiments accurate, and fulfil the intention of the R. S.

8. At Albany Fort, in Hudson's Bay, Sept. 14, 1774, long. 82° 30′ w., lat. 52° 22′ N., variation 17° w. Dip 80° 2′.

Observations on Hoy 1774.

Month.	Hour.	Barometer.	Thermometer.	Weather.	Circumstances.
June 11, 1774.	0 15	28.63	59	Clear.	On the top of the hill.
	0 30	28.60	$56\frac{1}{2}$	Foggy.	Ditto.
	4 15	30.22	63	Clear.	At low water mark.

Hoy is a remarkable high hill near Stromness, in the Orkneys, and is placed by Mr. Mackenzie in lat. 58° 58′ N, and long. 3° 30′ w. from London. The first 2 observations were made on the highest part of the hill. Soon after the first, a fog was seen below arising from the water, at length it reached the summit of the hill; the air seemed very raw and cold to the touch, and the instruments showed as in the 2d observation. The barometer continued at 28.60 inches after the fog was gone off, but the thermometer rose 2 or 3 degrees. The last observation was made at low water mark, about half a mile from the bottom of the hill. THOMAS HUTCHINS.

"The height of Hoy above low water mark according to these observations should be 249.93 fathoms, or as near as may be 500 yards, neglecting the correction for the difference that may be supposed in the temperature of the quicksilver at the two stations, the quantity of which is uncertain." S. HORSLEY.

XV. A Meteorological Journal for the Year 1774, kept at the Royal Society's House by Order of the President and Council. p. 139.

The observations of the barometer and thermometers were made two times on every day of the year, viz. at 8 o'clock in the morning, and about 2 afternoon. And the numbers collected for the several months were as in the following table.

1774	Thermometer without.					Thermometer within.			Barometer.			Rain.
	Greatest height.	Least height.	Mean height. A. M.	Mean height. P. M.	Mean whole day.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Inches.
January	50.5	24.0	30.0	37.7	35.3	50.5	27.0	37.4	30.17	28.79	29.57	2.958
February	52.0	24.5	37.6	43.5	40.5	50.0	33.0	42.4	30.46	29.16	29.81	2.360
March	60.0	33.5	39.6	50.5	45.0	61.5	38.0	47.4	30.33	29.14	29.82	1.780
April	67.0	36.5	44.9	54.8	49.8	60.0	45.0	51.3	30.24	29.33	29.79	1.242
May	69.0	45.0	49.7	59.9	54.8	51.5	51.0	55.9	30.18	29.34	29.87	1.413
June	77.5	52.0	59.1	68.4	63.7	71.0	59.0	64.6	30.34	29.47	29.90	2.237
July	83.5	55.5	59.7	70.1	64.9	73.5	60.0	65.9	30.36	29.61	30.00	2.438
August	78.0	73.0	58.2	69.2	63.7	73.0	52.5	54.6	30.32	29.38	29.95	3.340
September	73.0	42.5	52.6	62.1	57.3	69.5	49.5	59.7	30.28	29.11	29.79	3.743
October	64.5	36.0	46.0	56 3	51.1	61.0	45.5	53.6	30.57	29.40	30.13	1.348
November	58.5	41.0	39.2	43.5	41.3	56 0	34.5	43.7	30.22	29.17	29.81	1.627
December	53.5	25.0	37.3	40.8	39.7	49.0	30.5	40.8	30.71	29.11	30.09	1.826
Means of all					50.6			52.4			29.88	26.312

The quantity of rain in the whole year was 26.312, or about $26\frac{1}{3}$ inches.

For the Variation of the Magnetic Needle.—These observations were 4 times every day, from August 21 to Sept. 5, viz. in the morning, at noon, at 2 afternoon, and in the evening; the means of all which are respectively as below.

Morn. 21° 25′.... noon 21° 33′.... 2 p.m. 21° 28′.... even. 21° 17′.

The mean of all.................. 21° 26′
Error of instrument .... — 10
Correct variation.................. 21 16 west.

XVI. An Abridged State of the Weather at London in the Year 1774, collected from the Meteorological Journal of the R. S. By S. Horsley, LL.D., Sec. R. S. p. 167.

Though the practice of keeping meteorological journals is, of late years, become very general, no information of any importance has yet been derived from it. The reason of which perhaps may be, that after great pains and attention bestowed in registering particulars, as they occur, with a scrupulous minuteness, observers have not taken the trouble to form, at proper intervals of time, compendious abstracts of their records, exhibiting the general result of their observations in each distinct branch of meteorology. The following tables are given as an example of the method that may be taken in future to remedy this neglect. With the general state of the barometer and thermometer, already given at the end of the meteorological journal, they form a history of the weather at London during the last year. If the example were to be followed, in different parts of the kingdom, we might in time be furnished with an experimental history of the weather of our island.

TABLE I.

An abridged View of the Winds at London, in 1774.
Compiled from the Meteorological Journal of the Royal Society.

	N.	S.	E.	W.	N.W.	S.E.	N.E.	S.W.	Days	Rain.	
January	1½	0	2	1½	4	2½	4	13	31	2.958	Five half days omitted in the Journal.
February	1	1½	1	3	2	0	3½	16	28	2.360	
March	2	1½	1	1	½	3	14	7½	31	1.780	Half a day missed in the Journal.
April	2½	3	2	4½	2½	2	5	8½	30	1.242	
May	3	½	3½	0	6	3	10½	4	31	1.413	A half day missed in the Journal.
June	½	3½	2½	2½	4	2	1½	13½	30	2.273	
July	½	2	0	1	6½	2	1	18	31	2.438	
August	2	1½	0	1½	2	4	6	14	31	3.340	
September	1½	2½	1	2½	4	3½	6	9	30	3.743	
October	1	2½	1	2	3½	3	8	10	31	1.348	
November	7	1	1½	0	2½	1½	7	9½	30	1.627	
December	2½	1½	2	4½	6	3½	7½	3½	31	1.826	
	25	21	17½	24	43½	30	74	126½		26.312	

This table shows the number of days that each wind blew in each month, dividing the compass only into 8 points, and reckoning all the winds between N. and W., N. W.; all between S. and E., S. E.; all between N. and E., N. E.; and all

between S. and W. S. W. The number of days that each blew in all the months being collected into one sum at bottom, shows the number of days each wind blew in the whole year. The quantity of rain that fell in each month is added, that the connection between wet and dry, and the several winds may the more readily appear. It appears that the winds from the S. W. prevailed more than any other in the year 1774; and next to the S. W. the N. E. But the S. W. was more frequent than the N. E. in the proportion of 126 to 74. Of the winds from the 4 cardinal points, the N. was the most frequent, and the E. the most rare. In the 3 summer months, June, July, and August, there fell more rain than in the 3 of any other season. Of the 26.312 inches of rain which fell in the whole year, 13.826 fell in the winter half year, consisting of the 6 months of September, October, November, December, January, and February, and 12.486 in the summer half year, consisting of the 6 months of March, April, May, June, July, and August. So that the winter's rain exceeded the summer's by 1.340 inches; that is, by little more than $\frac{1}{10}$ part of half the rain of the whole year. September gave the greatest quantity of rain, and April the least of any single month in the whole year.

TABLE II.

Sub-division of the S. W.

	W.S.W.	S.W.	S.S.W.	Sums.
January	2	9½	1½	13
February	4	7½	4½	16
March	1½	4½	1½	7½
April	3	3	2½	8½
May	½	2	1½	4
June	1	9	3½	13½
July	5	9	4	18
August	5½	4½	4	14
September	2	1½	5½	9
October	2	5	3	10
November	2½	5	2	9½
December	1	2	½	3½
Sums	30	62½	34	126½

TABLE III.

Sub-division of the N. E.

	E.N.E.	N.E.	N.N.E.	Sums.
January	2½	½	1	4
February	0	2½	1	3½
March	2½	9	2½	14
April	½	3½	1	5
May	2	8½	0	10½
June	0	½	1	1½
July	0	½	½	1
August	2	1	3	6
September	1	1	4	6
October	2½	3½	2	8
November	4	1½	1½	7
December	1½	3½	2½	7½
Sums	18½	35½	20	74

In these 2 tables the winds between the W. and the S. W. are all set down to the W. S. W.; and those between the S. and the S. W. are all reckoned S. S. W. In like manner, the winds between the E. and N. E. are all reckoned E. N. E.; and those between the N. and N. E. are all reckoned N. N. E. It appears that of the winds between the S. and W. those from the point of S. W. were far more frequent than those from either side of it. And the winds from the point of N. E. more frequent than those on either side of it, nearly in the same proportion.

TABLE IV.

Sub-division of the S. E.

	E. S. E.	S. E.	S. S. E.	Sums.
January	1½	1	0	2½
February	0	0	0	0
March	½	1½	1	3
April	0	1	1	2
May	0	0	3	3
June	0	1½	½	2
July	½	0	1½	2
August	1	2½	½	4
September	0	1½	2	3½
October	½	1½	1	3
November	0	1	½	1½
December	0	2½	1	3½
Sums	4	14	12	30

TABLE V.

Sub-division of the N. W.

	W. N. W.	N. W.	N. N. W.	Sums.
January	½	2½	1	4
February	1	1	0	2
March	0	½	0	½
April	½	1½	½	2½
May	½	3½	2	6
June	1½	1½	1	4
July	2	4½	0	6½
August	0	1½	½	2
September	2	1	1	4
October	0	3½	0	3½
November	1	½	1	2½
December	2½	3½	0	6
Sums	11½	25	7	43½

By these 2 tables it appears that, of all the winds between the N. and W., those from the point of N. W. were far more frequent than those from either side of it. Of the winds between the S. and E., those from the point of S. E. were more frequent than those to the E. of that point, and rather more frequent than those to the S. of it; but the difference in the latter case was very inconsiderable. Of the winds from all quarters, those from the E. S. E. and N. N. W. were the most rare, especially the former. The numbers in the last columns of each of the last 4 tables, are the sums of the preceding columns ranging in the same horizontal lines. They ought to correspond with the numbers in columns S. W. N. E. S. E. N. W. of table 1, respectively, and serve as a check on the work in making the tables.

The general state of the winds collected from the preceding 5 tables, according to their different degrees of prevalence, is as follows:

E. S. E.	N. N. W.	W. N. W.	S. S. E.	S. E.	E.	E. N. E.	N. N. E.	S.	W.	N. W.	N.	W. S. W.	S. S. W.	N. E.	S. W.	Sum.
4	7	11½	12	14	17½	18½	20	21	24	25	25	30	34	35½	62½	361½

Days missed in the Journal.. 3½

365

TABLE VI.

Showing the number of fair and frosty days in each half month and in the whole year.

	Fair		Fair days in whole month	Frosty days		Frosty da. in whole month
	1st half	Latt. half		1st half	Latt. half	
January	9	6	15	10	7	17
February	7	3	10	6		6
March	7	13	20	4		4
April	11	8	19			
May	8	6	14			
June	10	6	16			
July	6	8	14			
August	9	8	17			
September	7	2	9			
October	12	10	22			
November	5	6	11		1	1
December	6	13	19	5	3	8
Total fair days.............			186	Total frost....		36

There were but 10 days in the whole year that gave any snow, viz. 3 in January, 1 in February, 5 in November, and 1 in December. The first snow on the 9th of January in the afternoon, after a rainy morning, set in with a N. N. E. wind, and was succeeded by a sharp frost for 3½ days, with the wind E. N. E. The second, which happened in the night between the 17th and 18th,

came likewise after rain, and was succeeded by a frost of 4½ days, wind shifting between N. W. and S. E. The last snow in January, on the 24th, fell with a S. W. wind, which set in the day before. It was followed by a moderate frost of one day, though the wind continued in the S. W. The snow on the 1st of February came with a S. W. during a sharp frost. The wind was in the N. E. before the snow, and returned to the same point the next morning; the frost sharper than before the snow. The snows in the latter part of November were generally accompanied with rain, and did not bring actual frost. The snow on the 9th of December came after 2 days frost, which it seems to have put an end to. For though it froze in the evening after the snow, the frost was much less severe than the preceding night, and a thaw came with rain, wind N. E. the next day.

There were only 2 thunder storms this year, viz. August 27, 2 p. m. barometer 29.64 inches, thermometer 63°, wind N. W. September 24, 9 p. m. barometer 29.42 inches, thermometer at 2 p. m. 64°.

TABLE VII.

For trial of the influence of the winds on the barometer.

	S. W.	W. S. W.	S. S. W.	N. E.	E. N. E.	N. N. E.	S. E.	E. S. E.	S. S. E.	N. W.	W. N. W.	N. N. W.	N.	S.	E.	W.
an.	29.37	29.61	29.60	29.34	29.64	29.58	29.57	29.72		29.66	29.895	29.675	29.72		29.80	29.67
eb.	29.63	29.81	29.70	30.30		30.41				29.73	29.615		29.69	29.86	29.35	29.90
lar.	29.39	29.79	29.89	29.945	29.87	30.20	29.52	29.83	29.52	29.615			30.14	29.87	29.90	29.75
pril	29.77	30.07	29.61	29.95	30.21	29.80	30.00		29.69	29.80	29.58	29.90	29.82	29.475	29.53	29.80
lay	29.91	29.46	29.45	29.96	29.86				29.71	29.81	29.97	30.04	29.98	29.34	29.925	
une	29.91	29.92	29.80	30.21		29.75	29.92		29.95	30.00	29.69	29.87	29.60	29.83	29.84	30.09
uly	29.97	29.97	29.98	29.92		30.33		29.78	29.97	30.07	30.085		30.26	30.25		29.94
ug.	29.97	29.85	29.80	29.95	30.00	30.17	30.05	30.07	29.535	29.950		30.12	30.13	29.74		29.98
ept.	29.89	29.80	29.70	29.97	29.82	29.94	29.51		29.56	29.925	29.82	29.94	29.85	29.64	29.775	29.76
ct.	29.98	30.28	30.225	30.32	29.865	30.23	30.15	29.54	30.18	29.98			30.19	30.22	29.95	30.27
ov.	29.74	29.91	29.92	29.79	29.73	29.53	29.84		29.86	29.81	29.74	29.90	29.93	29.61	29.54	
ec.	30.07	29.795	29.71	30.15	29.82	30.38	29.62		29.20	30.21	30.31		30.38	29.64	29.90	30.51
leans	29.76	29.92	29.80	30.02	29.82	30.01	29.80	29.81	29.70	29.94	29.93	29.92	29.99	29.80	29.80	30.02

It is an old observation, that a N. E. wind in this country generally makes the barometer rise. This naturally leads to an inquiry, whether there be any general connection of the rise and fall of the barometer with the setting of the wind. On comparing the general account of the barometer for the year 1774, as stated at the end of the meteorological journal, with the journal at large, I found that, in 7 months out of the 12, the greatest height of the barometer was accompanied with a north-easterly wind; and in 8 months out of the 12, the least height of the barometer was accompanied with a S. W. This incited me to take the trouble of making out the preceding table, which shows the mean height of the barometer which accompanied each wind in every month, and for the whole year. And it appears, that though the barometer may be almost at any height with any

wind, yet the mean height was greater, in the course of the last year, with the winds which set from that semicircle of the compass, which is intercepted between the points of W. S. W. inclusive, and E. N. E. exclusive, going round by the W. and N. than with the winds which set from the opposite semicircle intercepted between the E. N. E. inclusive, and W. S. W. exclusive, going round by E. and S. In the former semicircle the W. and N. E. give the greatest mean height, and in the latter the S. S. E. and S. W. give the least.*

TABLE VIII.

For trial of the moon's influence.

	Last qr.		New.		First qr.		Full.		
	d.	h.	d.	h.	d.	h.	d.	h	
January	5	6	11	21	19	3	27	7	6 7 9 10 14 18 23 25 26 30
February	3	15	10	9	18	0	25	23	4 7 8 10 13 15 17 20
March	4	22	11	22	19	20	27	11	10
April	3	5	10	12	18	15	25	22	4 8 28 29
May	2	12	10	3	18	7	25	5	5 7 23
							Last qr.		
							31	20	
	New.		First qr.		Full.		Last qr.		
June	8	18	16	19	23	12	30	7	6 17 18 20 22 28 30
July	8	9	16	5	22	19	29	20	5 15 20 22 27 31
August	7	0	14	12	21	3	28	12	4 6 11 15 17 26
September	5	14	12	17	19	13	27	7	1 5 7 11 13 14 17 20 22
October	5	3	12	0	19	2	27	3	3 23 24 29
November	3	15	10	7	17	18	25	23	2 6 7 18 21 26
December	3	2	9	17	17	12	25	17	2 5 11 14 15

* It is to be noted, that the means of the whole year, stated in the lowermost horizontal row, are not found by collecting the means of all the months into one sum, and dividing by the number of months (for this method would always be fallacious, except each wind had blown for the same number of days in all the different months); but by adding together the heights attending each wind day by day, and dividing the sum by the number of days each wind blew in the whole year.—Orig.

This table exhibits a comparison of the actual changes of the weather from fair to foul, with the aspects of the moon; and needs no other explanation than an interpretation of the characters in the last column.

— frost	Any one of these marks placed over a number signifies, that the weather indicated by that mark continued from the day of the month denoted by the number underneath to the day denoted by the next following number, bearing some other mark over it.
+ thaw	
⊙ fair	
〰 rainy	
≈ stormy	
✻ snow	

Thus, in the month of July, rainy weather set in on the 5th, and lasted till the 15th; from the 15th to the 20th it was fine; when it changed again, and continued rainy till the 22d; then it was fine to the 27th, and rainy again till the 31st.

Such tables of comparison, made yearly for a succession of years, would in the end decide with certainty for or against the popular persuasion of the moon's influence on the changes of our weather; which has some how or other gained credit even among the learned, without that strict empiric examination, which a notion in itself so improbable, so destitute of all foundation in physical theory, so little supported by any plausible analogy, ought to undergo. The vulgar doctrine about this influence is, that it is exerted at the syzygies and quadratures, and for 3 days before and after each of those epochs. There are 24 days therefore in each synodic month, over which the moon at this rate is supposed to preside; and as the whole consists but of 29 days $12\frac{3}{4}$ hours, only $5\frac{1}{2}$ days are exempt from her pretended dominion. Hence, though the changes of the weather should happen to have no connection whatever with the moon's aspects, though the fact should be, that they take place at all times of the moon indifferently, and are distributed in an equal proportion through the whole synodic month, yet any one who shall predict, that a change shall happen on some one of the 24 days assigned, rather than on any one of the remaining $5\frac{1}{2}$, will always have the chances 24 to $5\frac{1}{2}$ in his favour. Merely because more changes will fall within the greater time, and, on an average, as many more in proportion as the time is greater. It is evident therefore, that this is a matter in which men may easily deceive themselves, especially in so unsettled a climate as that of this island; and the advocates for lunar influence are not to imagine they have fact on their side, unless it should appear, from such tables as these carefully kept for a long course of years, that the changes happening on the days, which they hold to be subject to the moon, are more than those which happen on the exempted days, in a much greater proportion than that of 24 to $5\frac{1}{2}$.

The antiquity of the opinion may perhaps be alleged in its favour; and it may seem an answer to the objection taken from the instability of the weather of this part of the world, that it had its origin in more settled climates. We find it, it must be confessed, in the earliest Greek writers, who probably had it with

the rest of their physics from the East. And to this circumstance, I am persuaded, the opinion owes the credit it has met with among men of learning. But whatever general assertions may be found in some writers, concerning celestial influences in general, and the moon's in particular, as being of all the heavenly bodies the nearest to the earth, the writers who treat of the signs of the weather practically, for the information of husbandmen and mariners, derive their prognostics from circumstances, which neither argue any real influence of the moon as a cause, nor any belief of such an influence; but are merely indications of the state of the air at the time of observation: namely, the shape of the horns, the degree and colour of the light, and the number and quality of the luminous circles which sometimes surround the moon, and the circumstances attending their disappearance.* It is true, that each of these prognostics is expressly confined, by the early writers, to a particular time of the moon's age.† But not, as I conceive, on account of any particular influence of the moon in this or that aspect; but merely because the prognostics, that she affords at one age, are such in themselves as she cannot afford at another. For instance, the bluntness of the horns in the new moon is a sign of approaching rain, because it indicates a turbid state of the atmosphere; for if the air were clear and dry, the horns should appear sharp and pointed, that being then their natural shape. But the bluntness of the horns is no sign of change after the dichotomy; because then the horns will appear blunt in all states of the air, the elliptic arc on the deficient side of the moon presenting its concavity to the circular limb, and forming with it an obtuse angle. Again, the degree of the moon's light on the 4th day furnished a prognostic. It ought then to be strong enough, if the air was clear, for terrestrial objects to cast a shadow.‡ If their shadows were not discernible, it was a sign that the air was impure, and bad weather was to be expected. But this prognostic did not take place before the 4th day, because the light of the moon was yet too weak for shadows to be formed in the purest state of the air. It did not take place after the 4th day, because the enlightened part was so much increased, that shadows would be

* See the Διοσημεια of Aratus and the Scholia of Theon.—Orig.

† Σήματα δ' ὄτ' ἄρ πᾶσιν ἐπ' ἤμασι πάντα τέτυκται.
Ἀλλ' ὅσα μὲν τριτάτῃ τετραταίῃ τε πέληται,
Μέσφα διχαιομένης· διχάδος γέ μεν ἄχρις ἐπ' αὐτὴν
Σημαίνει διχόμηνον· ἀτὰρ πάλιν ἐκ διχομήν
Ἐς διχάδα φθιμένην· ἔχεται δέ οἱ αὐτίκα τετρὰς
Μηνὸς ἀποιχομένου. Αρατ. Διοσημεια.—Orig.

‡ ——ὅτε πρώτη ἀποκίδναται αὐτόθεν αὐγὴ
Ὅσσον ἐπισκιάειν ἐπὶ τέτρατον ἦμαρ ἴουσα. Αρατ. Διοσημεια.
Τεταρταία γενομένη ἡ σελήνη ἄρχεται δύνασθαι σκιάζειν ἐν τῷ φωτὶ αὐτῆς· τριταία γὰρ οὐ δύναται διὰ τὴν περικειμένην τοῦ φωτὸς ἀδράνειαν Theon in locum.—Orig.

formed in any state of the air, if the moon was not actually hidden by a cloud, or obscured by sensible mists. The prognostics furnished by the new moon served only till the dichotomy, and those of the dichotomy till the full moon, and so on; not because a new and distinct influence was exerted in each new aspect, but because each new aspect furnished a new set of signs, of a different kind. That this is a true representation of the most ancient lunar prognostics, appears from hence; that others of a similar kind were derived from the sun and the fixed stars, particularly the Præsepe and Aselli in Cancer, and the bright star in the Altar; and it is remarkable, that Aratus says, the prognostics taken from the sun are the most certain of all.* The vulgar soon began to consider those things as causes, which had been proposed to them only as signs. The manifest effect of the moon on the ocean, while the mechanical cause of it was totally unknown, was interpreted as an argument of her influence over all terrestrial things; and these notions were so consistent with that visionary philosophy, which assigned distinct places to corruption, change, and passivity, on the one side, and the active governing powers of nature on the other, and made the orb of the moon the boundary between the two, that they who should have been its opponents, ranged themselves on the side of popular prejudice. And the uncertain conclusions of an ill-conducted analogy, and a false metaphysic, were mixed with the few simple precepts derived from observation, which probably made the whole of the science of prognostication in its earliest and purest state. Hence both Theophrastus and Aratus teach us to remark the position of the moon's horns, and take conjectures of approaching fair weather or tempest, according as they appear, at different times of the moon's age, erect, reclined, or prone: not knowing that the position of the line joining the moon's cusps, with respect to the horizon, depends merely on the mutual approach, or recess, of the pole of a great circle drawn through the centres of the sun and moon, and the pole of the horizon, in the course of the diurnal revolution. And so great a man as Varro, as he is quoted by Pliny, was not ashamed to give this childish rule, for predicting the weather, for a whole month to come, from appearances at the new moon. "If the upper horn be obscure, the decline of the moon will bring rain. If the lower horn, the rain will happen before the full. At the time of the full moon, if the blackness be in the middle."† After this one cannot be surprized, that the poet Virgil should make the prognostics of the 4th day decisive for the whole lunation:

* Ηελίῳ καὶ μᾶλλον ἐοικότα σήματα κεῖται. Διοσημεια.—Orig.

† Apud Varronem ita est.———Nascens Luna si cornu superiore obatro surget, pluvias decrescens dabit: si inferiore, ante plenilunium: si in mediâ nigritia illa fuerit, imbrem in plenilunio. Plin.Nat. Hist. lib. xviii. cap. 35.—Orig.

Sin ortu quarto, namque is certissimus auctor,
Pura neque obtusis per cœlum cornibus ibit,
Totus et ille dies, et qui nascentur ab illo,
Exactum ad mensem pluviâ ventisque carebunt.

Georgic. lib. 1, lin. 143.

But in this he contradicts Aratus, whose authority in general he follows implicitly. With Aratus, the signs of the new moon extend only to the first quarter.

The ancients ascribed an influence to the constellations and fixed stars as well as to the sun and moon; and there seems to have been much the same foundation for one as the other. In the parapegmata or calendars, introduced in Greece, as we learn from Theon,* by the astronomer Meton, and renewed either annually, or as I rather conjecture, at the expiration of every 19 year period, the heliacal risings and settings of different stars were marked as bringing in different sorts of weather. The truth is, the earliest astronomers imagined, that the weather was governed by the sun, and that its varieties were every where owing to the different degrees of the sun's heat in the different seasons. They had therefore taken great pains to collect, by a long series of observations, the weather that usually prevailed in this or that particular place during the sun's passage through every degree of every sign. Upon these observations, not upon any whimsical theory of celestial influences, the predictions in the calendars were founded. It seemed reasonable to announce, as the weather of each part of the year, what had been found to be then most frequent. And while the civil reckonings of time were so different among the different Greek states, and so rudely digested in all, the heliacal risings and settings of the stars were the only certain and obvious marks, the compilers of those popular directories could hit upon, of the sun's return to the different parts of the zodiac.† Hence they proposed them to people as signals of the weather to be expected. The form of the year being now the same in all parts of Europe, and pretty accurately adjusted to the motions of the heavenly bodies, and the heliacal risings and settings of the stars, from the different manner of life of our country people, not falling so much under popular observation with us, as they did among the Greeks, they are not marked as prognostics in our modern almanacks: and this I take to be the reason, that though the moon hath maintained her reputation among us, the influence of the fixed stars is sunk, as it well deserves, in utter oblivion. On the whole I do not deny, that the observant husbandman will find a variety of useful prognostics in the appearances of the moon, and the heavenly bodies in general; but they will be prognostics of no other kind, and for no other

* Scholia in Aratum.—Orig.

† Geminus. Εἰσαγωγὴ εἰς τὰ φαινόμενα. c. 14.—Orig.

reason (though perhaps less fallible) than the sputtering of the oil in the industrious maiden's lamp, or the excrescences which gather round the wick.* They will be symptoms destitute of all efficient powers. They will show the present state of the air, as that on which they depend, not as that which they govern, and may furnish probable conjectures for 2 or 3 days to come. To what I have already advanced in support of this opinion, I shall only add the last lines of the Διοσημεια of Aratus. They speak the sentiments of the earliest ages most decisively, as they show how little the doctrine of the influence of lunar aspect had gained ground, even in his days, among practical writers. That elegant versifier, there is little room to doubt, delivers the practical maxims of his time, just as he received them. He was too little of a poet to disguise the truth with ornamental fiction, and too little of a philosopher to adulterate it with hypothesis.

Τῶν μηδὲν κατόκνησο, καλὸν δ' ἐπὶ σήματι σῆμα
Σκέπτεσθαι, μᾶλλον δὲ δυοῖν εἰς ταυτὸν ἰόντοιν
Ἐλπωρὴ τελέθοι· τριτάτῳ δέ κε θαρσήσειας.
Αἰεὶ δ' ἂν περιόντος ἀριθμοίης ἐνιαυτῦ
Σήματα, συμβάλλων εἴπε καὶ ἐπ' ἀστέρι τοίη
Ἠώς ἀντέλλοντι κατέρχεται, ἢ κατιόντι,
Ὁπποῖον καὶ σῆμα λέγοι· μάλα δ' ἄρκιον εἴη
Φράζεσθαι φθίνοντος, ἐφισταμένοιό τε μηνὸς
Τετράδας ἀμφοτέρας. αἱ γάρ τ' ἄμυδις συνιόντων
Μηνῶν πείρατ' ἔχουσιν, ὅτε σφαλερώτατος αἰθὴρ
Ὀκτὼ νυξὶ πέλει, χήτει χαροποῖο σελήνης.
Τῶν ἄμυδις πάντων ἐσκεμμένος εἰς ἐνιαυτὸν,
Οὐδέποτε σχεδίως κεν ἐπ' αἰθέρι τεκμήραιο.

Which I render thus: " Neglect none of these prognostics [none, he means, of the great variety he hath enumerated, taken from the heavens, from animals, plants, terrestrial objects, &c.], it is a good thing to combine the observation of one prognostic with another. If 2 agree, there is the greater likelihood of the event, and a 3d makes it certain. Whatever you do, register [ἀριθμοίης] the prognostics of the current year, carefully noting what the prognostic says [ὁπποῖον καὶ σῆμα λέγοι; that is, what the event shows it to be a sign of], if such

* Ne nocturna quidem carpentes pensa puellæ
Nescivere hiemen: test' cum ardente viderent
Scintillare oleum, et putres concrescere fungos.
Georgic. lib. i. lin. 390.

Ἢ λύχνοιο μύκητες ἀγείρωνται περὶ μύξαν,
Νύκτα κατὰ σκοτίην, μηδ' ἢν ὑπὸ χείματος ὥρῃ
Λύχνων ἄλλοτε μέν τε φάος κατὰ κόσμον ὀρώρῃ,
Ἄλλοτε δ' ἀΐσσωσιν ἀπὸ φλόγες, ἠΰτε κοῦφαι
Πομφόλυγες &c. Αρατ. Διοσημ.—Orig.

a sort of morning* come on with the rising or setting of any particular star. And it will be of the highest importance to attend particularly to the 2 quaternions of the expiring and the incipient month† [that is to the last 4 days of the month going out, and the first 4 of that which is setting in], for they comprize the extremities of the 2 months, where they meet: and the weather [or the state of the air] is then particularly uncertain [difficult to guess at] for 8 nights, for want of the silver-coloured moon. If you attend to all these put together, all through the year, you will never form a random guess about the weather." The uncertainty of the weather for these 8 nights cannot be an uncertainty of the effect depending on the moon's aspect; but it is an uncertainty of foreknowledge, the poet speaks of, for want of the moon as an index. For though the word σφαλερώτατος by itself would be ambiguous, as it might be taken either in the sense of δυςόχαςος or εὐμετάϐλητος, the words χήτει χαροποῖο σελήνης are decisive for the first interpretation. The moon exists during these 8 months as at other times. There is no want of her therefore as a physical agent: the only want there can be, is the want of her appearance. It would be unpardonable not to mention, that so great an authority as that of Theophrastus is against the side of the question to which I incline. The doctrine of the influence of lunar aspect is expressly asserted in his Treatise on the Signs of Rain and Wind. He says, that the new moon is generally a time of bad weather, because the light of the moon is wanting;‡ and that the changes of the weather generally

* Such a sort of morning.—That is, a morning marked with such or such appearances. So I understand τοίη ἠώς. The spirit of the precept seems to be, that the heliacal risings of the stars are to be attended to, in conjunction with the particular appearances attending the dawn or sun-rise. The heliacal risings show the season, or general constitution of the time of the year; the particular appearances of the morning indicate the minute circumstances of the weather for 2 or 3 days to come. Thus the heliacal rising of Arcturus was a sign, in all the ancient parapegms, that the stormy season was at hand, and bad weather of various sorts, rain, thunder, high wind, was to be expected; but what the particular weather would be for a day or two to come, whether it would be only windy, or wet, with thunder or without, from what quarter the bad weather would come, all this would be pre-signified by the particular appearances of the morning. Perhaps the same appearance may be subject to some variety of interpretation at different seasons of the year, and in different places. In this, experience and observation will be the only sure guides. And for this reason Aratus advises his scholar, not only to attend to the general rules laid down for him, but to keep a journal for himself, and make his own conclusions.—Orig.

† And it will be of the highest importance to attend to, &c. μάλα δ' ἄρκιον εἴη φράζεσθαι. I have sometimes thought these words might be rendered thus: "This will be of great importance [that is, this joint observation of the general indications of season and of particular prognostics will be of great importance] in order to form a conjecture about the two quaternions, &c." This interpretation would make the most connected meaning for the whole passage; but I do not recollect, nor can I find on the strictest search, any instance, wherein the verb φράζεσθαι is used in the sense of conjecturing, or forming a judgment or opinion about.—Orig.

‡ Διὸ καὶ αἱ σύνοδοι τῶν μηνῶν χειμέριοί εἰσιν. ὅτι ἀπολείπει τὸ φῶς τῆς σελήνης, &c. Theophrast. de signis Pluv. p. 417. Edit. Heins.—Orig.

fall on the syzygies or quadratures. But this seems to have been merely an opinion founded on an imaginary analogy between the epochs of syzygy and quadrature in the months, and the equinoctial and tropical epochs in the year. For the moon, he says, is, as it were, the sun of the night. Theophrastus, though a diligent observer of nature, was deep in the theory of that school, of which he was himself one of the brightest ornaments: and his testimony, with respect to the matter of fact, hath not, like Aratus's, a credibility founded on the mediocrity of his genius.

In the table, p. 620, the changes which fell on the syzygies and quadratures, or on any one of Pliny's critical days of the moon's age (which are the 3d, 7th, 11th, 15th, 19th, 23d, 27th), are distinguished from the rest by a larger character.* And out of 69 changes registered in this table, 32 claim that distinction. Which is rather a larger proportion of the whole number, than is due to the time made up of all the days of syzygy and quadrature, in the whole year, together with Pliny's critical days, thrown into one sum. For since there were 365 days in the year, and the days of syzygy and quadrature, with Pliny's critical days, amount to 113, out of 69 changes in the whole year, 22 are as many as belong to these particular days, on a proportional distribution. But in the preceding table, there are many alterations marked as changes, when it appears that the weather returned to what it had been before the time of change, within the space of 24 hours after it. Now if we reject all these on both sides of the question (which I think is the fair way of reckoning, for sudden alterations, of so short a duration, are rather to be called irregularities than changes of weather), we shall find but 46 changes in all, from one settled state to another, of which only 20 fell on the days of syzygies, quadrature, or Pliny's days, which is still more than the just proportion.

But again. Pliny's 8 critical days were probably intended for the 4 days of syzygy and quadrature, and the 4 of octagonal aspect.† For if the time of the conjunction be rightly assumed, the mean quadratures, and the mean opposition, and the mean octagonal aspect, will always fall either on one of Pliny's days, or on the day next to it. The deviation, I suspect, was intentional, and for the sake of the odd numbers. Thus the 4th, 8th, and 12th days of the moon should have been critical, instead of the 3d, 7th, and 11th, if the mean motions of the moon had been the single thing attended to. But Pliny, or whoever was the first author of the rule he gives us, chose the latter as containing, besides much of the lunar influence, all the magic virtue of imparity,

* Sunt et ipsius Lunæ octo articuli quoties in angulos solis incidit, plerisque inter eos tantum observantibus præsagia ejus, hoc est tertia, septima, undecima, decima quinta, decima nona, vigesima tertia, vigesima septima, et interlunium. Plin. Nat. Hist. lib. 18, c. 35.—Orig.

† The words, Quoties in angulos solis incidit, imply this.—Orig.

of which the others, taking their numerical denomination from even numbers, are totally destitute. Among the numerous believers in the moon of our days, few, I suppose, retain any confidence in the physical powers of the odd numbers. They may imagine therefore, that the apparent inconsistence of Pliny's rule with the truth of things, may be owing to his superstition about the odd numbers, which led him wilfully to deviate from the mean epochs, little apprized (for the Romans never were astronomers) how much they sometimes differ from the true ones, on account of the great and various inequalities of the moon's motions, and how very widely his arbitrary arrangement would in consequence often differ from the times it was intended nearly to represent.

Instead of Pliny's critical days, I shall now therefore examine the days for which I imagine they were substituted; those I mean of true syzygy, true quadrature, and true octagonal aspect. The following table distinguishes the changes of weather which fell on these days. There were only 22 such, out of all the 69; which is scarcely 4 more than their even proportion. And rejecting, as before, on both sides, the alterations of weather which were reversed within the space of 24 hours, there remain, out of 46 changes in all, only 10 on the days of lunar influence, which are 2 less than belong to them on the even chance; for the days of syzygy, quadrature, and octagonal aspect, in the whole year are 98; and $365 : 98 = 46 : 12\frac{1}{2}$ very nearly. It is remarkable that, of these 10 changes, 2 only coincide with a new moon; namely, those of the 10th of February and 5th of September, and none at all with a full moon. There were indeed 2 changes in the year on the day of the full moon; videlicet, those of the 20th of September and 18th of Nov. but both were reversed within the space of 24 hours.

TABLE IX.

Jan.	6 7 9 10 14 18 23 25 26 30	10	3
Feb.	4 7 8 10 13 15 17 20	8	3
March	10	1	0
April	4 8 28 29	4	1
May	5 7 23	3	0
June	6 17 18 20 22 28 30	7	3
July	5 15 20 22 27 31	6	1
Aug.	4 6 11 15 17 26	6	2
Sept.	1 5 7 11 13 14 17 20 22	9	4
Oct.	3 23 24 29	4	1
Nov.	2 6 7 18 21 26	6	3
Dec.	2 5 11 14 15	5	1
		69	22

I have added in this table 2 columns, showing the number of changes in each month, and the number out of each agreeing with the moon. I shall only add, that no conclusion must be drawn from the observations of a single year.

XVII. Extract of a Meteorological Journal for the Year 1774, *kept at Bristol. By Samuel Farr, M. D.* p. 194.

Months.	Barometer. Highest.	Lowest.	Mean.	Vicis.	Rain.
Jan.	30.1	28.8	29.5	1 1-2	4.951
Feb.	30.4	29.2	29.7	0 9-1	5.549
March	30.2	29.1	29.7	0 9-4	5.297
April	30.1	29.3	29.7	0 8-5	2.349
May	30.1	29.3	29.9	0 7-4	2.955
June	30.2	29.4	29.7	0 6-3	2.602
July	30.2	29.7	29.8	0 4-1	2.972
Aug.	30.2	29.4	29.8	0 5-2	2.999
Sept.	30.1	29.0	29.6	0 7-2	7.035
Oct.	30.5	29.3	30.0	0 8-2	1.927
Nov.	30.2	29.2	29.7	0 6-1	1.683
Dec.	30.6	29.0	29.7½	0 7-2	2.047
Mean of the whole was			29.74		42.366

The barometer was placed 17 yards above the level of the river Avon, which runs very near the house. By vicissitude is meant the greatest rise or fall of the quicksilver in the smallest number of days.

Dr. Farr had also given the mean heights of the thermometer within doors for every month in the year. But these are omitted, because observations of the thermometer in the house are of no importance, unless accompanied with corresponding ones of an instrument kept in the shade in the open air. The air of a room, though kept without a fire, and so situated as never to see the sun, alters its degree of heat or cold so much more slowly than the external air, that no judgment can be formed of the temperature of the one from that of the other; except after a continuance of weather of the same kind for a long time together, their mutual relation is vague and undetermined. Dr. Farr, likewise sent a particular account of the winds and changes of the weather for every day of the year; from which I have composed the two following tables.

S. HORSLEY.

An Abridged Table of the Winds for Bristol, for the Year 1774.

	N.	S.	E.	W.	N.W.	S.E.	N.E.	S.W.	Da.		Number of frosty days.
January	3½	½	6	3	1½	2	7	7½	31	3 days in the year are omitted in Dr. Farr's account, viz. Feb. 7, April 29, and July 12.	10
February	½	1½	½	1	3½	3	5½	11½	27		7
March	½	1½	4½	½	3½	5½	11	4	31		7
April	½	2	½	0	8	4½	5	8½	29		
May	½	1½	2	0	2	2	14½	8½	31		
June	1	2½	2	½	4	1	2½	16½	30		
July	1	1	0	2	6½	2	0	17½	30		
August	0	½	1½	0	1	4	6½	17½	31		
September	½	½	0	½	4	10	7½	7	30		
October	0	1	2	½	3½	6	5½	12½	31		Frost at times.
November	1	½	0	0	4	5	13½	6	30		Frosty nights.
December	0	0	3	0	½	8	13½	6	31		18
	9	13	22	8	42	53	92	123			42

Thunder, February 16, 23, 24, S. W.—March 8, 20, E. 28, E. and W. 30, E. and N. E.—April 27, with hail storm, S. W. and N. W.—May 1, 4, N. E. 9, 10, E. 24, S. W. and S. E.—June 25, S. W. and S.—July 10, S. W. 26, N. and N. W.—September 4, S. E. and N. E. 6, N. W. 12, S. W. and S. E.

Table for Trial of the Moon's Influence at Bristol, for the Year 1774.

	Last qr. d. h.	New. d. h.	First qr. d. h.	Full. d. h.			
January	5 6	11 21	19 3	27 7	1 5 10 13 19 22 30	7	2
February	3 15	10 9	18 0	25 23	7	1	1
March	4 22	11 22	19 20	27 11	6 10 17 20 30	5	1
April	3 5	10 12	18 15	25 22	6 10 13 .	3	1
May	2 12	10 3	18 7	25 5 Last qr. 31 20	1 10 19	3	1
June	New. 8 18	First qr. 16 19	Full. 23 12	Last qr. 30 7	1 6 13	3	1
July	8 9	16 5	22 19	29 20	1 10 14 17 19 26	6	2
August	7 0	14 12	21 3	28 12	1 3 7 25	4	3
September	5 14	12 17	19 13	27 7	Only 9 fair days.		
October	5 3	12 0	19 2	27 3	6 17 25 30	4	4
November	3 15	10 7	17 18	25 23	Cloudy with rain till the 5th, hard rain, then frequent frosty nights, and gentle rain in the day-time.		
December	3 2	9 17	17 12	25 17	3 11 21	3	2
						39	14

When a number appears in this table without any character over it, it is to be understood, that the weather was quite unsettled from that day to the next bearing a mark; and when 2 or more marks are found over the same number, all the different kinds of weather, denoted by the several marks, took place on that day. The same is to be understood in the tables, in my paper preceding.

This table distinguishes the changes of weather which fell on the days of true syzygy, true quadrature, and true octagonal aspect. Setting aside the very changeable months of September and November, there were 39 changes in the remaining 10, 14 of which happened on the days specified; which is almost 4 more than belong to them on the even chance. Of these 14 changes, only 4 fell on the day of a new moon, and none at all on the day of the full.

S. HORSLEY.

XVIII. Extract of a Register of the Barometer, Thermometer, and Rain, at Lyndon, in Rutland, 1774. *By Thomas Barker, Esq.* p. 199.

		Barometer.			Thermometer.						Rain.
					In the house.			Abroad.			
		Highest.	Lowest.	Mean.	High.	Low.	Mean.	High.	Low.	Mean.	
January	Morn.	20.77	28.32	29.15	42	31½	35	43	20	29	3.308
	Aftern.				43	32	36	46	28	33½	
February	Morn.	30.05	28.49	29.25	46	33½	40	45	22	34½	1.946
	Aftern.				46½	35	41	51½	29½	41	
March	Morn.	29.81	28.56	29.30	48½	38	43	44	28½	36	2.728
	Aftern.				51	39	44½	57½	35½	46	
April	Morn.	29.77	28.72	29.24	53	44½	48	52½	32½	42	1.523
	Aftern.				54½	45½	49	62½	37½	51	
May	Morn.	29.67	28.76	29.35	55	48	51½	55½	40	46	3.142
	Aftern.				56½	49	53	69½	45	57	
June	Morn.	29.76	28.87	29.33	62	54	59	61	50	55	2.483
	Aftern.				66	55½	60	73½	56	65½	
July	Morn.	29.76	29.10	29.41	63½	57½	60	61	52	56	3.227
	Aftern.				66½	58½	62	76½	61	66	
August	Morn.	29.80	28.80	29.38	68	58	61½	64	47	55½	3.910
	Aftern.				70	60	63½	78½	59	67	
September	Morn.	29.74	28.70	29.28	65	53	56	61	40	49½	8.000
	Aftern.				68½	53½	57½	73	48½	59	
October	Morn.	30.06	28.92	29.64	56½	46	52	51	34	43½	1.156
	Aftern.				57½	46	53½	64½	42	53	
November	Morn.	29.73	28.73	29.36	52½	35½	43	49	28	37	1.530
	Aftern.				52½	36	44	55½	32	41	
December	Morn.	30.21	28.68	29.60	45½	32	39½	44½	20	33½	2.280
	Aftern.				46	32½	40	47	25	38	

Means of all .... 29.35 .... 49.7 .... 47.3 .. 35.235

XIX. Of some Thermometrical Observations, made by Sir Robert Barker, F. R. S., at Allahabad in the East Indies, in Lat. 25° 30′ *N. during the Year* 1767, *and also during a Voyage from Madras to England, in* 1774. *From the original Journal by the Hon. Henry Cavendish, F. R. S.* p. 202.

The greatest part of the observations at Allahabad were made within doors; several were made within a tent placed under the shade of trees, some in the open air in the sun, and some in the open air in the shade; but there is no regular series of observations in any one place; nor were they made at stated times of the day. Though a thermometer kept within doors is but a very indifferent measure of the heat of any climate; yet as I have not seen any thermometrical observations made in that country, except a few during the heats of the summer, and printed in the Philos. Trans., vol. lvii, p. 218, I have set down the greatest and least heights met with in each month.

	Least.	Great.		Least.	Great.		Least.	Great.
January	58	72	May	72	101	September	78	83
February	60	84	June	81	99	October	72	87
March	62	94	July	81	90	November	52	86
April	79	96	August	80	86	December	51	64

From the 3d of May to the 4th of June inclusive, a thermometer placed within a tent, under the shade of trees, was almost every day above 100°, and several times above 109°, once at 112°. The trees under which the tent was placed, formed a very thick shade; so that probably these heights are more likely to fall short of the true heat of the open air at that time, than to exceed it. The least height he met with of the thermometer in the open air in the shade, is 42°; which it was at twice in the month of January, at 7 A. M. The greatest heat is on June 9th, at noon, when it was at 114°, the sky cloudy; the thermometer within doors at the same time 95°, which is less than it had frequently been in the month of May; so that it seems likely, that the heat in the open air in May had frequently been above 114°. During the voyage to England, the thermometer was placed in the round-house, and was observed regularly at 8 in the morning, at noon, and at 3 in the afternoon; the winds and weather are also set down. The round-house is one of the uppermost row of cabins, and is reckoned the coolest and most airy part of the ship. From February 13 to April 7, between Madras and the southern tropic, the thermometer was constantly between 77° and 86°, and very seldom lower than 80°. From that to April 23, lat. 34° 12′, about 15° E. of the Cape of Good Hope, between 70° and 80°. Thence to May 20, at St. Helena, between 62° and 72°. Thence to August 2, in lat. 43° 14′ N, between 71° and 80°; and thence to August 15, in the British Channel, between 62° and 70°. At land it is well known that the heat is usually considerably greater in the middle of the day, than in the morning or night; but it appears from these observations, that in the open sea, there is scarcely any sensible difference; for in settled weather, the difference between the different times of the day was rarely more than 1°, oftener none at all. In unsettled weather there was frequently a difference of 2°, sometimes 4°, scarcely ever more; but then there seems no connection between this difference and the time of the day, it being as often colder in the middle of the day than in the morning or evening, as warmer. There is added a register of the thermometer, in the soldiers' barracks at Allahabad, on June 8, 1769, when from 10 in the morning to 8 in the afternoon it stood constantly above 100°, in the hottest part of the day at 107°, and during the whole night between 99° and 98°.

Sir Robert Barker gives the following account of the general state of the weather in Bengal.

The rains at Bengal generally set in between the 1st and 15th of June, and continue till the middle of October, when it remains fair till February, the wind blowing mostly from the N. E. quarter, in which month and March it is interrupted by the N. W. squalls, attended with violent gusts of wind, thunder, and lightning, with short, but excessive hard showers of rain or hail, commonly one, but rarely 2 in each day. From the middle of March to the middle of

June the weather is very hot. At Allahabad and the upper country the rains are not expected till the 20th of June, and seldom exceed the 30th, excepting in extraordinary seasons, when it has been known to keep off till the 5th of July; but such an event is usually attended with a great mortality both of men and beasts. They break up about the middle of Sept. and from this time to the beginning of Jan. it continues fair cold weather. In Jan. there are almost always a few days rain, seldom more than a week, and that gentle and pleasant, which is productive of a 2d crop, which they usually reap. The winds at Allahabad set in easterly from the beginning of the rains, and blow almost constantly from that quarter till the conclusion of the cold weather in March, when it changes more northerly, and is attended by violent north-west squalls of thunder, lightning, rain, and hail, at which time it changes to the west, blowing with violence, and a heat which frequently destroys the birds and beasts in the fields, till the rain affords a relief. The river Ganges begins to swell before the commencement of the rains, reported by the natives to proceed from the melting of the snow on the northern mountains during the heats of May and June. But the sudden rise of the waters in the Ganges, a few days after the setting in of the rains, is almost incredible; since it has been known to rise 20 feet in 48 hours; and its sudden fall is as extraordinary. In Bengal the rivers are of course affected by the rise and fall of the Ganges. Floods continue the whole time of the rains, more or less; but the greatest overflowings are generally at the beginning and the end or the breaking up of the rains, at which period it rains with the greatest violence. The waters at Allahabad, and in all the upper countries, run off into the rivers as soon as the rain has ceased, the soil being for the most part of sand, and the country intersected with small rivulets; but in Bengal, and particularly so low down as Calcutta, being of a clay soil and an extensive flat, the whole country is overflowed, forming lakes of great extent, some of them being 6 miles over. The water therefore generally remains till the sun has exhaled it, by which it becomes putrid, and renders those parts extremely unwholesome, occasioning those deadly putrid fevers, which carry off the patient in a few hours, known by the name of pucker fevers.

XVI. A 2d Essay on the Natural History of the Sea Anemonies. By the Abbé Dicquemare. Translated from the French. p. 207.

I was concluding my essay on the sea anemonies, says Mr. D., inserted in the 63d volume of the Philos. Trans., [page 460, of this abridged volume,] when I discovered a 4th species of that animal; and I have reason to think that I have since observed a 5th species. New observations have increased the number of my experiments: my ideas have been enlarged, my views extended; and the phenomena crowd in so fast upon me, that I dare not flatter myself with the

hopes of ever arriving at the end of this pursuit. The scarcity of high tides, the vicissitudes of seasons, and other similar impediments, make it less wonderful that a series of years should often elapse before it is possible to present the curious with any discoveries of which they might avail themselves, either by analysis, combination, or analogy, and thereby furnish general views and a chain of ideas leading to a new field of discovery, the usual effect of contemplation. I shall here communicate some of the ideas that have been suggested to me by my last experiments.

How many are the animal functions, which seem to depend on sensibility and irritability; and yet how little are these faculties understood? how ignorant are we of their cause? The nerves seem to be the chief, perhaps the only organs of sensibility in man, and the muscular fibres to be the principal seat of irritability; yet how many are the doubts entertained concerning the parts that are and are not endowed with one and the other! how false and erroneous the conclusions relating to the effects they produce, notwithstanding the many experiments made on animals, whose interior structure is the most similar to our own! It is then from accurate observations on such animals as bear the least resemblance to our species that we may hope for new discoveries. The sea anemonies are exceedingly gelatinous, and at the same time so irritable, that even light affects them, though to all appearance destitute of eyes. Might not the rapid and singular reproduction of the parts of this animal be attributed to their gelatinous texture? and if so, may we not reasonably conclude, that the reproduction of our vascular and fleshy parts in the consolidation of wounds, is in great measure owing to such a gelatinous matter; and should we not seek for means to increase or diminish the quantity of that matter as circumstances may require. If it be true, that earth and a gelatinous substance are the constituent parts of the muscular fibres of such animals as we are best acquainted with, and that only the latter are capable of irritation; doth it not follow, that the gelatinous nature of the sea anemonies is the true cause of the effect produced on them by the impression of light? and may we not conjecture, from the very gelatinous nature of these animals, and from their being affected by light on every part of their bodies, but more particularly on those that are recently cut; may we not then, from hence conjecture, that the gelatinous part of the muscular fibre is the only one capable of irritation in ourselves? Might not these animals, by a sober use of analogy, or by new experiments, lead us to a more perfect knowledge of those singular enemies to man, the tape, the hair worm, and the sea-dragon?

I continued to observe the inferior half of a purple anemony of the first species, which I had cut in two on the 12th of July 1772, and which was alive on the 8th of April 1773, the day on which I concluded my former essay: it ap-

peared to be daily recovering strength. On the 26th I found it at the bottom of the vessel. On the 1st of July it climbed up the sides almost to the surface of the water; and this it repeated on the 15th and 22d, above a year after the time it had been cut. On the 25th a crab (cancer lanosus, cancer venenatus) half dried, fell into the vessel, and after continuing in it some hours, infected and tinged the water in the same manner as if husks of walnut or pieces of soot had been thrown into it, which had such an effect on the piece of anemony, that it threw up a great quantity of its intestines. On the 30th it laid hold of the side again, but was considerably shrunk. In the beginning of September it received a 2d injury, from another piece of anemony, which having been damaged in the same manner by the former accident, suddenly putrified and infected the water: more of the intestines were now discharged; and this last accident, added to the former one, affected the creature to such a degree, that it wasted gradually till the 8th of October, when it was totally dissolved. The sea anemonies are undoubtedly susceptible of irritation to a very great degree; but is all that has been described to be considered as the mere effect of irritability? Allowing that to be the case, will it not follow, that we are more in the dark concerning that faculty than is generally thought? It is usual to ascribe to it the palpitation that is perceived in the flesh of oxen, when cut from the animal, in the severed pieces and hearts of some reptiles, as the sloth, and other involuntary spasmodic motions; but is it possible that determinate motions, that actions which seem to imply will, such as clinging, &c. which in our experiment were continued for the space of 15 months, and, but for an accident, might probably have been carried on much longer, should arise from mere irritability, without any other cause? The upper part of another sea anemony, of which the inferior was become a perfect animal, lived 6 months after its being cut, and seemed to feed by suction on pieces of muscle I put in its way.

Sea anemonies, cut diametrically and perpendicularly, were not essentially hurt by that operation; which might be expected to disorder more than any the whole animal economy, and to be particularly injurious to the basis of this animal, which is its most essential part, and in some species is exceedingly tender. The two sides soon came together, but were some time in contact before they connected. The junction however was at last so perfect, that no visible scar remained on the robe, the continuity of the little blue edge was not in the least interrupted, and the mouth was perfectly restored. These semi-anemonies have long since acquired the appearance of the perfect animal, and perform all its functions, such as moving from place to place, swallowing, &c. This leads to the reflection, that if, as has been asserted, the power of locomotion in these animals depends on a certain combination of straight and circular tubes, it is not requisite, in order to exert it, that the continuity of these

tubes be uninterrupted, since half an anemony newly cut changes its place with as much ease as a whole one. It will no doubt appear a curious inquiry, whether these semi-anemonies, after becoming in a manner whole ones, are capable of propagating their species. To this I can only answer at present, that I have not yet seen the generation of anemonies, except in the sea, or in animals newly taken out of it. It must further be observed, that these anemonies, perfect as they seem to be, may perhaps have only half the number of limbs of the whole ones, of which they made a part: so that the whole wonder comes to this, that the severed halves of an animal should recover, and each taking the appearance of an entire individual, continue to live as if they were such. And such in fact I believe they are; but this I have not yet been able to ascertain, as the anemonies of this species have not all the same number of limbs, as it is always very difficult to count them, and as all those on which I have hitherto made the experiment had a great number of them. However, as no manner of difference appears, I am inclined to suspect that new limbs shoot out between the old ones.

After having observed these animals during several years, both in the sea and in my study, it will no doubt be expected, that I should now give a particular account of their manner of propagation; but here I can only confess my ignorance, having never been able to get at the knowledge of any one circumstance relating to it: which makes me suspect, that these animals propagate without any communication of individuals. What I would here suppose, is by no means unexampled. Among the aphides, for instance, whose mode of propagation deserves to be further examined, though the sexual parts have been discovered, individuals nevertheless are found, which, though deprived of all communication one with another from the very moment they are brought forth, yet produce an offspring, which being likewise denied all intercourse, still propagates; and so on, through a great number of generations, which succeed each other very rapidly. The muscle also is thought to be an animal of the same nature.

The anemonies of the 2d species are not only less obvious to our observation, but it is with difficulty they are preserved in any degree of perfection. They cannot be taken out of the sand without depriving them of their natural position. Common mixed sand kills them in a few days; and that which is purified affords them no longer the slime, the small insects, or other necessary sustenance, which we cannot possibly divine. In plucking them from their native soil, their bases generally suffer, and the wounds in that part are frequently mortal. One of the safest expedients is to gather with them the pebbles to which they adhere; or what is still preferable, to observe them in their natural element the sea. It is there that, without the least hostile appearance, they are seen to make an in-

credible havoc. I have seen an anemony of a moderate size swallow a smelt at least 6 inches long. The limbs of this species, which are much thicker than those of any other, being clipped, new ones shoot out as in former cases. The progress of this reproduction, which is effected in a few days, is scarcely perceptible; and it is so perfect, that no protuberant rim or visible scar remains. Neither the colour, the size, nor the form are any ways altered. This anemony is able to creep when deprived of its limbs; which seems to prove, that the communication, which is thought to exist between the limbs and the hollow muscles, may be interrupted, without sensibly restraining the animal's locomotive powers. Those limbs, it is true, enable the animal to crawl when turned on its back; but do by no means serve as legs for walking steadily, as hath been erroneously asserted, and misrepresented in ill-drawn figures. I made large incisions on several anemonies in the sea, which healed in a very short time; but I always took care not to injure the basis, as any considerable wound, and especially the least rent, on that part of this species, proves often mortal. I do not mean to question the possibility of what hath been repeatedly said of an anemony, which not being able to void a muscle it had swallowed, forced it out through a rent it made with the muscle itself at its basis, and that this rent was soon after perfectly cicatrized. But a love of the marvellous too plainly appears throughout the whole narration, and the inferences drawn from the fact give room to suspect, that little attention had been paid to the concomitant circumstances. Wounds of this nature often occasion a disorder in the interior part of the anemony, the progress of which soon brings on its total dissolution. Of all the kinds of sea anemonies, I should prefer this for the table: being boiled some time in sea water, they acquire a firm and palatable consistence, and may then be eaten with any kind of sauce. They are of an inviting appearance, of a light shivering texture, and of a soft white and reddish hue. Their smell is not unlike that of a warm crab or lobster. I have seen some of the young of this species, but have not been able to make any discovery concerning their mode of propagation.

A detail of my former observations and experiments would be here a useless repetition: I shall therefore only observe, that the semi-anemonies of the 3d species have so entirely recovered the parts they had been deprived of, whether the superior or the basis, that no manner of difference could be perceived. Some of the men of learning, whom my first discoveries brought to my study, imagined that the basis was the most essential part of the animal, and that the mouth and limbs were to be considered only as extremities. I was myself inclined to adopt that notion, seeing that in all the species abovementioned, the basis ever gave the greatest marks of sensibility, that the intestines are situated in that region, &c. but who, on seeing the upper part of an anemony producing

a new basis, perfectly similar to that which had been severed from it, will any longer maintain such an opinion? During the great equinoctial tides, in places whence the sea seldom recedes, I saw several of these animals which had been cut through the middle, perhaps by some crab, or by the sudden collision of pebbles, or by some other means, which though not unnatural, we may yet not be able to account for. They soon began to recover. I should have taken them for a new species, had not my former experiments pointed out to me the gradual reproduction with which nature, no less various than impenetrable in her resources, kindly indulges them. Are not the accidents which happen to birds, quadrupeds, and even to man, frequently followed by effects, which seem intended to convince us, that we lay too great a stress on the resources of art, and trust less than we ought to do to nature? Though I could never yet arrive at any certain knowledge concerning the generation of this species, I suspect that it is different from that of the others. Several of my specimens have suddenly let fall to the bottom of the vessel, in which they were kept, a slimy substance, nearly of the colour of their bodies, perhaps somewhat yellower, which, in the microscope, appeared to consist of a great number of globular particles, pretty much resembling the spawn of fish.

The first anemonies procured of the 4th species, had probably been brought near the coast by fishermen, for they generally keep in deep water, where they are found adhering to oyster shells. I caused several to be brought into my study, where being put into sea water they soon expanded. The largest, which opened first, puzzled me not a little. I could discover no basis, but only saw limbs projecting on every side. I flattered myself that a greater expansion would clear up the difficulty; on the contrary it only added to it. The others opened, and appeared in a shape much more similar to that which I expected. I saw a basis, a body, a great number of slender limbs, the assemblage of which formed, at first, different kinds of tufts, and afterwards various fine plumes of a whitish hue inclining to carnation. I returned to my first specimen, which now appeared to consist of 2 animals joined at the basis. I became very solicitous to unravel the mystery of this singular union. At length I perceived, that this was a monster of its kind, consisting of 3 different animals blended into one. It perished 12 days after I had received it. Its internal structure, though in great confusion, was yet an interesting object to those who are acquainted with that part of this animal, and who have a taste for comparative anatomy. It appeared in such disorder, that I can scarcely conceive how it was possible for the creature to live in that condition. The state of dissolution, which began soon after, and the impossibility of representing every part at one view, prevented my taking a drawing of this remarkable inside. Its mouths on either side were regularly shaped, but rather less than the usual size; and in the folds, formed

by the bases, several limbs appeared, which seemed to belong to a 3d animal, incorporated in the 2 that were more apparent. The sequel will show, that this is not the only peculiarity observed in this species, which by its manner of propagating seems particularly calculated for producing monsters. The anemonies of this kind are commonly found adhering to the convex shells of oysters: they abound in the road of Havre-de-Grace, so that I had no difficulty in procuring whatever number I chose. A viscose matter, like that which is seen on fish newly caught, issues from them. I have opened 2 or 3 hundred of a large size, but I never found in any of them either whole or parts of animals; and yet as often as I offered them a piece of oyster or muscle, they would swallow it. The large anemonies of this species are generally surrounded by a multitude of small and middle-sized ones, which form very pleasing groups, see fig. 9, pl. 12. The bases of some of these small anemonies were not perfectly round, and in others they mutually adhered to each other: and when the basis between two connected anemonies were slightly touched with a pointed instrument, they both contracted at the same time. This common basis distended itself gradually near the middle, where it assumed the appearance of a net, which at length bursting, left every small anemony to live by itself. There issues out of the body of the anemonies of this species, through little pores, and also out of their mouths, a considerable number of round, soft, limber threads, of the thickness of a horse-hair, and of the colour of the animal. On viewing them through a lens, I observed a great resemblance between them and the spermatic vessels of men, when stripped of their outward sheath. Through a common microscope I saw fibres in them which crossed each other in every direction: and by means of a solar microscope, which magnified them to a diameter of 5 inches, they appeared of a very close texture, which, when decomposed, seemed to consist of an infinite number of vessels, crossing each other in almost every direction; but farthest extended lengthwise. A liquor seemed to circulate in the largest of these vessels, which, where they meet, form kinds of ganglions like the optic nerves in man. Such an organization cannot surely but be intended for valuable purposes. Is it not probable that these threads contain certain knots, bulbs, knobs, or buds, which open in time, and cleaving to the bodies on which those threads are extended, produce small anemonies, which at first communicate with each other, but afterwards separate by a contraction, as I have indeed observed in some of them. This I concluded from never having found any young ones in the great number of anemonies I have opened; and yet I have seen prodigious quantities, of a very small size, adhering to oyster shells. But, from a series of observations, I have learnt, among other singularities, that these animals having their bases irregularly distended, and their extremities closely adhering to some hard body, commonly an oyster shell, by suddenly shrinking, they leave

on that body some small portions of the rim of their bases, in size inferior to a lentil. These little shreds have at first no determined figure; but gradually assume the rounded shape of a drop of tallow: at length, in about 2 or 3 months, a hole appears in the middle, which forms the mouth. An internal organization, dilatations and contractions, sensibility, and other gradual improvements, soon after prove them to be animals similar to those to which they owe their origin. It might be imagined, that some time must elapse before they can grow to a circumference of 2 feet. I have not been able to follow them to that degree of increase; but I have seen them in my house, where they are far from being so favourably situated as in the sea, growing to a size large enough to convince persons of ever so little observation, that they belong to the species of large anemonies. The same shred often produces several small anemonies, which at first adhere together, and in time are separated by the little contraction already mentioned; but if they happen to remain connected, they then produce singular forms and often monsters. Besides the anemony abovementioned, the old one of this species, which has particularly unravelled this mystery, was formed in the shape of a Y, having 2 perfect bodies, of which the bases, both perforated, adhered and communicated to the same trunk; as appeared by observing that the food descended into the main trunk: neither did these 2 anemonies, thus connected, ever appear to have different inclinations, as is the case with those that are once separated. Is it not reasonable therefore to suppose, that in this state of union, every want was common, and each had its separate desire of satisfying it; and that, to keep up the habitual exercise of the animal functions in each, both were on all occasions prompted to the performance of the same function at the same time.

In order to imitate the effects of nature, I clipped several small pieces from the rim of the bases of anemonies of this species, and preserved them. Some of them became small anemonies, similar to those that had been torn off of their own accord; but many perished without producing any thing. May we not conclude from this experiment, that the prolific pieces contained a small bulb intended to become a new anemony, and to be soon after torn off by the mother; and that those which perished, either contained none of those bulbs, or such as were not sufficiently formed to thrive and grow after a violent amputation? I the rather incline to this opinion, having observed, that among the pieces I had cut off, those particularly succeeded which appeared interiorly replete and of a certain thickness. If so, this conjecture may possibly lead us to another. It is well known that the fresh water polypi increase by section, and that being cut into several pieces, each of these pieces becomes an animal similar to the original one. Thus a polypus being divided in 2, 4, 8, 16, or more parts, each of these parts probably contains a bulb capable of becoming anothe

polypus. In the course of my experiments, the small pieces cut off from the bases and robes of the anemonies did not exceed the 500th part of the animal; it is not therefore to be wondered, if many of them did not prove prolific; they probably contained none of the fertile bulbs. The reproduction of the polypus by section will then no longer be attributed to any of its rude and shapeless parts; but rather to parts that are organized in a particular manner, to eggs, or perhaps to something more than eggs. The singular propagation of several kinds of this animal seems to favour this conjecture. In so minute an object as the fresh-water polypus, much is easily overlooked; but in the sea anemonies, though we are far from seeing every thing, yet it is possible, even without the assistance of glasses, to discern a great deal which must escape us in the most diligent examination of the other animal. The first observation of any note, was made on the 16th of June 1773. The animals of this species being very large, I only operated on young ones, and on that day cut one that was not thicker than a goose-quill. On the 30th the upper part was perfectly restored, and the fold which is seen near the upper extremity of the body of this species, appeared exactly like that I had cut off. By practice I arrived at such dexterity as to cut in two, at one snip of the shears, in a very straight line, an animal of this species as thick as my arm. This was performed on the 18th of October: before the end of the month new limbs appeared, of which the large ones, within the tufts, shot forth long before the others. On the 10th of December the animal began to eat, though its mouth was scarcely formed. The upper part was still alive. I tied a string round some of these anemonies while they were considerably extended lengthwise, and pulled the noose very tight. They had the dexterity to free themselves in a few hours of this troublesome ligament, by gradually withdrawing their upper extremity: then on measuring the noose I found it not quite 6 lines in diameter. This species is good to eat.

Among the sea anemonies brought by the fishermen, I have some reason to think that I have discovered a 5th species, which seems to reside only in places from which the sea seldom recedes. They appeared to be as small as those of the first species: their limbs, which are somewhat confusedly arranged in 3 rows, are also nearly the same. They have the form and the knobs of the 2d, and the threads of the 4th species, which latter however are coloured. Their mouths are round, and bordered with small reddish limbs; only 1 white spot is seen on one side of the mouth, whereas 2 of them appear in those of the 3d species. The middle between the mouth and the limbs is of a greenish hue, with narrow variegated streaks extending from the centre to the circumference. The specimens I have seen were white on the superior edge of the robe, of a golden yellow in the middle, inclining to a duskier colour towards the bottom; that is, the ground-colour of the robe changed gradually, from white at the top

to brown at the bottom, passing by imperceptible transitions through a succession of yellow shades, partaking more or less of the colour of gold. This whole robe was speckled with light crimson spots, and no rim appeared at the basis. These anemonies had been found on old volutes, called spindle-shells (fucus brevis.) Another specimen, which was found adhering to an oyster-shell, was of a darker colour; but its limbs bore some resemblance to the horns of cattle: they were of a pale green, with circles of a fine dark brown, which had a very pleasing effect. These limbs appeared, at first sight, to tend towards the centre, by the continuation of some semicircles which gradually diminished.

My very earliest observation showed that the sea anemonies feel and prognosticate, within doors, the different changes of temperature in the atmosphere. I had not leisure at that time to form tables of their various indications; but I have since done it. This fact, if applied to practice, might be of use in the formation of a sea-barometer, an object of no small importance, which several ingenious men have hitherto endeavoured in vain to furnish us with. I should prefer the anemonies of the 3d species for this purpose, their sensation being very quick; they are also easily procured, and may be kept without nourishment. Five of them may be put in a glass vessel, 4 inches wide and as many in depth, in which they will soon cleave to the angle formed by the sides and the bottom. The water must be renewed every day, and, as they do not require a great quantity of it, as much may be fetched from the sea (if they be kept on land) as will supply them for several days; its settling some time will only improve it. If the anemonies be at any time shut and contracted, I have reason to apprehend an approaching storm; that is, high winds and a rough agitated sea. When they are all shut, but not remarkably contracted, they forebode a weather somewhat less boisterous, but still attended with gales and a rough sea. If they appear in the least open, or alternately and frequently opening and closing, they indicate a mean state both of winds and waves. When they are quite open, I expect tolerable fine weather and a smooth sea. And lastly, when their bodies are considerably extended, and their limbs divergent, they surely prognosticate fixed fair weather and a very calm sea. There are times when some of the anemonies are open and others shut; the number must then be consulted; the question is decided by the majority. The anemonies used as barometers should not be fed, for then the quantity of nourishment might influence their predictions. Anemonies of this and of the first species live and do well for several years, without taking any other food but what they find disseminated in the sea water: but should a respite of some days be granted them, they might then be fed with some pieces of muscles or soft fish, and thus restored to their original vigour. Whenever the vessel is sullied by the sediments of salts, slime, the first shoots of sea-plants, &c. it may, on changing

the water, be cleansed by wiping it with a soft hair pencil, or even with the finger, carefully avoiding to rub or press hard on the anemonies. Should any of them drop off during this operation, they may be left at liberty, for they will soon, of their own accord, fix themselves to some other place. Should any of them die, which will soon be discovered by the milky colour of the water, and an offensive smell on changing it, it must be taken out, and on the first opportunity another of the same species be put in its place; those of a moderate size are the most eligible.

Explanation of Fig. 9, pl. 12, which represents a group of sea anemonies of the 4th species, adhering to an oyster-shell. N° 1, is a contracted anemony of the natural and middling size adhering to the oyster shell. N° 2, Anemonies united to the same trunk, compared in the essay to the letter Y. At its basis a little shred appears ready to be torn off, in order to become a new anemony. N° 3 and 4, Two young anemonies moderately expanded, in the middle of which the mouths appear. N° 5, An anemony, somewhat more grown, on which the projecting rim appears. N° 6, Eight small anemonies, 2 of which adhere together, as do also 2 others, which however are on the point of being separated by the contraction of the part that unites them. Other small anemonies, of different sizes, are seen on the oyster shell.

XXI. On the Sea-Cow, and the Use made of it. By Molineux Shuldham, Esq.* p. 249.

There is nothing in this paper sufficiently interesting for re-publication.

XXII. The Process of making Ice in the East-Indies. By Sir Robert Barker,† F. R. S. p. 252.

This paper contains an account of the method by which ice was made at Allahabad, Mootegil, and Calcutta, in the East-Indies, lying between 25½ and 23½ degrees of north latitude. At the latter place Sir R. never heard of any persons having discovered natural ice in the pools or cisterns, or in any waters collected in the roads; nor has the thermometer been remarked to descend to the freezing point; and at the former very few only have discovered ice, and that but seldom. But in the process of making ice at these places, it was usual to collect a quantity every morning, before sun-rise, except in some particular kinds of weather, for near 3 months in the year: viz. from December till February.

The ice-maker belonging to Sir R. at Allahabad, made a sufficient quantity in the winter for the supply of the table during the summer season. The methods he pursued were as follow: on a large open plain, 3 or 4 excavations were made, each about 30 feet square and 2 deep; the bottoms of which were strewed about 8 inches or a foot thick with sugar-cane, or the stems of the large Indian corn dried. On this bed were placed in rows, near each other, a number of small, shallow, earthen pans, for containing the water intended to be frozen.

* *Trichechus Rosmarus.* Linn. † Some time commander in chief of the forces in India.

These are unglazed, scarcely a quarter of an inch thick, about an inch and a quarter in depth, and made of an earth so porous, that it was visible, from the exterior part of the pans, the water had penetrated the whole substance. Towards the dusk of the evening, they were filled with soft water, which had been boiled, and then left in the beforementioned situation. The ice makers attended the pits usually before the sun was above the horizon, and collected in baskets what was frozen, by pouring the whole contents of the pans into them, and thus retaining the ice, which was daily conveyed to the grand receptacle or place of preservation, prepared generally on some high dry situation, by sinking a pit of 14 or 15 feet deep, lined first with straw, and then with a coarse kind of blanketing, where it was beaten down with rammers, till at length its own accumulated cold again freezes and forms one solid mass. The mouth of the pit is well secured from the exterior air with straw and blankets, in the manner of the lining, and a thatched roof is thrown over the whole.

The quantity of ice depends much on the weather; so that it has sometimes happened, that no congelation took place. At others perhaps half the quantity will be frozen; and often the whole contents are formed into a perfect cake of ice: the lighter the atmosphere, and the more clear and serene the weather, the more favourable for congelation, as a frequent change of winds and clouds are certain preventives. For it is frequently remarked, that after a very sharp cold night, to the feel of the human body, scarcely any ice has been formed; when at other times the night has been calm and serene, and sensibly warmer, yet the contents of the pans will be frozen through. The strongest proof of the influence of the weather appears by the water in one pit being more congealed than the same preparation for freezing will be in other situations, a mile or more distant.

The climate may probably contribute in some measure to facilitate the congelation of water, when placed in a situation free from the heat of the earth, since those nights in which the greatest quantity of ice has been produced, were, as before observed, perfectly serene, the atmosphere sharp and thin, with very little dew after midnight. The spongy nature of the sugar-canes, or stems of the Indian corn, appears well calculated to give a passage under the pans to the cold air; which, acting on the exterior parts of the vessels, may carry off by evaporation a part of the heat. The porous substance of the vessels seems equally well qualified for the admission of the cold air internally; and their situation being full a foot beneath the plane of the ground, prevents the surface of the water from being ruffled by any small current of air, and thus preserves the congealed particles from disunion. Boiling the water is esteemed a necessary preparative to this method of congelation; but how far this may be consonant with philosophical reasoning, Sir R. presumes not to determine.

From these circumstances it appears, that water, by being placed in a new situation free from receiving heat from other bodies, and exposed in large surfaces to the air, may be brought to freeze when the temperature of the atmosphere is some degrees above the freezing point on the scale of Fahrenheit's thermometer; and by being collected and amassed in a large body, is thus preserved, and rendered fit for freezing other fluids, during the severe heats of the summer season. In effecting which, there is also an established mode of proceeding; the sherbets, creams, or whatever other fluids are intended to be frozen, are confined in thin silver cups of a conical form, containing about a pint, with their covers well luted on with paste, and placed in a large vessel, filled with ice, saltpetre, and common salt, of the two latter an equal quantity, and a little water to dissolve the ice and combine the whole. This composition presently freezes the contents of the cups to the same consistency of our ice creams, &c. in Europe; but plain water will become so hard as to require a mallet and knife to break it. On applying the bulb of a thermometer to one of these pieces of ice, thus frozen, the quicksilver has been known to sink 2 or 3 degrees below the freezing point; so that from an atmosphere apparently not cold enough to produce natural ice, ice shall be formed, collected, and a cold accumulated, that shall cause the quicksilver to fall even below the freezing point.

XXIII. Of the House-Swallow, Swift, and Sand-Martin. By the Rev. Gilbert White. p. 258.

This excellent paper is reprinted in Mr. White's History of Selborne, to which the reader is referred.

XXIV. Of a Machine for raising Water, executed at Oulton, in Cheshire, in 1772. By Mr. John Whitehurst. p. 277.

Presuming the mode of raising water by its momentum may be new and useful to many individuals, Mr. W. was induced to send a description of a work, executed in 1772, at Oulton, Cheshire, the seat of Philip Egerton, Esq. for the service of a brewhouse and other offices, which was found to answer effectually. The circumstances attending this water-work require a particular attention, and are as follow. A, in fig. 10, pl. 12, represents the spring or original reservoir, its upper surface coinciding with the horizontal line BC, and with the bottom of the reservoir K. D the main pipe, 1½ inch diameter, and nearly 200 yards in length. E a branch pipe, of the same diameter, for the service of the kitchen offices, situated at least 18 or 20 feet below the surface of the reservoir A; and the cock F was about 16 feet below it. G represents a valve-box, g the valve, H an air vessel, oo the ends of the main pipe inserted into H, and bending downwards, to prevent the air from being driven out when the water is forced into it

w the surface of the water. Now it is well known, that water discharged from an aperture, under a pressure of 16 feet perpendicular height, moves at the rate of 32 feet in a second of time; therefore such will be the velocity of the water from the cock F. And though the aperture of the cock F is not equal to the diameter of the pipe D, yet the velocity of the water contained in it will be very considerable: consequently, when a column of water, 200 yards in length, is thus put into motion, and suddenly stopped by the cock F, its momentous force will open the valve g, and condense the air in H, as often as water is drawn from F. In what degree the air is thus condensed, is needless to say in the instance before us; therefore Mr. W. only observes, that it was sufficiently condensed to force out the water into the reservoir K, and even to burst the vessel H, in a few months after it was first constructed, though apparently very firm, being made of sheet lead, about 9 or 10 pounds weight to a square foot. Whence it seems reasonable to infer, that the momentous force is much superior to the simple pressure of the column IK; and therefore equal to a greater resistance, if required, than a pressure of 4 or 5 feet perpendicular height. It seems necessary further to observe, that the consumption of water in the kitchen offices is very considerable; that is, that water is frequently drawing from morning till night all the days of the year.

XXV. On Occultations of Stars and Geometrical Theorems. Being an Extract of a Letter from Mr. Lexel, to Dr. Morton. Dated Petersburg, June 14, 1774. p. 280.

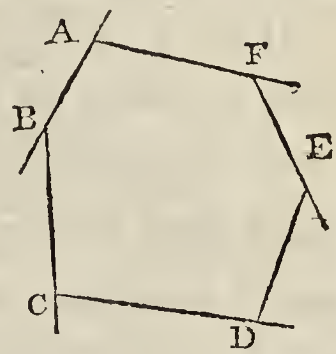
As I propose, says Mr. L., to make some researches concerning the difference of the meridians of the principal Observatories of Europe, which I am persuaded can best be ascertained by the occultations of the fixed stars by the moon; it would be of great service to me to be furnished with the observations that have been made, or that will be made, this year, of the occultations of α or of γ Tauri by the moon. I beg therefore Sir, you will please to desire Mr. Maskelyne to communicate them to me, towards the beginning of the next year, directed to Mr. Euler, secretary of our Academy. It would also be of great use to me to have the observation of the occultation of the Pleiades by the moon the 15th of March, 1766, in case it has been taken at Greenwich. The following are some observations of Mr. Wargentin, of the occultations of α and γ Tauri.

1773, Nov. 1....	11^h	56^m	12^s	....Emersion of α, uncertain to some seconds.	
1774, Jan. 22....	6	0	26½	....Immersion of the eye of ♉,	both very certain.
....	7	15	51	....Emersion,	
Feb. 18....	6	39	51	....Immersion of γ, very certain.	
	7	19	33	....Emersion, within 2 seconds.	

The following are my observations.

1773, Nov.	1....	12h	56m	47s	Emersion of α almost certain; the immersion was not observed on account of clouds.
1774, Jan.	22....	7	2	52........	Immersion, } both certain.
		8	20	44........	Emersion, } both certain.
April	14....	8	28	34........	Immersion of α, very certain.
		9	3	20........	Emersion of the same.
	15....	9	32	0........	Immersion of Flamstead's 115 in ♉.
	16....	10	21	31........	Immersion of a star of the 6th magnitude in ♊.
May	22....	13	2	20........	Immersion of m Virginis, very certain.

I have lately discovered two curious theorems, which I shall here communicate to the R. S.



Theorem.—Let A, B, C, D, E, F, be a polygon whose sides are named a, b, c, d, e, f; and the exterior angles $\alpha, \beta, \gamma, \delta, \varepsilon, \zeta$, so that the side a be placed between the angles α and β, b between β, γ, &c.

1. $a \times \sin.\alpha + b \times \sin.(\alpha + \beta) + c \times \sin.(\alpha + \beta + \gamma) + d \times \sin.(\alpha + \beta + \gamma + \delta) + e \times (\sin.\alpha + \beta + \gamma + \delta + \varepsilon) + f \times \sin.(\alpha + \beta + \gamma + \delta + \varepsilon + \zeta) = 0.$

2. $a \times \cos.\alpha + b \times \cos.(\alpha + \beta) + c \times \cos.(\alpha + \beta + \gamma) + d \times \cos.(\alpha + \beta + \gamma + \delta) + e \times \cos.(\alpha + \beta + \gamma + \delta + \varepsilon) + f \times \cos.(\alpha + \beta + \gamma + \delta + \varepsilon + \zeta) = 0.$

In fact it is $\sin.(\alpha + \beta + \gamma + \delta + \varepsilon + \zeta) = \sin. 360° = 0$, and $\cos.(\alpha + \beta + \gamma + \delta + \varepsilon + \zeta) = +1$; but in order to give the same form to the two expressions, I rather chose to represent them as I have done. By means of these two theorems the solution of polygons will be as easy as that of triangles by common trigonometry.

XXVI. Investigation of a General Theorem for finding the Length of any Arc of any Conic Hyperbola, by Means of two Elliptic Arcs. With some other New and Useful Theorems deduced from it. By J. Landen, F.R.S. p. 283.

1. From the theorem noticed in art. 1 of the author's paper in the Philos. Trans., 1771, (p. 150, of this abridged vol.) it follows, that in the hyperbola AD (pl. 12, fig. 11), if the semi-transverse axis AC be $= m - n$; the semi-conjugate $= 2\sqrt{mn}$; and the perpendicular CP, from the centre C on the tangent DP, $= \sqrt{(m-n)^2 - t^2}$; the difference DP — AD, between the said tangent DP and the arc AD, will be equal to the fluent of $\sqrt{\frac{(m-n)^2 - t^2}{(m+n)^2 - t^2}} \times \dot{t}$.

2. It is well known, that in any ellipsis whose semi-transverse axis is m, and semi-conjugate n; if x be the abscissa, measured from the centre on the transverse axis, and z the arc between the conjugate axis and the ordinate corresponding to x, $\sqrt{\frac{m^2 - gx^2}{m^2 - x^2}} \times \dot{x}$ will be $= \dot{z}$, g being $= \frac{m^2 - n^2}{m^2}$.

Hence, $\sqrt{\frac{(m+n)^2 - t^2}{(m-n)^2 - t^2}} \times \dot{t}$ being $= \sqrt{\frac{(m+n)^2 - t^2}{(m+n)^2 - (\frac{m+n}{m-n})^2 t^2}} \times \frac{m+n}{m-n}\dot{t}$, it ap-

pears, that in the ellipsis aed (fig. 12) whose semi-transverse axis cd is $= m + n$, semi-conjugate ca $= 2\sqrt{mn}$, and abscissa cb (corresponding to the ordinate be) $= \frac{m+n}{m-n}t$; the arc ae is equal to the fluent of $\sqrt{\frac{(m+n)^2 - t^2}{(m-n)^2 - t^2}} \times \dot{t}$.

3. In the ellipsis aefd (fig. 13), the semi-transverse axis cd being $= m$; the semi-conjugate ca $= n$; and the abscissa cb (corresponding to the ordinate be) $= x$; if ep, the tangent at e, intercepted by a perpendicular (cp) drawn to it from the centre c, be denoted by t; $gx \times \sqrt{\frac{m^2 - x^2}{m^2 - gx^2}}$ (as is well known) will be $= t$, g being as in the preceding article.

Hence $x^2 = \frac{m^2g + t^2}{2g} - \sqrt{\frac{(m^2-n^2)^2 - 2\times(m^2+n^2)\times t^2 + t^4}{2g}}$. From which equation, by taking the fluxions, we have, $x\dot{x}$ $= \frac{t\dot{t}}{2g} + \frac{(m^2+n^2)\times t\dot{t} - t^3\dot{t}}{\sqrt{[2g(m^2-n^2)^2 - 2\times(m^2+n^2)\times t^2 + t^4]}} = \frac{t\dot{t}}{2g} + \frac{(m^2+n^2)\times t\dot{t} - t^3\dot{t}}{\sqrt{[2g((m-n)^2 - t^2\times(m+n)^2 - t^2)]}}$. But $\dot{z}$ being $= \sqrt{\frac{m^2 - gx^2}{m^2 - x^2}} \times \dot{x}$, as observed in the preceding article, it appears that $\frac{g}{t} \times x\dot{x}$ is $= \dot{z}$. It is obvious therefore that $\dot{z}$ is $= \frac{1}{2}\dot{t} + \frac{1}{2} \times$ $\frac{(m^2+n^2)^2\times\dot{t} - t^2\dot{t}}{\sqrt{[((m-n)^2 - t^2)\times((m+n)^2 - t^2)]}} = \frac{1}{2}\dot{t} + \frac{1}{4}\times\frac{(m-n)^2\times\dot{t} - t^2\dot{t}}{\sqrt{[((m-n)^2 - t^2)\times((m+n)^2 - t^2)]}}$ $+ \frac{1}{4}\times\frac{(m+n)^2\times\dot{t} - t^2\dot{t}}{\sqrt{[((m-n)^2 - t^2)\times((m+n)^2 - t^2)]}} = \frac{1}{2}\dot{t} + \frac{1}{4}\sqrt{\frac{(m-n)^2 - t^2}{(m+n)^2 - t^2}} \times \dot{t} + \frac{1}{4}$ $\sqrt{\frac{(m+n)^2 - t^2}{(m-n)^2 - t^2}} \times \dot{t}$. Whence, taking the fluents by the theorems in art. 1 and 2, we have z = ae (fig. 13) $= \frac{1}{2}t + \frac{\text{DP} - \text{AD}}{4}$ (fig. 11) $+ \frac{\text{ae}}{4}$ (fig. 12); consequently the hyperbolic arc AD is = DP + ae + $2t$ − 4ae. Thus, beyond my expectation, I find, that the hyperbola may in general be rectified by means of two ellipses.

Writing E and F for the quadrantal arcs ad, ad (fig. 12 and 13) respectively, and L for the limit of the difference DP − AD, while the point of contact (D) is supposed to be carried to an infinite distance from the vertex A of the hyperbola (fig. 11), we find 2F − E = L, the value of ae being $= \frac{1}{2}$F $+ \frac{1}{2}m - \frac{1}{2}n$ when t is $= m - n$; that is, when e coincides with d (fig. 12), and P with c (fig. 11), by what I have proved in the before-mentioned paper, art. 10.

4. From what is done above, the following useful theorems are deduced.

Theorem 1. The fluent of $\frac{1}{2}a^{\frac{1}{2}}z^{-\frac{1}{2}}\dot{z}\sqrt{\frac{\frac{b^2}{a} + z}{a - z}}$ is = de.

Theorem 2. The fluent of $\frac{1}{2}a^{\frac{1}{2}}z^{-\frac{1}{2}}\dot{z} \times \sqrt{\frac{a - z}{\frac{b^2}{a} + z}} = (\frac{2a^2}{b^2} + 1)\,\text{de} - (\frac{2a^2}{b^2} + 2)\,\text{ef}$.

Theorem 3. The fluent of $\frac{\frac{1}{2}a^{\frac{1}{2}}z^{\frac{1}{2}}\dot{z}}{\sqrt{(b^2 + 2kz - z^2)}}$ = 2ef − de = 2F − E + AD − DP.

Theorem 4. The fluent of $\frac{\frac{1}{2}a^{-\frac{1}{2}}b^2z^{-\frac{1}{2}}\dot{z}}{\sqrt{(b^2 + 2kz - z^2)}}$ = 2 (de − ef.) N.B. $k = \frac{a^2 - b^2}{2a}$.

These theorems still refer to fig. 11, 12, 13; but now the values of the several lines in them (being not as before) are as here specified, viz. Fig. 11, in the hyperbola AD, the semi-transverse axis AC is now $= a$; the semi-conjugate $= b$; the perpendicular CP, from the centre C on the tangent DP, is $= \sqrt{az}$, the said tangent DP $= \sqrt{\frac{a}{z}} \times (b^2 + 2kz - z^2)$; and the abscissa CB (corresponding to the ordinate BD) is $= a\sqrt{\frac{a}{z}} \times \sqrt{\frac{az + b^2}{a^2 + b^2}}$.

Fig. 2. In the ellipses aed, the semi-transverse axis cd is $= \sqrt{(a^2 + b^2)}$; the semi-conjugate ca $=$ b; the abscissa cb $=$ $\sqrt{\frac{a^2 + b^2}{a}} \times \sqrt{(a - z)}$; and the ordinate be $= b\sqrt{\frac{z}{a}}$.

Fig. 13. In the ellipsis aefd, the semi-transverse axis cd is $= \frac{1}{2}\sqrt{(a^2 \times b^2)} + \frac{1}{2}a$; the semi-conjugate ca $= \frac{1}{2}\sqrt{(a^2 + b^2)} - \frac{1}{2}a$; the tangents ep, fq, intercepted by perpendiculars (cp, cq) drawn to them from the centre c, each $= \sqrt{a} \times (a - z)$; and the abscissa (cb′ or cb″) on cd, corresponding to the point e or f, of the curve is determined by the expression $\frac{\sqrt{[\sqrt{(a^2+b^2)} + a - z \mp \sqrt{(z^2 + \frac{b^2}{a}z)}]}}{\sqrt{2} + \sqrt{(a^2 - b^2)}} \times$ cd.

The quadrantal arc ad (fig. 12) is denoted by E; and the quadrantal arc ad (fig. 13) is denoted by F; L the limit of DP — AD (fig. 11) is $=$ 2F — E.

From what is now done, I might proceed to deduce many other new theorems, for the computation of fluents; but I shall at present decline that business: and, after giving a remarkable example of the use of theorem 4, in computing the descent of a heavy body in a circular arc, conclude this paper with a few observations relative to the contents of the preceding articles.

5. Let lpqn (fig. 14) be a semi-circle perpendicular to the horizon, whose highest point is l, lowest n, and centre m. Let ps, qt, parallel to the horizon, meet the diameter lmn in s and t: and let the radius lm (or mn) be denoted by r, the height ns by d; and the distance st by x. Then, putting h for ($16\frac{1}{12}$ feet) the space a heavy body, descending freely from rest, falls through in one second of time; and supposing a pendulum, or other heavy body, descending by its gravity from p, along the arc pqn, to have arrived at q; the fluxion of the time of descent will be $= \frac{\frac{1}{2}rh^{-\frac{1}{2}}x^{-\frac{1}{2}}\dot{x}}{\sqrt{[2rd - d^2 - 2(r - d)(d - x)]}}$. The fluent of which, or the time of descent from p to q is, by theorem 4 of the preceding article, $= \frac{2r}{\sqrt{h} \times (2r - d)} \times$ de — ef; a (in that theorem) being taken $= \sqrt{d}$, $b = \sqrt{(2r - a)}$, cb (fig. 2) $= \sqrt{\frac{2r}{d}} \times \sqrt{(d - x)}$, and ep, fq, (fig. 13) each $= \sqrt{(d - x)}$. Hence it appears, that the whole time of descent from p to n is $= \frac{2r}{\sqrt{h} \times (2r - d)} \times$ (E — F); when, in fig. 12 and 13, the semi-axes are taken according to the values of a and b just now specified.

6. If pqn be a quadrant; that is, if d be $= r$, the whole time of descent from p to n will be $= \frac{2}{\sqrt{h}} \times (\text{E} - \text{F})$, by the above theorem. Which time, by what I have shown in the Philos. Trans. for 1771, is $= \frac{1}{\sqrt{h}} \times (\frac{1}{2}\text{E} + \frac{1}{2}\sqrt{\text{E}^2 - 2c})$, c being $\frac{1}{4}$ of the periphery of the circle whose radius is r. Consequently, $\frac{2}{\sqrt{h}} \times (\text{E} - \text{F})$ being found $= \frac{1}{\sqrt{h}} \times (\frac{1}{2}\text{E} + \frac{1}{2}\sqrt{\text{E}^2 - 2c})$, we find from that equation $\text{F} = \frac{3}{4}\text{E} - \frac{1}{4}\sqrt{\text{E}^2 - 2c}$, where E is the quadrantal arc of the ellipsis, whose semi-transverse and semi-conjugate axes are $\sqrt{2r}$ and $\sqrt{r}$; and F the quadrantal arc of another ellipsis, whose semi-transverse and semi-conjugate axes are $\sqrt{\frac{r}{2}} + \frac{1}{2}\sqrt{r}$ and $\sqrt{\frac{r}{2}} - \frac{1}{2}\sqrt{r}$.

Before Mr. Maclaurin published his Treatise of Fluxions, some eminent mathematicians imagined that the elastic curve could not be constructed by the quadrature or rectification of the conic sections. But that gentleman has showed in that treatise, that the said curve may in every case be constructed by the rectification of the hyperbola and ellipsis; and he has observed that, by the same means, we may construct the curve along which, if a heavy body moved, it would recede equally in equal times from a given point. Which last mentioned curve Mr. James Bernoulli constructed by the rectification of the elastic curve, and Mr. Leibnitz and Mr. John Bernoulli by the rectification of a geometrical curve of a higher kind than the conic sections. It is observable, that Mr. Maclaurin's method of construction just now adverted to, though very elegant, is not without a defect. The difference between the hyperbolic arc and its tangent being necessary to be taken, the method always fails when some principal point in the figure is to be determined; the said arc and its tangent then both becoming infinite, though their difference be at the same time finite. The contents of this paper properly applied, will evince that both the elastic curve, and the curve of equable recess from a given point, with many others, may be constructed by the rectification of the ellipsis only, without failure in any point.

XXVII. Astronomical Observations made at Chislehurst, in Kent, in the Year 1774. By the Rev. Francis Wollaston, LL.B., F. R. S. p. 290.

Mr. W. having now completed his original design, and kept his clock going for a 3d year, without the least touch of the oil, or any alteration whatever, he presumes the result of his observations to ascertain the rate of its going, may not be an unacceptable addition to the former papers on that subject, delivered to the Society. The regular difference between the summer and winter months, and some degree of similarity between those differences, seems to show a regularity in the cause. What that may be, is not fully to be ascertained by these observations; though it seems to have been difference of moisture, rather than

of heat. By comparing these last 3 years with that first given, when the clock was in some degree foul, it seems as if it were most affected when the work is clean. Though that is not quite certain; for the differences, which decreasing gradually in the following table, would justify this conclusion, it may be observed increase again in the last instance. For hence it appears that July and August are the months for greatest acceleration, and Jan. and Feb. for retardation; contrary to the affection of metalline rods, but agreeable to the effect to be expected from moisture on wood. Yet this difference is not so great in any degree, nor (what is more material to observation) by any means so sudden in its changes, as what is occasioned by heat on metals. And even this perhaps might be obviated by a strong coat of varnish on the rod, or some preparation of the wood itself. One thing it may be proper to mention, as an accidental experience Mr. W. had the last year; that a clock so fixed, with a pendulum of so simple construction, is not easily affected by any tremulous motion of the building to which it is fastened. In the months of March, April, and part of May, he had occasion to make alterations in the top of his house, in order to gain more rooms in it; and notwithstanding the great jarring necessarily consequent on taking off the old rafters, and laying on a new leaded roof, and new joists and floor over the observatory itself, the clock seemed not to have been disordered at all by it.

XXVIII. Of Triangles described in Circles and about them. By John Stedman, M.D. p. 296.

Prop. 1. *An equilateral triangle inscribed within a circle is larger than any other triangle that can be inscribed within the same circle.*—Let ABC, fig. 15, pl. 12, be an equilateral triangle, inscribed in the circle ADCB; and let ADE be a triangle supposed larger than ABC. Let ADE be drawn with one of its angles at the same point with one of the angles of the equilateral triangle, suppose at A, and then its other two angles will fall either on the segments ADB and AEC, or one of the angles on the segment BC. First, let one of its angles fall at D, between A and B; and the other at E, between A and C; and draw the line BE. In the triangles ABC, ABE, the triangle ABF is common, and the two remaining triangles BFC, AFE, are similar; for the angle AFE is equal to its opposite angle BFC; and the two angles EAC, EBC, are equal, being subtended by the same segment EC, and so the two remaining angles AEF, BCF, must be equal; therefore the sides are proportional, and BC and AE, subtending equal angles, must be homologous; but BC is equal to AC, which is greater than AE; consequently the triangle BFC is greater than AFE, and so the equilateral triangle ABC is greater than the triangle ABE. In the same manner, the triangle ABE may be proved greater than ADE; for AHE is common, and the two triangles ADH, BHE are similar, and their sides proportional; and AD and BE, subtending equal

angles, must be homologous; but BE is greater than BC, which is equal to AB, and that again greater than AD; consequently BE is greater than AD, and the whole triangle AEB greater than AED; and so the equilateral triangle must, à fortiori, be greater than AED. Q.E.D.

Next, let the triangle ADE be supposed greater than the equilateral triangle ABC; and let the angle ADE fall somewhere in the segment BDC, (fig. 16,) so that the segment EC may be greater than BD; for if it were not, the angle AED being applied to any of the angles of the equilateral triangle, the demonstration would become the same as in the first case: therefore the segments AEC, BDC, being equal, and BD being less than EC, AE must be less than DC. Draw the right line DC; then, in the two triangles ADC, ADE, the triangle AFD is common, and the two triangles AFE, DFC are equiangular and similar, and the sides AE, DC, subtending equal angles, are homologous; but DC is greater than AE; so the triangle DFC is greater than the triangle AFE, and the whole triangle ADC is greater than ADE; but the equilateral triangle may be proved greater than ADC from the first case, and consequently greater than ADE. Q.E.D.

PROP. 2. *An equilateral triangle described about a circle is less than any other triangle that can be described about the same circle.*—Let the equilateral triangle ABC, fig. 17, be described about the circle HIK, and let the triangle BDG be supposed less than the equilateral triangle. Draw the line AF parallel to BC; then the triangles AFE, EGC, are similar; for the opposite angles AEF, GEC, are equal, as likewise the angle AFE to the angle EGC; the lines AF and GC being parallel, and falling on the same line FG, the angles AFE and EGC are therefore equal, and the sides AE, EC, subtending equal angles, are homologous; but the side of the equilateral triangle AC being equally divided at I, the line AE is greater than EC, and consequently the triangle AFE is larger than the triangle EGC; and the triangle DAE much larger than EGC: therefore, in the triangles DBG and ABC, the part ABGE being common, the whole triangle DBG is larger than the equilateral triangle. Q.E.D.

Whatever other triangles can be described about a circle, may be demonstrated to be larger than an equilateral triangle described about the same circle, on the same principles as the preceding.

PROP. 3. *The square of the side of an equilateral triangle, inscribed in a circle, is equal to a rectangle under the diameter of the circle, and a perpendicular let fall from any angle of the triangle on the opposite side.*—The two triangles ADC, AEC, fig. 18, are equiangular and similar, the angles ACD, AEC, being both right, and that at A common; therefore AD : AC :: AC : AE, and $AC^2 = AD \times AE$. Q.E.D.

The square of one side of the triangle being compleated, so as to include the triangle; then that part of the side of the square that falls within the circle is

equal to the radius; and the other part, lying without the circle, is equal to the radius minus twice the portion lying between the side of the square, and the circumference of the circle; or is equal to that part of the radius that lies between the centre and the side of the square minus the remainder of the radius; that is CL is equal to the radius, and LI = KG — 2MG; or LI = KM — MG. FG being parallel to BC, and consequently perpendicular to IC, must divide the chord LC in two equal parts; so that MC being equal to KE, LC must be equal to 2KE; but KE (by Eucl. l. xiii, pr. 12, cor. 2. Clav.) is equal to ED; therefore LC = KD the radius. The side of the square IC, being equal to BC, is likewise equal to NM; but LC being equal to KG, the remaining part LI must be equal to NK—MG; or to KM — MG. Q.E.D.

XXIX. On Polygons of the Greatest and Least Area, or Perimeter, inscribed in a Circle, or circumscribed about a Circle. By S. Horsley, LL.D., Sec. R.S. p. 301. *Translated from the Latin.*

Theorem 1. If a right line touch a circular arc intercepted by two tangents; then its segments intercepted by its point of contact and those tangents, will be equal or unequal, according as the arc is equally or unequally divided by the point of contact. And the greater or less segments of the arc (when unequally divided) and of the right line, lie on the same side of the dividing point.—Thus, if the right line BD, fig. 1 and 2, pl. 13, in the point E touch the circular arc AEC, intercepted by the two tangents AB, CD. Then the right line BD will be equally or unequally divided in the point E, accordingly as the arc AEC is equally or unequally divided at the same point E. So that, when AE = CE, then BE = DE, as in fig. 1; but when AE is greater than CE, then BE is greater than DE, as in fig. 2.

Theorem 2. Of all the right lines which touch a circular arc, and meet two other tangents at the extremities of the arc; that is the least which touches the arc in its middle point.—Thus, of all the lines, AC, GH, touching the arc BED, fig. 3, and intercepted by the two tangents BAG, DHC, that AC is the least which touches the arc in its middle point E.

Theorem 3. Of all the polygons, of a given number of sides, and circumscribing a given circle, the equiangular one has the least perimeter.

Theorem 4. Of all the polygons, having a given number of sides, and circumscribing a given circle, the equiangular one has the least area.

Theorem 5 and 6. Of all polygons, having a given number of sides, and inscribed in a given circle, the equilateral one has the greatest perimeter and area. All which theorems Dr. Horsley demonstrates with his usual geometrical rigour.

XXX. Of an extraordinary Acephalous Birth. By W. Cooper. M.D. p. 311.

Mrs. Brackett, of Clerkenwell-close, aged 23 years, was, at the end of her first pregnancy, by a natural labour, delivered of a perfect female child, on Friday the 8th of October, 1773, at 7 o'clock in the morning. The attending midwife, Mrs. Ayres, soon perceived by the abdominal tumour that there was another child. After waiting about 3 hours, a flooding came on; but without pain, or any advancement of the 2d delivery. The hæmorrhage producing faintness, debility, and danger, the attendants and midwife were alarmed, and Dr. C. was sent for. When he came, he found her in the situation above described; and therefore thought it his duty to accomplish the remaining part of the labour, as soon as he could, consistently with the safety of the mother. On all occasions, when the concomitant circumstances render it necessary to turn a child in utero, it is of the utmost consequence to understand, as nearly as we can, its general situation, in order to deliver with the greater ease, safety, and expedition. And to an experienced accoucheur, if the breech, knees, or feet, do not immediately present themselves, the head and face of the child will, in most cases, be a sufficient index to the position of the other parts of its body. This circumstance arises from the fœtus commonly coiling itself up into an oblong, oval, snug, compact figure, with its knees towards its chin, in order to take up as little room as possible, by being adapted to the cavity of the uterus. In the present case, when the patient was placed in a proper situation, having introduced his hand as gently as possible through the vagina, cervix uteri, and enveloping membranes, and no part of the inferior extremities or breech presenting itself, Dr. C. examined carefully for the head of the child, as usual, but without success. This disappointment somewhat embarrassed him. But as the woman's situation was become very serious by the increasing uterine hæmorrhage, he attempted without delay to get at the feet. He easily secured one of them; but though he made use of very little force in bringing it towards the os externum, the structure was so very tender that the tibia began to give way at its superior epiphysis. On this account he was reduced to the disagreeable necessity of again introducing his hand into the uterus; and as one leg had thus unexpectedly failed him, he thought it extremely futile to attempt any thing with the other. The most eligible resource which he apprehended he had now left, was to fix a blunt hook on one groin, and, when it was brought low enough, to assist gently at the other, with the 2 fore-fingers of his right hand. By these means he happily accomplished the delivery of the remaining fœtus, which proved to be a very singular kind of monster. And as the late ingenious Mr. Hewson injected its blood vessels, and dissected it, Dr. C. was enabled to attempt a short anatomical description of it, for the satisfaction of the curious in philosophy and physiology.

This extraordinary animal production was of the size and appearance of a common twin child at its full time, excepting the particularities now to be pointed out. When first born it was very plump, but soft and flabby, and the bones remarkably small and tender. It had neither head, neck, hands, nor arms .In the place where the neck should originate, was a little mammilla, somewhat larger than a woman's nipple, but quite soft. And on each side, in the place where the arm should begin, there was a small papilla, about the size, and very much like the extremity of a common quill. The spine seemed perfect, but ended abruptly at the upper vertebræ colli. Below the navel the parts were nearly entire, except the feet, where the toes were of an irregular form and size, and some of them united together. The external parts of generation, which indicated it to be a female, were also perfect. On a careful inspection internally, there was evidently no brain nor spinal marrow. A few nerves however were scattered about the abdomen; but their origin, through fear of destroying the preparation, was not traced. The uterus was perfect; but only one ovarium could be found. There was also the appearance of a bladder; but it was so contracted as to have no cavity. A large intestine arose from the anus; was a good deal convoluted above the brim of the pelvis, and ended in a blind pouch or cul de sac, on the left side of the abdomen. This viscus appeared to be about 6 or 7 inches in length, varied its size in different parts, gradually became smaller towards its superior extremity, and seemed fully distended with a colourless mucus.* All above the navel was extremely defective. There was no heart, lungs, diaphragm, stomach, liver, kidneys, spleen, pancreas, nor small intestines. However, there were 3 small glands in the place of the thymus, whose substance, when examined with a microscope, Mr. Hewson remarked, exactly resembled that of the thymus itself. And on each side of the vena cava, just under the navel, were 2 little glandular substances, which seemed to be somewhat like capsulæ renales, only very small to what are commonly found.† There was a large artery running on the spine, which might be called the aorta. As this approached the upper extremity of the little animal, it was divided into smaller and smaller branches; and in its course it distributed lateral ones also to the contiguous parts of the trunk. Below the navel it sent off 2 branches that

* Does not this circumstance almost amount to a proof, that the meconium, universally found in the bowels of new-born children, is nothing more than the mucus naturally secreted by the intestinal glands, mixed with bile, and perhaps a small portion of the pancreatic juice? In the present instance, as there was no liver there could be no bile, and consequently the meconium, if Dr. C. might so call it, was colourless.—Orig.

† Mr. Hewson, some time before his death, seemed to be confirmed in the opinion, that whenever children are born with little or no brain, the capsulæ renales are always very much diminished. This is certainly the case in 1 or 2 almost brainless children which Dr. C. had by him, and whose renal capsulæ he examined, with a view of being further satisfied on this subject.—Orig.

constituted the umbilical arteries, one of which was considerably larger than the other. And then below these, 2 other branches descend to the inferior extremities. A large umbilical vein came in at the navel, and was immediately divided into 2 considerable branches; one ascending, the other descending. Each of these was again subdivided into smaller and smaller branches, which, as they passed upwards and downwards, seemed to correspond with the different ramifications of the ascending and descending aorta. The funis umbilicalis was only about 2 inches in length,* and so very tender also, that it unavoidably separated near the navel of the child during the delivery. Whether therefore there was any pulsation in this short funis, he was not able to determine. The placenta was not particularly examined.

There were evidently in this fœtus 2 distinct systems of vessels, arteries and veins,† that carried red blood.‡ It was plain also, that the blood passed from the internal iliac arteries, through the hypogastrics and umbilicals to the placenta, and was returned from it by the umbilical vein to the navel, and thence distributed in the manner before observed. But as there was no heart, nor any thing analogous to one, it became extremely difficult to ascertain the powers by which the circulation was carried on through this physiological phenomenon. Might we not however venture to advance a conjecture, that the peristaltic, or living muscular power of the arteries, was principally subservient to this important end? Many examples are to be met with in the collections of the curious and learned in the different parts of Europe,§ which are somewhat similar to that now related. When carefully examined however, excepting a very few instances, they are generally found either essentially to differ, or else their structure has not been, with any tolerable precision, explained. The present history

* An exactly similar circumstance to this Dr. C. took particular notice of, in the delivery of another almost brainless monster.—Orig.

† Mr. Hewson attempted to inject the whole blood vessels by the umbilical vein as usual. To his great surprize, no part of the injection returned by the umbilical arteries. He could not account for this singularity at that time: but as only a part of the vessels were filled, he injected afresh by one of the hypogastric arteries. On dissection afterwards, this mystery was unravelled by the heart's being totally absent. It then appeared also, that by the first injection he had filled the venal system, and by the latter the arterial.—Orig.

‡ See a very curious case related by Mons. Winslow, in the Memoires de L'Academie des Sciences for 1740, p. 586 and 604. Among other remarkable singularities in this little monstrous abortion of 6 months, that excellent anatomist particularly takes notice, that there was no appearance of one drop of red blood in any of its vessels, which were universally filled with a serous lymph; and that there were no vestiges of any veins at all.—Orig.

§ F. Licetus, de Monstris, p. 300 et seq. Palfyn, Traite des Monstres, p. 325. Cheselden's Anatomy, 5th ed. p. 379. Phil. Trans., 1739-40, N° 456. Ibid. 1767, p. 1. L'Academie des Sciences, Hist. 1720, p. 13. Ibid. Mem. 1720, p. 8. Ibid. 1740, p. 585 and p. 592. Miscellania Curiosa Ephemeridum Germanicarum Ann. 19, p. 258. Acta Eruditorum Lipsiæ, Ann. 1724, p. 501.—Orig.

affords also an exception to a frequent remark among authors, "That brainless children are always very brisk before they are born;"* for the mother has frequently said, "That she felt no motion at all within her after the first birth; and that she had not the least suspicion of there being a 2d child till it was delivered." This circumstance may however perhaps be attributed to the medulla spinalis being totally deficient, as well as the cerebrum and cerebellum.

Physiologists and philosophers have spent a great deal of time in attempting to investigate the causes of these extraordinary phenomena. With this view many opinions have been started; but most, if not all of them, as far as Dr. C. was able to judge, being built on the tottering basis of conjecture only, afford, on an attentive inspection, but little satisfaction to a dispassionate inquirer after truth. The particular hypothesis, which has been almost universally adopted, is, that monstrosity and marks in children depend on the imagination and longing of the mother. Such a pernicious principle as this ought to have very rational evidence, and the most striking facts to support it. But is it not directly the contrary? Indeed a great many ridiculous stories have been related to the world,† which however on a little reflection either obviate themselves, or else are contradicted by those facts that occur. May we not exemplify this observation by the case of twins now related? One of the children was perfect, and is still living; the other proves to be remarkably defective. Does not the question naturally arise here, how could one child be affected by the disturbed imagination of the mother, and the other not? But the mother, on repeated examination, recollects no fright in particular while she was pregnant. Neither, if she did, would it at all invalidate the force of our argument on this subject; for she could not possibly see any child without a head: and more especially, because other parts, as the viscera and medulla spinalis, were equally defective, which are entirely out of the reach of the eye or imagination of the mother to form any idea about them. To elucidate this point still further, can any candid person possibly suppose, that the casual agitation of mind of a pregnant woman, should either produce or destroy a whole system of blood vessels, nerves, and fibres, which are indispensable constituents of almost every part of the body? And may we not adduce one proof more, in support of our argument, from what happens to animals and vegetables? Among these also, such extraordinary deviations from the general course of nature are by no means uncommon: yet the former are possessed of a much less share of imagination than is generally allotted to the human species; and the latter have none at all. Reasoning in

* Phil. Trans. 1674, No. 99, p. 6157. Ibid. 1767, p. 18.—Orig.

† Mauriceau, p. 53, obs. 64. Ibid. p. 63, obs. 118. Smellie's Midwifry, vol. 3, p. 402. Phil. Trans., 1684, N° 160, p. 599. Ibid. 1739-40, N° 456, p. 303 and 306.—Orig.

the same manner on several occasions of this kind in which he had been concerned, his conclusions had always been similar; viz. that the usually assigned cause of the mother's imagination is by no means equal to the manifold effects produced. And on the other hand, this injurious doctrine is pregnant with continual mischief to society. It frequently makes women very unhappy. And the fear of mutilating or marking their infants often affects them so much, that they at last miscarry. Having therefore indubitable facts to go on, and the cause of humanity so powerfully coinciding with the truth, is it not right to affirm and maintain with confidence, that neither the longing nor frighted imagination of the mother appears to have any power at all to imprint marks or monstrosity on children? That this is a very weak supposition, entirely void of foundation, directly contrary to all philosophy and experience, and has nothing to support it but a vulgar opinion, transmitted to us from the ages of anatomical ignorance? And is it not more reasonable to conclude, as Dr. Hunter in his lectures has done, that whatever be the defect or deformity in a monstrous birth, it can never be occasioned by accidents of any kind during pregnancy; but probably has its existence always originating, causâ adhuc incognitâ, in the first stamina of the embryo.*

Thus have been faithfully related the particulars of this singular phenomenon among the human species, which, to a demonstration, confirm Dr. Hunter's opinion, that the nourishment of the fœtus in utero is principally by means of the funis umbilicalis. M. Merry observes, that defective monsters are more instructive than others that have redundancies.† If this be true, here is still an ample field for speculation, notwithstanding the few very obvious remarks which Dr. C. already ventured to make. In conformity to the general language of authors, he had in this essay occasionally adopted the use of the term monster: there is however something in that word extremely repugnant to our common feelings, and very apt to leave a terrifying impression on the mind. Why may not the Author of Being sometimes produce variations in the human species, as well as in the animal and vegetable kingdoms,‡ and equally exempt too from such frightful appellations? Would it not therefore be more eligible in the present instance, and every similar one, to explode the common term, and call it simply a lusus naturæ; or with Pliny to say, "Hoc nobis miraculum, sibi ludibrium, ingeniosa finxit natura."

* Baron Haller is of opinion also, that this is evidently the case in that species of monsters to which parts are added. Vide Opera Minora Halleri, tom. 3, p. 148.—Orig.

† L'Académie des Sciences, Hist. 1720, p. 13.—Orig.

‡ See F. Licetus., J. Palfyn des Monstres, &c. in which are many instances of each kind.—Orig.

XXXI. Observations on the State of Population in Manchester, and other adjacent Places, concluded. By Thomas Percival, M. D., F. R. S., and S. A. p. 322.

Reprinted in Dr. Percival's collected works recently, 1807, edited by his son, in 4 vols. 8vo. We shall therefore only observe here, that from these tables it appears, that the proportion of males to females baptized, is nearly as 12 to $11\frac{1}{3}$, or 19 to 18; but that the number of females living is to the number of males as about 11 to $10\frac{1}{5}$, or more exactly as 14 to 13; and that the widows are almost double the number of widowers.

XXXII. On the Effects of Lightning on a House, which was furnished with a Pointed Conductor, at Tenterden, in Kent. In Two Letters from Richard Haffenden, Esq., the Proprietor of the House, to Mr. Henley. To which are added some Remarks by Mr. Henley. p. 336.

This was an oblong house, about 50 feet long and 30 broad, having 4 stacks of chimnies, 2 at each end, to one stack of which was fixed a pointed iron conductor, projecting 5 feet above the top of the chimney. The lightning struck the chimney diagonally opposite to and the farthest from that of the conductor, and in its passage injured several parts of the building, where the conductors were imperfect or discontinued. On the account given of the house and the accident, &c. Mr. Henley remarks that, 1st, A sharp pointed conductor did not, in this instance, invite or draw down on itself a stroke of lightning. 2dly, Such a conductor, elevated 5 feet above the top of the chimnies, to a house of this dimension, may not perhaps be sufficient, by its silent attractive force, to protect the whole of such a building from a stroke; especially when a chimney, a blunt body, wetted with the rain, standing at 50 feet distance from the conductor, and being within 5 feet of its height, is in actual contact with so large, though irregular, a communication of metal, leading from the chimney directly to the conductor; though, in this instance, it should be remarked, that the conductor itself was not in contact throughout; and it is, for that reason, a very exceptionable case. 3dly, Two such conductors; one, for instance, on the chimney where this was placed; and the other on the chimney which was stricken, with a communication of lead between them, would probably have protected the house: but a conductor on each chimney would certainly have secured the whole building effectually. 4thly, As the 3 branches, or divisions of the lightning, all concentrated on an iron bar, three quarters of an inch square, and produced no sign of heat in it, an iron bar of that size seems to be fully sufficient for the purpose. There appears however to have been 2 defects in Mr. Haffenden's conductor: 1. The leaden pipe and the iron bar at the bottom were not in contact. 2. The iron bar, or a thick plate of lead,

should have been continued down into the moist earth or water; and had not the earth, as Mr. Haffenden observes, flowed with water, at the time of the accident, the want of this precaution might perhaps have been attended with some damage to the foundation. In Mr. Haffenden's 2d letter, he observes, that the bell wire, mentioned in his first letter, was brass; and that so much of it as went through the passage painted: and the painted part, he says, was not destroyed; but the paint was loosened on the wire, without being broken off, like the loose rind of a tree; which resembles the effect of the artificial electricity, in an experiment of Mr. Kinnersley's, where a wire was, by a great explosion, both lessened in diameter, and extended in length. The other part of the wire, which was not painted, except a short piece at the end, somewhat larger and of iron, was entirely melted. Query, if the wire before spoken of had passed through a stone, particularly a wet one, inclosing it firmly, would not that stone have been shivered to pieces?

XXIII. On the Torpidity of Swallows and Martins. By James Cornish, Surgeon, Totness, Devon. p. 343.

In the beginning of November, Mr. C. being fishing on the banks of the river Dart, which runs at the bottom of a very steep hill, from the side of which project several large rocks, overgrown with ivy and thicket; he was at once surprised with the sight of a great number of martins. He desisted from his amusement, the more carefully to observe the birds, which he concluded had been brought out of their winter quarters by the fineness of the afternoon, it being remarkably pleasant and warm for the time of the year; the sun at that time darting its rays directly against the rocks, just opposite to which Mr. C. had fixed his station. They continued to flit to and fro for near half an hour, keeping very near together, and never flying in a direct line above 30 or 40 yards, and never, when at the farthest, above 100 yards distant from the rocks; closer to which they now, as the sun lowered, began to gather very fast. Their numbers now lessened considerably; and in a very short time they all returned into the fissures of the rocks, whence they had been induced to venture out by the warmth of the evening. Mr. C. was particularly careful to observe if there was a swallow among them; but there was not one. Of this he was certain; for they were several times within the distance of 20 yards from the places where he stood. He was the more attentive to this, as he had been repeatedly assured, by many masters of vessels in the fish trade, that they constantly saw every autumn, as they sailed up the Mediterranean, vast flights of swallows, bending their course towards the south. From which there is the strongest reason to believe, that these birds seek a warmer climate during the winter months;

though Mr. Buffon has left that point undetermined. The above account Mr. C. thinks settles the question, relative to martins.

*XXXIV. Description and Use of a portable Wind Gage. By James Lind, M. D., Edinburgh.** p. 353.

This simple instrument consists of 2 glass tubes AB, CD, of 5 or 6 inches in length, pl. 13, fig. 4. Their bores, which are so much the better always for being equal, are each about $\frac{4}{10}$ of an inch in diameter. They are connected together, like a siphon, by a small bent glass tube ab, the bore of which is $\frac{1}{10}$ of an inch in diameter. On the upper end of the leg AB there is a tube of latten brass, which is kneed or bent perpendicularly outwards, and has its mouth open towards F. On the other leg CD is a cover, with a round hole G in the upper part of it, $\frac{2}{10}$ of an inch in diameter. This cover and the kneed tube are connected together by a slip of brass cd, which not only gives strength to the whole instrument, but also serves to hold the scale HI. The kneed tube and cover are fixed on with hard cement or sealing wax. To the same tube is soldered a piece of brass e, with a round hole in it, to receive the steel spindle KL, and at f there is just such another piece of brass soldered to the brass hoop gh, which surrounds both legs of the instrument. There is a small shoulder on the spindle at f, on which the instrument rests, and a small nut at i, to prevent it from being blown off the spindle by the wind. The whole instrument is easily turned round on the spindle by the wind, so as always to present the mouth of the kneed tube towards it. The end of the spindle has a screw on it; by which it may be screwed into the top of a post, or a stand made on purpose. It has also a hole at L, to admit a small lever for screwing it into wood with more readiness and facility. A thin plate of brass k is soldered to the kneed tube, about half an inch above the round hole G, so as to prevent rain from falling into it. There is likewise a crooked tube AB, fig. 5, to be put occasionally on the mouth of the kneed tube F, in order to prevent rain from being blown into the mouth of the wind gage, when it is left out all night, or exposed in the time of rain.

The force or momentum of the wind may be ascertained by the assistance of this instrument, by filling the tubes half full of water, and pushing the scale a little up or down, till the 0 of the scale, when the instrument is held up perpendicularly, be on a line with the surface of the water, in both legs of the wind gage. The instrument being thus adjusted, hold it up perpendicularly, and turning the mouth of the kneed tube towards the wind, observe how much the water is depressed by it in the one leg, and how much it is raised in the other. The sum of the two is the height of a column of water which the wind is capable of sustaining at that time; and every body that is opposed to that

* Now of Windsor.

wind, will be pressed on by a force equal to the weight of a column of water, having its base equal to the surface that is opposed, and its height equal to the altitude of the column of water sustained by the wind in the wind gage. Hence the force of the wind on any body, where the surface opposed to it is known, may be easily found; and a ready comparison may be made between the strength of one gale of wind and that of another, by knowing the heights of the columns of water, which the different winds were capable of sustaining. The heights of the columns in each leg will be equal, provided the legs are of equal bores; but unequal, if their bores are unequal. For suppose the legs equal, and the column of water the wind sustains to be 3 inches, the water in the leg, which the wind blows into, will be depressed $1\frac{1}{2}$ inch below 0, and raised just as much above it in the other leg. But if the bore of the leg which the wind blows into, be double that of the other, the water in that leg will be depressed only 1 inch, while it is raised twice as much, or 2 inches, in the other; and vice versa, if the same wind blows into the smaller leg, it will depress the water in it 2 inches, while it raises it only 1 inch in the other. The force of the wind may be likewise measured with this instrument, by filling it till the water runs out of the hole G. For if we then hold it up to the wind as before, a quantity of water will be blown out; and, if both legs of the instrument are of the same bore, the height of the column sustained, will be equal to double the column of water in either leg, or the sum of what is wanting in both legs. But if the legs are of unequal bores, neither of these will give the true height of the column of water which the wind sustained. But the true height may be obtained by the following formulæ.

Suppose that after a gale of wind, which had blown the water in one of the tubes from A to B, fig. 6, forcing it at the same time through the other tube out at E, the surface of the water should be found standing at some level DG, and it were required to know what was the height of the column EF or AB, which the wind sustained. In order to obtain which, it is only necessary to find the height of the columns DB or GF, which are constantly equal to each other: for either of these added to one of the equal columns AD, EG, will give the true height of the column of water which the wind sustained.

Case 1. Let the diameters AC, EH, of the tubes be respectively represented by c, d; and let a = AD or EG, and x = DB or GF. Then it is evident that the column DB, is to the column EG, as c^2x to d^2a. But these columns are equal. Therefore $c^2x = d^2a$; and consequently $x = \frac{d^2a}{c^2}$.—Example. If the diameters AC, EH, be respectively 10 and 1, and AD or EG = 3.96 inches, x will be = .0396 of an inch. For $d^2a = 1 \times 3.96 = 3.96$, which divided by $c^2 = 100$, gives $x = .0396$.

Case 2. But if at any instant of time, while the wind was blowing, it was

observed, that when the water stood at E, the top of the tube out of which it is forced, it was depressed in the other tube to some given level BF, the altitude at which it would have stood in each, had it immediately subsided, may be found in the following manner. Let b = AB or EF. Then it is evident, that the column DB is equal to the difference of the columns EF, GF. But the difference of these columns is as $d^2b - d^2x$. Therefore $c^2x = d^2b - d^2x$; and consequently, $x = \frac{d^2b}{c^2 + d^2}$.

For the cases when the wind blows in at the narrow leg of the instrument. Let AB = EF = b, EG or AD = a, GF = DB = x, and the diameters EH, CA, respectively = d, c, as before. Then it is evident that the column AD, is to the column GF, as ac^2 to d^2x. But these columns are equal. Therefore $d^2x = ac^2$; and consequently $x = \frac{ac^2}{d^2}$. This answers to case 1. It is also evident, that the column AD is equal to the difference of the columns AB, DB. But the difference of these columns is as $bc^2 - c^2x$. Therefore $d^2x = bc^2 - c^2x$. Whence we get $x = \frac{bc^2}{d^2 + c^2}$. This corresponds to case 2.

As there is always a calculation to be made for every experiment when the legs of the instrument are of unequal bores, Dr. L. recommends it to the makers of these instruments to choose tubes that are equal, or at least nearly so, that the error may become next to nothing, it being a thing very easy to be done. In this manner we can readily determine the greatest force, with which the wind has blown during the time the instrument has been exposed to its action. But as it may be safely left alone, by screwing its spindle into the proper stand, or into the top of a post, and as the wind never fails to turn the mouth of it towards itself, it is not necessary for the observer to continue always by it; for it may be allowed to stand all night, exposed to the wind, without any inconvenience, though it should even happen to rain very heavily. However, recourse can only be had to this method of using the instrument on shore; for at sea it must always be held up in a perpendicular position in the hand, whether it be used when only half full of water, or when quite full; which last will be frequently found to be the only practicable method of ascertaining the force of the wind during the night, when it blows so hard that it is impossible to keep any lights on deck.

A person filling the wind-gage, in a calm place, with water, in order to determine the force of the wind, in the way just described, will be apt to imagine, that it cannot give the measurement correct; for he will find such a repulsion to arise from the edges of the hole G, as to sustain a column of water in the kneed or bent tube, perhaps half an inch above the level; but by either blowing across the round hole, or moving his finger over it, he will soon bring the water in the kneed tube to stand at the same level with it, by taking off gradually the convex surface of the water, which projects out at the hole in the form of a

drop or spherule; and this effect the wind very soon produces itself. There ought always to be a cover on the top of the tube, out of which the water is expelled by the wind; but it should be made very thin. For if there be no such cover, and the mouth of the kneed tube be stopped, after the instrument is quite full of water, in order to prevent the wind from having any influence in raising it, you will find, on exposing it to a strong gale, that in a very short time it will blow out perhaps half an inch of water. Whence it appears, that a very considerable error would arise from using the wind-gage in this state. The use of the small tube of communication ab (fig. 4) is to check the undulation of the water, so that its height may be read off from the scale with ease and certainty. But it is particularly designed to prevent the water from being thrown up to a much greater or less altitude, than the true height of the column, which the wind is able at that time to sustain, from its receiving a sudden impulse, while it is vibrating either in its ascent or descent. For water in the legs of a siphon is capable of being put into a vibrating motion like a pendulum; and therefore, if acted on when in the ascent, the height which it ascends to will come out greater than the truth, and less, if acted on in the descent.

The height of the column of water sustained in the wind-gage being given, the force of the wind on a foot square is easily had by the following table, and consequently on any known surface.

TABLE I.

Height of the water in the wind-gage.	Force of the wind on the foot square in avoirdupois pounds.	Common designation of such a wind.
12 inches	62.5	
11	57.293	
10	52.083	most violent hurricane.
9	46.875	
8	41.657	very great ditto.
7	36.548	great hurricane.
6	31.75	hurricane.
5	26.041	very great storm.
4	20.833	great ditto.
3	15.625	storm.
2	10.416	very high wind.
1	5.208	high wind.
$0\frac{1}{2}$	2.604	brisk gale.
$0\frac{1}{10}$	.521	fresh breeze.
$0\frac{1}{20}$	.260	pleasant wind.
$0\frac{1}{40}$	.130	a gentle wind.

It may be sometimes necessary to employ other fluids besides water, particularly if the degree of cold be below freezing; for then we must use a fluid that will not freeze in the degree of cold in which we expose the instrument, otherwise the wind can have no influence on it, and the liquor freezing in the tube will break it. Dr. L. therefore mentions a few liquors in the following table that will answer the purpose, as also subjoins a general method of reducing them all to one common measure. But of all the fluids he was acquainted with, when the effects of frost are to be feared, he knew none better adapted to the purpose than a saturated solution of sea-salt; since it does not freeze till the thermometer falls to 0 degrees, and is a fluid constantly of the same specific gravity. Spirit of wine, independent of its being more variable in respect of specific gravity by the influ-

ence of heat and cold, is also more or less so, as it is more or less rectified. And though the true specific gravity were known at the beginning of the operation, it would even change during the time of using it, by imbibing moisture from the air.

Example. If it were required to know the force of the wind, when the column of water sustained was equal to $4\frac{6}{10}$ inches. Then, by tab. 1,

4 inches =	....	20.833 lb.
0.5 or $\frac{1}{2}$ inch =		2.604
0.1 =	....	0.521
sum 4.6 =	....	23.958 = force on every square foot.

TABLE II.

Names of liquors.	Specific gravities.	Common multiplier.	Weight Measuring the forces of the wind.
Water........	1.000		$1 \times nw$
Sat sol. of salt..	1.244		$1.244 \times nw$
Urine........	1.030		$1.030 \times nw$
Ditto	1.016	nw	$1.016 \times nw$
Alkohol	0.825		$0.825 \times nw$
Proof spirits ..	0.927		$0.927 \times nw$
&c. &c.			&c. &c.

Let w represent the weight of a column of water, having its altitude measured by one of the divisions on the scale, and its base equal to any given surface whatever; and let n denote in general the number of these divisions that measures the whole length of the column of the water which the wind sustains. Then nw will represent always its weight, and will serve as a common multiplier for the specific gravities of all other liquors.

Example. Let w represent the weight of a column of water $\frac{1}{20}$ of an inch high, standing on a square foot; and let $n = 80 = 4$ inches. Then, by tab. 1, nw is equal to 20.833 avoirdupois pounds. Therefore $1.244 \times 20.833 =$ weight of a saturated solution of sea-salt of the same altitude, and $\frac{4}{1.244} =$ the altitude of a column of a saturated solution of the same, weighing 20.833 lb. avoirdupois; w may represent a square yard, the surface of a sail, &c.

If the velocity and density of the wind in any particular case were accurately determined, this instrument, which gives its force or momentum, would enable us to ascertain the velocity in every other case, the density being known. For it appears from experiments, made by Mr. James Ferguson, F.R.S., on the whirling table, that its force is as the square of its velocity. But as the density, which is one of the data requisite for determining the velocity of this instrument, was not taken into consideration in these experiments, all that we can do at present is to suggest the idea.

P. S.—The wind-gage ought to be somewhat longer than that first mentioned. For they had a gale at Edinburgh May 9th, 1775, which supported a column of water of $6\frac{7}{10}$ inches. The force of this gale on a square foot was equal to 34.921 lb avoirdupois, and it did great damage to the gardens. West India hurricanes would require gages of a still greater length to measure them.

XXXV. Astronomical Observations made at Leicester. By the Rev. Mr. Ludlam, Vicar of Norton, near Leicester. p. 366.

The first observations are a set of zenith distances of stars, to determine the

latitude of the place; which comes out 52° 38′ very exactly. Next follow a few observed occultations; and some solar transits, to examine the clock.

XXXVI. Remarks and Considerations relative to the Performance of Amputation above the Knee, by the Single Circular Incision. By Benjamin Gooch, Surgeon, at Norwich. p. 273.

Reprinted in Mr. Gooch's Chirurgical Works, 3 vols. 8vo., 1792.

XXXVII. Concerning Aneurysms in the Thigh. By Benjamin Gooch, Surgeon, at Norwich. p. 378.

May be consulted in Mr. Gooch's Chirurgical Works above referred to.

XXXVIII. An Account of further Discoveries in Air. By the Rev. Joseph Priestley, LL. D., F. R. S. p. 384.

Reprinted in Dr. Priestley's collected works on different Kinds of Air.

XXXIX. An Account of the Gymnotus Electricus. By John Hunter, F. R. S. p. 395.

To Mr. Walsh, the first discoverer of animal electricity, the learned will be indebted for whatever the following pages may contain, either curious or useful. The specimen of the animal which they describe was procured by that gentleman, and at his request this dissection was performed, and this account of it is communicated.

This fish, on the first view, appears very much like an eel, from which resemblance it has most probably got its name; but it has none of the specific properties of that fish. This animal may be considered, both anatomically and physiologically, as divided into 2 parts; viz. the common animal part; and a part which is superadded, viz. the peculiar organ. I shall at present consider it only with respect to the last; as the first explains nothing relating to the other, nor any thing relating to the animal economy of fish in general. The first, or common animal part, is so contrived as to exceed what was necessary for itself, in order to give situation, nourishment, and most probably the peculiar property to the second. The last part, or peculiar organ, has an immediate connection with the first; the body affording it a situation; the heart, nourishment; and the brain, nerves, and probably its peculiar powers. For the first of these purposes, the body is extended out in length, being much longer than would be sufficient for what may be called its progressive motion. For the real body, or that part where the viscera and parts of generation lie, is situated, with respect to the head, as in other fish, and is extremely short; so that, according to the ordinary proportions, this should be a very short fish. Its great length, therefore, seems chiefly intended to afford a surface for the support of the peculiar

organ: however, the tail part is likewise adapted to the progressive motion of the whole, and to preserve the specific gravity; for the spine, medulla spinalis, muscles, fin, and air bladder, are continued through its whole length. Besides which parts, there is a membrane passing from the spine to that fin which runs along the belly or lower edge of the animal. This membrane is broad at the end next the head, terminating in a point at the tail. It is a support for the abdominal fin, gives a greater surface of support for the organ, and makes a partition between the organs of the two opposite sides.

The Organs.—The organs which produce the peculiar effect of this fish, constitute nearly one-half of that part of the flesh in which they are placed, and perhaps make more than one-third of the whole animal. There are 2 pair of these organs, a larger, and a smaller; one being placed on each side. The large pair occupy the whole lower or anterior, and also the lateral part of the body, making the thickness of the fore or lower parts of the animal, and run almost through its whole length; viz. from the abdomen to near the end of the tail. It is broadest on the sides of the fish at the anterior end, where it incloses more of the lateral parts of the body, becomes narrower towards the end of the tail, occupying less and less of the sides of the animal, till at last it ends almost in a point. These two organs are separated from one another at the upper part, by the muscles of the back, which keep their posterior or upper edges at a considerable distance from one another; below that, and towards the middle, they are separated by the air bag; and at their lower parts they are separated by the middle partition. They begin forwards, by a pretty regular edge, almost at right angles with the longitudinal axis of the body situated on the lower and lateral parts of the abdomen. Their upper edge is a pretty straight line, with small indentations made by the nerves and blood vessels, which pass round it to the skin. At the anterior end they go as far towards the back as the middle line of the animal; but in their approach towards the tail they gradually leave that line, coming nearer to the lower surface of the animal. The general shape of the organ, on an external or side view, is broad at the end next the head of the animal, becoming gradually narrower towards the tail, and ending there almost in a point. The other surfaces of the organ are fitted to the shape of the parts with which they come in contact; therefore on the upper and inner surface it is hollowed, to receive the muscles of the back. There is also a longitudinal depression on its lower edge, where a substance lies, which divides it from the small organ, and which gives a kind of fixed point for the lateral muscles of the fin. Its most internal surface is a plane adapted to the partition which divides the two organs from one another. The edge next the muscles of the back is very thin, but the organ becomes thicker and thicker towards its middle, where it approaches the centre

of the animal. It becomes thinner again, towards the lower surface or belly; but that edge is not so thin as the other. Its union with the parts to which it is attached is in general by a loose, but pretty strong, cellular membrane; except at the partition, to which it is joined so close as to be almost inseparable.

The small organ lies along the lower edge of the animal, nearly to the same extent as the other. Its situation is marked externally by the muscles which move the fin under which it lies. Its anterior end begins nearly in the same line with the large organ, and just where the fin begins. It terminates almost insensibly near the end of the tail, where the large organ also terminates. It is of a triangular figure, adapting itself to the part in which it lies. Its anterior end is the narrowest part; towards the tail it becomes broader; in the middle of the organ it is thickest; and from thence becomes gradually thinner to the tail, where it is very thin. The two small organs are separated from one another by the middle muscle, and by the bones on which the bones of the fins are articulated. The large and the small organ on each side, are separated from one another by a membrane, the inner edge of which is attached to the middle partition, and its outer edge is lost on the skin of the animal. To expose the large organ to view, nothing more is necesary than to remove the skin, which adheres to it by a loose cellular membrane. But to expose the small organ, it is necessary to remove the long row of small muscles which move the fin.

Of the structure of these Organs.—The structure is extremely simple and regular, consisting of 2 parts; viz. flat partitions or septa, and cross divisions between them. The outer edge of these septa appear externally in parallel lines nearly in the direction of the longitudinal axis of the body. These septa are thin membranes, placed nearly parallel to one another. Their lengths are nearly in the direction of the long axis, and their breadth is nearly the semidiameter of the body of the animal. They are of different lengths, some being as long as the whole organ. I shall describe them as beginning principally at the anterior end of the organ, though a few begin along the upper edge; and the whole, passing towards the tail, gradually terminate on the lower surface of the organ; the lowermost at their origin terminating soonest. Their breadths differ in different parts of the organ. They are in general broadest near the anterior end, answering to the thickest part of the organ, and become gradually narrower towards the tail, however they are very narrow at their beginnings or anterior ends. Those nearest the muscles of the back are the broadest, owing to their curved or oblique situation on these muscles, and get gradually narrower towards the lower part, which is in a great measure owing to their becoming more transverse, and also to the organ becoming thinner at that place. They have an outer and an inner edge. The outer is attached to the skin of the animal, to the lateral muscles of the fin, and to the membrane which divides

the great organ from the small; and the whole of their inner edges are fixed to the middle partition formerly described, also to the air bladder, and 3 or 4 terminate on that surface which incloses the muscles of the back. These septa are at the greatest distance from one another at their exterior edges near the skin, to which they are united; and as they pass from the skin towards their inner attachments, they approach one another. Sometimes we find 2 uniting into one. On that side next the muscles of the back, they are hollow from edge to edge, answering to the shape of those muscles; but become less and less so towards the middle of the organ; and from that towards the lower part of the organ, they become curved in the other direction. At the anterior part of the large organ, where it is nearly of an equal breadth, they run pretty parallel to one another, and also pretty straight; but where the organ becomes narrower, it may be observed in some places, that 2 join or unite into 1; especially where a nerve passes across. The termination of this organ at the tail is so very small that I could not determine whether it consisted of one septum or more. The distances between these septa will differ in fishes of different sizes. In a fish of 2 feet 4 inches in length, I found them to be about $\frac{1}{27}$ of an inch distant from one another; and the breadth of the whole organ, at the broadest part, about an inch and a quarter, in which space were 34 septa. The small organ has the same kind of septa, in length passing from end to end of the organ, and in breadth passing quite across; they run somewhat serpentine, not exactly in straight lines. Their outer edges terminate on the outer surface of the organ, which is in contact with the inner surface of the external muscle of the fin, and their inner edges are in contact with the centre muscles. They differ very much in breadth from one another; the broadest being equal to one side of the triangle, and the narrowest scarcely broader than the point or edge. They are pretty nearly at equal distances from one another; but much nearer than those of the large organ, being only about $\frac{1}{56}$ part of an inch asunder: but they are at a greater distance from one another towards the tail, in proportion to the increase of breadth of the organ. The organ is about half an inch in breadth, and has 14 septa. These septa, in both organs, are very tender in consistence, being easily torn. They appear to answer the same purpose with the columns in the torpedo, making walls or butments for the sub-divisions, and are to be considered as making so many distinct organs. These septa are intersected transversely by very thin plates or membranes, whose breadth is the distance between any 2 septa, and therefore of different breadths in different parts; broadest at the edge which is next to the skin; narrowest at that next to the centre of the body, or to the middle partition which divides the 2 organs from one another. Their lengths are equal to the breadths of the septa, between which they are situated. There is a regular series of them continued from one end of any 2

septa to the other. They appear to be so close as even to touch. In an inch in length there are about 240, which multiplies the surface in the whole to a vast extent.

Of the Nerves.—The nerves in this animal may be divided into 2 kinds; the 1st, appropriated to the general purposes of life; the 2d, for the management of this peculiar function, and very probably for its existence. They arise in general from the brain and medulla spinalis, as in other fish; but those from the medulla are much larger than in fish of equal size, and larger than is necessary for the common operations of life. The nerve which arises from the brain, and passes down the whole length of the animal (which I believe exists in all fish) is larger in this than in others of the same size, and passes nearer the spine. In the common eel it runs in the muscles of the back, about midway between the skin and spine. In the cod it passes immediately under the skin. From its being larger in this fish than in others of the same size, one might suspect, that it was intended for supplying the organ in some degree; but this seems not to be the case, as I was not able to trace any nerves going from it to join those from the medulla spinalis, which run to the organ. This nerve is as singular an appearance as any in this class of animals; for surely it must appear extraordinary, that a nerve should arise from the brain to be lost in common parts, while there is a medulla spinalis giving nerves to the same parts. It must still remain one of the inexplicable circumstances of the nervous system. The organ is supplied with nerves from the medulla spinalis from which they come out in pairs between all the vertebræ of the spine. In their passage from the spine they give nerves to the muscles of the back, &c. They bend forwards and outwards on the spine, between it and the muscles, and send out small nerves to the external surface, which join the skin near to the lateral lines. These ramify on the skin, but are principally bent forwards between it and the organ, into which they send small branches as they pass along. They seem to be lost in these 2 parts. The trunks get upon the air-bladder, or rather dip between it and the muscles of the back, and continuing their course forwards on that bag, they dip in between it and the organ, where they divide into smaller branches, they then get upon the middle partition, on which they continue to divide into still smaller branches; after which they pass on, and get upon the small bones and muscles, which are the bases for the under fin, and at last they are lost on that fin. After having got between the organ and the abovementioned parts, they are constantly sending small nerves into the organs; first into the great organ, and then into the small one; also into the muscles of the fin, and at last into the fin itself. These branches, which are sent into the organ as the trunk passes along, are so small, that I could not trace their ramifications in the organs. In this fish, as well as in the torpedo, the nerves which supply the organ are much larger than those

bestowed on any other part for the purposes of sensation and action; but it appears to me, that the organ of the torpedo is supplied with much the largest proportion. If all the nerves which go to it were united together, they would make a vastly greater chord, than all those which go to the organ of this eel. Perhaps when experiments have been made on this fish, equally accurate with those made on the torpedo, the reason for this difference may be assigned.

Blood Vessels.—How far this organ is vascular, I cannot positively determine; but from the quantities of small arteries going to it, I am inclined to believe, that it is not deficient in vessels. The arteries arise from the large artery which passes down the spine; they go off in small branches like the intercostals in the human subject, pass round the air bladder, and get upon the partition together with the nerves, and distribute their branches in the same manner. The veins take the same course backwards, and enter the large vein which runs parallel with the artery.

Pl. 14, fig. 1, Shows the whole animal of $\frac{1}{3}$ the full size. It lies on one side; which posture exposes the whole of the under fin. The head is twisted, to show its upper part, on which are seen the eyes, &c.

Fig. 2, Shows the animal lying in the same position, but the head is twisted in the contrary direction, so as to expose its under surface. Between the two fins, and before the beginning of the under fin, is the cavity of the belly of the fish; at the anterior part of which cavity is the anus.

Fig. 3, Exhibits the whole of the two organs on each side, the skin being removed as far as these organs extend. A The lower surface of the head of the animal; B, the cavity of the belly, C, the anus; D, the fin; E, the back of the fish where the skin has not been removed; FF, the fin which runs along the lower edge of the fish; GGG, the skin turned back; HHH, the lateral muscles of the above fin removed and carried back with the skin, to expose the small organ. I. Part of the muscle left in its place. KKK, the large organ; LLL, the small organ; MMMM, the substance which divides the large organ from the small; N, at this place the above substance is removed.

Fig. 4, Is a section of the whole thickness of the fish near the upper part. The skin is removed as far back as the posterior edge of the organ, and the other parts immediately belonging to it, such as the medulla spinalis. There are several pieces or sections taken out of the organ, which expose every thing that has any relation to it. At the upper and lower ends of the figure, FF, the organ is entire, the skin only being removed. AA, The body of the animal near the back, covered by the skin; BB, the belly-fin, covered also by the skin; C, part of the skin removed from the organ, and turned back; DD, the muscles which move the fin laterally, and which immediately cover the small organ; E, the middle muscles of the fin, which lay immediately between the two small organs; FF, the outer surface of the large organ, as it appears when the skin is removed; G, the small organ, as it appears when the lateral muscles are removed; HH, the cut ends of the muscles of the back, which have been removed to expose the deeper seated parts; II, the cut ends of the large organ, part of which has also been removed, to expose the deeper seated parts; K, the cut end of the small organ; L, a part of the large organ, the rest having been removed; M, the cut end of the above section; N, a section of the small organ; OO, the middle partition which divides the two large organs; P, a fatty membrane, which divides the large organ from the small; Q, the air bladder; R, the nerves going to the organ; S, the medulla spinalis; T, the singular nerve.

Fig. 5, is a transverse section of the fish, exposing at one view, all the parts of which it is composed; A, the external surface of the side of the fish; B, the under fin; CCCC, the cut ends of the

muscles of the back; D, the cavity of the air bladder; E, the body of the spine; F, the medulla spinalis; G, the large artery and vein; H H, the cut ends of the two large organs; I I, the cut ends of the two small organs; K, the partition between the organs.

XL. Some Observations on Myrrh, made in Abyssinia, in* 1771, *and sent to Wm. Hunter, M. D., with Specimens, in Feb.* 1775. *By James Bruce,† Esq.* p. 408.

The ancients, and particularly Dioscorides, spoke of myrrh in such a manner as to make us suppose, either that they have described a drug which they had never seen; or that the drug seen and described by them is absolutely unknown to modern naturalists and physicians. The Arabs, however, who form the link of the chain between the Greek physicians and ours, in whose country the myrrh was produced, and whose language gave it its name, have left us undeniable evidence, that what we know by the name of myrrh, is in nothing different from the myrrh of the ancients, growing in the same countries from which it was brought formerly to Greece; that is, from the east coast of Arabia Felix, bordering on the Indian Ocean, and that low land in Abyssinia on the south-east of the Red Sea, included nearly between the 12th and 13th degree of north latitude, and limited on the west by a meridian passing through the island Massowa, and on the east by another passing through Cape Guardsoy, without the straits of Babelmandel. This country the Greeks knew by the name of

* Some years after this paper was sent to the R. S. Mr. Bruce gave a farther account of myrrh in the 5th vol. of his travels to discover the source of the Nile, whence it would appear that the plant, from which the genuine myrrh is obtained, is a species of mimosa.

† This celebrated Abyssinian traveller was born at his paternal estate at Kinnaird, near Falkirk, in Dec. 1729, and where he died in 1794, in the 65th year of his age. He received the first rudiments of his education at Harrow school, which he finished in his own country. An early propensity to travel induced him to seek an appointment in India; but being disappointed in that, he engaged in the wine-trade in London; soon after, however, he visited several parts of Europe; but on the death of his father, he returned to Britain, in 1761. Being offered the consulship of Algiers, he accepted that office, as affording an opportunity of gratifying his roving disposition. Accordingly in 1763 he set out for that place; in his route visiting France, Italy, Greece, Syria, and the isles of the Mediterranean. After a year spent at Algiers, studying and practising the African languages, he commenced his travels in that country, visiting many of the interior parts, particularly Egypt and Abyssinia; where after numerous dangers and distresses he reached the sources of the Nile, the chief object of all his labours, where he arrived in November 1770; whence returning by Nubia, and encountering astonishing difficulties, he at length, after 4 years absence arrived at Cairo the beginning of the year 1773. Whence returning through several parts of Europe, where he met with the favourable reception due to so extraordinary a traveller, he arrived in his native country after an absence of 12 years. In the course of his travels, Mr. B. met with such uncommon events, that many persons were disposed to question his veracity in the accounts he gave of them; which for many years prevented the publication of his travels, which only took place in 1790, in 5 volumes in 4to. It has been said that the king purchased Mr. Bruce's drawings for 2000l. and also defrayed the expence of engraving the plates for his great work.

the Troglodytia; not to be confounded with another nation of Troglodytes, very different in all respects, living in the forests between Abyssinia and Nubia. The myrrh of the Troglodytes was always, as now, preferred to that of Arabia. That part of Abyssinia being half overrun and settled, half wasted and abandoned, by a barbarous nation from the southward, very little correspondence or commerce has been since carried on between the Arabians and that coast; unless by some desperate adventures of Mahometan merchants, made under accidental circumstances, which have sometimes succeeded, and very often likewise have miscarried.

The most frequent way by which this Troglodyte myrrh is exported, is from Massowa, a small Abyssinian island, on the coast of the Red Sea. Yet the quantity of Abyssinian myrrh is so very small, in comparison of that of Arabia sent to Grand Cairo, that we may safely attribute to this only the reason why our myrrh is not so good in quality as the myrrh of the ancients, which was Abyssinian. Though those barbarians make use of the gum, leaves, and bark, of this tree, in diseases to which they are subject; yet as very little is wanted for such purposes, and the tree is the common timber of the country, this does not hinder them from cutting it down every day, to burn for the common uses of life; and as they never plant, or replace the trees destroyed, it is probable, that in some years the true Troglodyte myrrh will not exist; and the erroneous descriptions of the Greek physicians will lead posterity, as they have done us now, into various conjectures, all of them false, on the question, what that myrrh of the ancients was?

Though the myrrh of the Troglodytes was superior to any Arabian, yet the Greeks perceived that it was not all of equal goodness. Pliny and Theophrastus makes this difference to arise from the trees being partly wild, partly cultivated. But this is an imaginary reason; all the trees were wild. But it was the age of the tree and its health, the manner of making the cut or wound in it, the time of gathering the myrrh, and the circumstances of the climate when it was gathered, that constantly determined, and does yet determine, the quality of the drug. In order to have myrrh of the first, or most perfect sort, the savages chuse a young, vigorous tree, whose bark is without moss, or any parasite plant; and, above the first large branches, give the tree a deep wound with an axe. The myrrh which flows the first year, through this wound, is myrrh of the first growth; and never in very great quantity. This operation is performed some time after the rains have ceased; that is, from April to June; and the myrrh is produced in July and August. The sap once accustomed to issue through this gash, continues to do so spontaneously, at the return of every season: but the tropical rains, which are very violent, and continue 6 months, wash so much dirt, and lodge so much water in the cut, that in the 2d year,

the tree has begun to rot and turn foul in that part, and the myrrh is of a 2d quality, and sells in Cairo about a 3d cheaper than the first. The myrrh also produced from gashes near the roots, and in the trunks of old trees, is of the 2d growth and quality, and sometimes worse. This however is the good myrrh of the Italian shops every where but in Venice. It is of a blackish red, foul colour, solid and heavy, losing little of its weight by being long kept; and it is not easily distinguished from that of Arabia Felix. The 3d and worst kind is gathered from old wounds or gashes, formerly made, in old trees; or myrrh that, passing unnoticed, has hung upon the tree ungathered a whole year; black and earth-like in colour, and heavy, with little smell and bitterness. This apparently is the caucalis of the ancients.

Pliny speaks of stacte, as if it was fresh or liquid myrrh; and Dioscorides, (cap. 67,) says something like this also. However, it is not credible that the ancients, either Greeks or Latins, placed at such a distance, could ever see the myrrh in that state. The natives of its country say, that it hardens on the tree instantly, on being exposed to air; and I, who was several months within 4 days journey of the place where it grew, and had the savages quite at my devotion to go and come from thence, could never see the newest myrrh softer than the state it now is in; though I think it dissolved more perfectly in water, than when it had been kept. Dioscorides too mentions a kind of myrrh, which he says was green, and of the consistence of paste. But as Serapion and the Arabs say, that stacte was a preparation of myrrh dissolved in water, it is probable, that this unknown green kind of Dioscorides was, like the stacte, a composition of myrrh and some other ingredient, not a species of Abyssinian myrrh, which he could never have seen, either soft or green.

It may be remarked, that when we buy fresh or new myrrh, it has always a very strong, rancid, oily smell; and when thrown into water, globules of an oily matter swim upon the surface. This greasiness is not from the myrrh; it is owing to the savages using goats-skins anointed with butter, to make them supple, in which to put their myrrh at gathering; and in these skins it remains, and is brought to market: so that, far from its being a fault, as some ignorant druggists at Rome and Venice believe, it is a mark that the myrrh is fresh gathered, which is the best quality that myrrh of the first sort can have. Besides, far from injuring the myrrh, this oily covering must rather at first have been of service; as it certainly imprisons and confines the volatile parts of new myrrh, which escape in great quantities, to a very considerable diminution in the weight. The piece of myrrh which I send you, is what a fine tree, less than 15 inches diameter in the trunk at the bottom, wounded in 2 places, produced at one of the wounds, in the year 1771. And it may be regarded as the only unexceptionable and authentic evidence in Europe, of what the Troglodyte

myrrh was; unless it be those pieces still remaining in my collection, and a piece, somewhat smaller than yours, which I gave to the king of France's cabinet at Paris. This piece which I send you, had lost near 6 drachms Troy of its weight, between the 27th of August, 1771, and the 29th of June, 1773. It has lost a very few grains since. It was kept, as were all the other pieces, with great care in cotton, separately in a box, to prevent its losing weight by friction.

Opocalpasum.—At the time when I was on the borders of the Tal-Tal, or Troglodyte country, I sought to procure myself branches and bark of the myrrh tree, enough preserved to be able to draw it; but the length and ruggedness of the way, the heat of the weather, and the carelessness and want of resources of naked savages, always disappointed me. In those goat-skin bags into which I had often ordered them to put small branches, I always found the leaves mostly in powder; some few that were entire, seemed to resemble much the acacia vera; but were wider towards the extremity, and more pointed immediately at the end. In what order the leaves grew, I never could determine. The bark was absolutely like that of the acacia vera; and among the leaves I often met with a small straight weak thorn, about 2 inches long. These were all the circumstances I could combine, relative to the myrrh tree, too vague and uncertain to risk a drawing on, when there still remained so many desiderata concerning it; and as the king was obstinate not to let me go thither, after what had happened to the surgeon, mate, and boat's crew, of the Elgin Indiaman, I was obliged to abandon the drawing of the myrrh tree to some more fortunate traveller. At the same time that I was taking these pains about the myrrh, I had desired the savages to bring me all the gums they could find, with the branches and bark of the trees that produced them. They brought me, at different times, some very fine pieces of incense, and at another time, a very small quantity of a bright colourless gum, sweeter on burning than incense; but no branches of either tree, though I found this latter afterwards, in another part of Abyssinia. But at all times they brought me quantities of gum, of an even and close grain, and of a dark brown colour, which was produced by a tree called sassa: and twice I received branches of this tree in tolerable order; and of these I made a drawing. Some weeks after, walking in a Mahometan village, I saw a large tree, with the whole upper part of the trunk and the large branches so covered with great bosses and knobs of gum, as to appear monstrous: and asking further about the tree, I found that it had been brought, many years before, from the myrrh country by merchants, and planted there for the sake of its gum, with which these Mahometans stiffened the blue Surat cloths, which they got damaged from Mocha, to trade in with the Galla and Abyssinians. Neither the tree which they called sassa, nor the name, nor the gum, could allow me to doubt a

moment that it was the same as what had been brought to me from the myrrh country; but I had the additional satisfaction to find the tree all covered over with beautiful crimson flowers, of a very extraordinary and strange construction. I began then a drawing anew, with all that satisfaction known only to those who have been conversant in such discoveries. I took pieces of the gum with me. It is very light. Galen complains that, in his time, the myrrh was often mixed with a drug which he calls opocalpasum, by a Greek name; but what this drug was, is totally unknown to us at this day. But as the only view of the savage, in mixing another gum with his myrrh, must have been to increase the quantity; and as the great plenty in which this gum is produced, and its colour makes it very proper for this use; and above all, as there is no reason to think there is another gum-bearing tree, of equal qualities, in the country where the myrrh grows, it seems to me next to a proof, that this must have been the opocalpasum.

I must however confess, that Galen says the opocalpasum was so far from an innocent drug, that it was a mortal poison, and had produced very fatal effects. But as those Troglodytes, though now more ignorant than formerly, are still well acquainted with the properties of their herbs and trees, it is not possible that the savage, desiring to increase his sales, would mix them with a poison that must needs diminish them. And we may therefore without scruple suppose, that Galen was mistaken in the quality ascribed to this drug; and that he might have imagined that people died of the opocalpasum, who perhaps really died of the physician. First, because we know of no gum or resin that is a mortal poison: 2dly, because, from the construction of its parts, gum is very ill adapted for having the activity which violent poison has; and considering the small quantities in which myrrh is taken, and the opocalpasum could have been but in an inconsiderable proportion to the myrrh, to have killed, it must have been a very active poison: 3dly, these accidents, from a known cause, must have brought myrrh into disuse, as certainly as the Spaniards mixing arsenic with the bark, would banish that drug when we saw people die of it. Now this never was the case: it maintained its character among the Greeks and the Arabs, and so down to our days; and a modern physician thinks it might make man immortal, if it could be rendered perfectly soluble in the human body.

Galen then was mistaken as to the poisonous quality of the opocalpasum. The Greek physicians knew little of the natural history of Arabia; still less of that of Abyssinia; and we who have followed them know nothing of either. This gum, being put into water, swells and turns white, and loses all its glue: it resembles gum adragant much in quality, and may be eaten safely. This specimen came from the Troglodyte country in the year 1771: a piece of myrrh from Arabia Felix, and a piece of gum of the sassa from Abyssinia, were packed

up in another separate box, to be sent you for comparison, but forgotten by my servant. They will be sent hereafter. The sassa, the tree which produces the opocalpasum, does not grow in Arabia. Arabian myrrh is easily known from Abyssinian by the following method: take a handful of the smallest pieces, found at the bottom of the basket where the myrrh was packed, and throw them into a plate, and just cover them with water a little warm; the myrrh will remain for some time without visible alteration, for it dissolves slowly; but the gum will swell to 5 times its original size, and appear so many white spots among the myrrh. The pieces sent are, N° 1, Virgin Troglodyte myrrh. N° 2, the worst sort of Troglodyte myrrh, called cancabs. N° 3, Opocalpasum from the myrrh-country.

XLI. An Account of a curious Giant's Causeway, or Group of Angular Columns, newly discovered in the Euganean Hills, near Padua, in Italy. By John Strange, Esq. F. R. S. Dated Venice, March 10, 1775. p. 418.

This phenomenon is situated at Castel Nuovo, a small village near Teolo, in the Euganean hills, about 4 miles south-west of the other Giant's Causeway of Monte Rosso before described. Il Sasso di San Biasio, which is the name of the spot where this causeway is situated, is a large insulated rock, composed of the same sort of grey granite that is common to the Euganean hills, before described. The columns which form this causeway, partly against the flank of the rock, and partly round its base, are of the same substance with the rock itself, to which they adhere, as I have constantly observed in all similar groups. They are therefore of a compound nature, like the columns of Monte Rosso, and differ entirely from the common sort, which are mostly homogeneous, or of a uniform texture; as is observable in the jointed, as well as simple species of basaltes. By comparing the pieces of these columns with the fragments of the columns of Monte Rosso, before transmitted to the society, some essential difference will appear between them. Those of San Biasio, though very hard, are rather porous, of a lighter colour than the columns of Monte Rosso, and very much resemble a species of lava.

This porousness Mr. S. once before observed, and more signally too, in some basaltic columns near Achon, in the province of Auvergne, in France. The pores in the columns of both these groups are also irregularly dispersed, and of unequal size, like those of pumice stones and other common pori ignei. Those of the columns of San Biasio are commonly invested with a sort of crocus martis, frequently observed in the pores of other vulcanic concretions. These properties are surely further marks in favour of the igneous origin of such columnar crystallization; especially, since they seem contrary to the principle by which the common aqueous crystals are formed, successively, et per juxta-positionem par-

tium ad partes. In fact, these crystals manifest no such porosity. The columns of Achon, though of a homogeneous substance, yet differ from the common basaltes by their immense size as well as colour, which is rather brown than black. The columns of San Biasio are likewise very large, measuring often 2 feet in diameter. They are also of the simple species, that is not jointed, and mostly quadrangular, which figure seems rather a principal characteristic of this group, being rarely observed in others. So true it is, as I formerly remarked, that some particular characteristic ever distinguishes the different groups of basaltes; which therefore cannot be too narrowly observed, before we pretend to form any opinion about their origin. Some few, but very few, chiefly of the smaller columns of San Biasio, are of a pentagonal form. But there are no hexagonal columns, which, in other basaltic groups, are the most common. The natural position of these columns, whether facing the rock, or about the bottom of it, is mostly perpendicular.

Another adjacent portion of this rock is also characterized by angular, and as it were winding strata, somewhat resembling the bending pillars of Staffa. The rock itself is also composed of angular masses, as are indeed most granites; and these masses are also ranged perpendicularly. Several emerge, as it were, from the tops and sides of the neighbouring rocks and hills, like so many stately and artificial pillars. The winding strata before-mentioned are also parallel with each other, as frequently observed in other granites, as well as common vulcanic strata in general, particularly of the harder sort. Desmarest calls the latter Basaltes en tables; which is a kind of vulcanic slate, formed in parallel strata of different thickness, from 2 or 3 to 5 and 6 inches. This is very common in the provinces of Velay and Auvergne, in France, where it is also used for coverings of houses. The same sort of slate is likewise common to the mountains of Genoa, many of which seem to be of vulcanic origin. These slaty tables, or parallel strata, of granite, are observed near the top of the famous San Gothard, in the ascent of that mountain on the side towards Switzerland. These strata are also ranged perpendicularly, like the other common ones in granites, and resemble Desmarest's basaltes en tables; affording thus another proof of the analogy remarkable between the organization of the different masses in granites, and that of common vulcanic strata in general. The former, as well as the latter, have their prismatic columns, their basaltes en tables, as Desmarest calls them, and en boules. Surely therefore these are strong proofs in favour of the common origin of both. The rocks of San Biasio abound with ferruginous vitrifications, which are frequently observable in granites; and the neighbouring tracts with lava or pori ignei.

XLII. Observations on the Difference between the Duration of Human Life in

Towns and in Country Parishes and Villages. By the Rev. Rich. Price, D. D., F. R. S. p. 424.

This society has lately been much obliged to Dr. Perceval, for the accounts he has communicated of the state of population at Manchester and its adjacent places. These accounts contain some facts which appear curious and important. From the last in particular, there appears to be reason for concluding, that whereas a 28th part of the inhabitants die annually in the town of Manchester, not more than a 56th part die annually in the adjacent country. This implies a difference so great between the rates of human mortality in these different situations, that some persons have thought it incredible. Dr. P. therefore offers the following observations on this subject.

In the first place, the evidence in this instance is such as seems to leave little room for doubt. From an accurate survey it appears, that the number of inhabitants in the town was 27246, in the year 1773. The number of deaths the same year (and also the average for 1772, 1773, and 1774,) was 973; that is, a 28th part of the number of inhabitants. From an equally careful survey it appears, that the number of inhabitants in that part of the parish of Manchester which lies in the country, was 13786. The number of deaths in 1772 was 246; that is, a 56th part of the number of inhabitants. The chief objection to this evidence is, that the number of deaths in that part of the parish which lies in the country is given only for one year; whereas the average of several years ought to be given. But first, the number of deaths in 1772, in the town was nearly the same with the medium for 7 years; and hence there arises a probability, that in the adjacent country, the number of deaths, in the same year, could not have been much lower than the medium. Secondly, supposing it lower, there is the highest probability, that it was not more than a 4th or 5th lower. Suppose then the true annual medium to be 300, instead of 246, and it will follow, that whereas a 28th part of the inhabitants die in the town annually, a 46th part die in the country; and this is a difference very considerable. But the difference which this survey gives between the rate of mortality in the town of Manchester and the adjacent country, is confirmed by a variety of other accounts. It may be stated in general, that whereas in great towns, the proportion of inhabitants dying annually is from 1 in 19 to 1 in 22 or 23, and in moderate towns from 1 in 24 to 1 in 28; in country parishes and villages, on the contrary, this proportion seldom exceeds 1 in 40 or 50. The proofs of this are numerous and unexceptionable. Thus, the number of inhabitants at Stockholm in 1763 was 72979. The average of deaths for the 6 preceding years had been 3802. Therefore 1 in 19 died there annually. At Rome, an account is taken every year of the number of inhabitants; and in the year 1771 it was 159675. The average of deaths for 10 years had been 7367;

therefore 1 in $21\frac{1}{2}$ died annually. In London, at least 1 in $20\frac{3}{4}$ of the inhabitants die annually. And, from a particular survey and a very accurate register of mortality at Northampton, it appears that 1 in $26\frac{1}{2}$ die there annually.

Let these facts be compared with the following. In 1767, a survey was made of the inhabitants of the island of Madeira, under the direction of Dr. Thomas Heberden, and their number was found to be 64614. The average of burials for 8 preceding years had been 1293. Only 1 in 50 therefore of the inhabitants died annually.

The district of Vaud, in Switzerland, in 1766, contained 112951 inhabitants. The average of deaths for 10 preceding years had been 2504. Only 1 in 45 therefore died annually. The number of inhabitants in the parish of Ackworth, in the county of York, in 1757, was 603; and the average of deaths for 10 years had been $10\frac{7}{10}$, or a 56th part. In 1767, the inhabitants were increased to 728; and the annual average of deaths was $15\frac{8}{10}$, or nearly a 47th part.

The reason of this striking difference between the rate of human mortality in towns and in country parishes and villages must be, first, the luxury and the irregular modes of life which prevail in towns; and, secondly, the foulness of the air. But it has been inquired, whether the migrations of people from the country to towns may not produce this difference, by lessening the proportion of inhabitants that die in the country, and increasing the same proportion in towns? In answer to this inquiry, Dr. P. observes, 1st, that this difference, being a difference of near a half, it is apparently much greater than can be accounted for by any such cause. But 2dly, it should be considered, that if migrations lessen the number of deaths, they also lessen the number of inhabitants; and that it depends entirely on the ages at which the inhabitants remove from a place, whether the effect of their removal shall be lowering or raising the proportion of the annual deaths to the number of inhabitants. In the present case, the truth appears to be, that the most common age of migration from the country, is such as raises this proportion in the country. This will be evident from the following considerations. The period of life in which persons remove from the country to settle in towns, is chiefly the beginning of mature life, or from the age of 10 or 15 to 25 or 30. In infancy none migrate; and in the decline of life, it is more usual to retire from towns than to remove to them. Towns therefore will be inhabited more by people in the firmest parts of life; and, on the other hand, the country will be inhabited more by people in the weakest parts of life; and the consequence of this is, that in the country, the inhabitants must die faster in proportion to their number than they otherwise would, and that in towns they must die more slowly. In particular, the number of children is always much greater in the country than in towns; and this is a circumstance which must be extremely unfavourable to the former: for it is well known, that

there are no years of life, in which so many of a given number die, as the first 3 or 4 years. Till the age of 5, human life, like a fire beginning to burn, is very feeble; and in some situations more than $\frac{1}{2}$, and in others, a 3d or 4th of all that are born die before that age. After this, life grows less and less precarious, till it acquires its utmost vigour at 10 or 15; and of the living at this age, not above 1 in 70 or 80 dies annually in the worst situations; and in the best situations, not above 1 in 150 or 160. After 15, life declines, and continues to do so more and more, till it becomes quite extinct in old age. If therefore, in any situation, the inhabitants consist more of persons in mature life, and yet die faster, it must be owing to some particular causes of mortality that operate there. This is the case in all towns where any observations have been made. Manchester, in particular, is not only kept up, but increases fast, by removals to it of persons in the prime of life. The country round it increases likewise; but it is by an excess of the births above the deaths; that is, by accessions to it of children in the very feeblest part of life. This ought to raise the proportion of annual deaths to inhabitants in the country, much above the same proportion in the town; but, instead of this, it is near one half lower.

In order however to put this matter out of all doubt, it appears in fact, from the accounts furnished by Dr. Percival, that the number of inhabitants in the periods of life when mankind die fastest, that is, in the first and last stages of life, is considerably less in the town of Manchester than in the adjacent country. The number of inhabitants in the town, under 15 and above 50, is 13467; in the country, 7305. And the whole number is, in the town, 27246; in the country, 13786. In the town therefore the inhabitants, in the first and last stages of life, do not make half the whole number; but in the country, they make considerably more than half. At Ackworth likewise, in Yorkshire, the inhabitants under 15 and above 50 are more than half the whole number; and the same is true at Hale near Altringham, at Horwich, at Darwen near Blackburn in Lancashire, and at Cockey Moor near Bolton, in the same county, and yet in some of these places it appears, that not a 60th part of the inhabitants die annually. At Stockholm, in 1763, the inhabitants under the age of 5, were only a 12th; above 70, only a 46th part of the whole number. But in all Sweden, the number under 5 was a 7th; and above 70, near the 32d part of all the inhabitants: and yet 35 die in the town to 19 in the whole kingdom. This may be easily deduced from Mr. Wargentin's tables in the Collection Academique.

To the accounts which give the proportion of inhabitants to annual deaths, so high as 50 or 60 to 1, it has been further objected, that if true, it must follow, that in such situations half the inhabitants must live to 50 or 60 years of age. But were this a right inference, there would be nothing in it incredible. For though in most cities one half die in the first 2 or 3 years after birth; yet

in many country situations, the greater part live to marry: and in the parish of Ackworth particularly, it appears with undeniable evidence from the register, that one half of all born there live to the age of 46. It appears also, with equal evidence, from M. Muret's tables in the Bern Memoirs for 1766, that in 43 parishes in the district of Vaud, one half of all born there live beyond the age of 41. In truth, did all mankind lead natural and virtuous lives, that waste of the species which happens in infancy and childhood would not take place, and few would die except in old age.

But to return to Dr. Percival's account of the town and parish of Manchester. It appears from this account, that the number of children under 15, compared with the number of inhabitants between 14 and 51, is greater in the country than in the town of Manchester, in the proportion of no less than 5 to 4. It follows therefore, that though, in consequence of a constant influx of people to the town, it is more filled than the country with inhabitants in the most vigorous periods of life; yet 1 child in 4 less is born in the town than in the country. This is a remarkable circumstance, and the reasons of it must be the two following. First, the town inhabitants, being less healthy, and dying faster, have not the same strength of constitution with the country inhabitants. 2dly, in the town a smaller proportion of the adult inhabitants marry; and they marry later than in the country. The survey fully proves this; for it appears, that though the number of inhabitants at the most common marrying ages, compared with the whole number of the living above the age of 14, is smaller in the country than the town; yet the proportion of the married to the living above 14, is very nearly the same in both situations. And there are more widows and widowers in the town than in the country in the proportion of near 16 to 11. Hence we learn clearly in what manner towns operate in checking population, and preventing the increase of mankind.

Dr. Percival informs us, that the reverend and learned Dr. Tucker has been led, by some observations he has made at Bristol, to doubt whether the common opinion is right, with respect to the disproportion between the number of male and female births; and that he therefore wishes a further inquiry may be made into this subject. This has induced Dr. P. to collect the following facts, which he thinks will abundantly settle this point.

	Born Males.	Females.	Proportion.
In London for the last 110 years, or from 1664 to 1773	862293	81 072	20 to 19
Paris, for 8 years	79693	76481	25 .. 24
Leyden, for 50 years	46773	44933	26 .. 25
Vienna, for 27 years, ending 1746	67060	64893	31 .. 30
Berlin, for 40 years, ending 1761	71188	67431	20 .. 19
Kurmark of Brandenburgh, for 9 years, ending 1759	102425	96521	18 .. 17
Dukedom of Magdeburgh, for 38 years, ending 1759	153227	145985	21 .. 20
All the Prussian towns, for a course of years	691826	659072	21 .. 20
In a great number of country parishes, for a course of years	59067	56282	21 .. 20

2.

	Born Males.	Females.	Proportion.
In the same country parishes, for another period of years	89530	84954	19 to 18
Leeds, Manchester, Coventry, &c. for a period of years	108784	103449	20 .. 19
In the same towns, for another period	57084	54128	20 .. 19
Total	2388950	2271201	20 .. 19
Sweden, for 9 years, ending 1763	416007	396124	20 .. 19

Mr. Derham, in his Physico-Theology, p. 175, has stated the proportion of male to female births at 14 to 13, and this proportion has ever since been generally received as the true one; but it appears from this table, that it ought to have been stated at 20 to 19. But though it appears, that the number of males born is in this proportion greater than the number of females born, yet in most places the number of males living, has been found to be less than the number of females. The reason is doubtless, that males are more short-lived than females; and this is owing partly to the peculiar hazards to which males are subject, and their more irregular modes of life; but it is owing principally to some particular delicacy in the male constitution, which renders it less durable: for there are many observations which prove, that the greater mortality of males takes place chiefly in the first and last stages of life. A few facts of this kind Dr. P. mentions, as he has just met with them.

In the parish of St. Sulpice, at Paris, during 30 years, 5 males under a year old died to 4 females. But under 10, only 13 males died to 12 females. In Stockholm, during 9 years ending in 1763, the number of still-borns amounted to 666; of whom 390 were males, and 276 females; that is, 10 to 7. The number of the living in the town above the age of 80 was, in 1760, 332; of whom 248 were females, and 84 males, or near 3 to 1. In the whole kingdom of Sweden, including all town and country inhabitants, the number of still-borns, during the 9 years just mentioned, was 19845; of whom 11424 were males, and 8421 females, or near 4 to 3. The number of the living in the whole kingdom consisted of more females than males, in the proportion of 10 to 9. It consisted of more females turned of 80 than males, in the proportion of 33 to 19; and of more females turned of 90 than males in the proportion of near 2 to 1. It appears also, that by the excess of the births above the deaths, Sweden gains every year an addition of above 20,000 inhabitants; and that in 6 years they increased from 2323195 to 2446394.

The following tables have been selected from several more of the same kind in M. Wargentin's Memoir on the state of population in Sweden. They are inserted here, because they fully verify most of the observations in the preceding paper, and contain more distinct and authentic information on the subject of human mortality than has ever before been met with.

TABLE I.

Showing the order of human mortality in Sweden.

	Annual deaths, being the average of 3 years 1761, 1762, & 1763. Males.	Females.		Number of living in 1763. Males.	Females.
Still-born	1324	988	Born	47216	44892
Died undre 1	11172	9850	Living under 1	36094	35453
Died between 1 and 3	4393	4336	Living between 1 and 3	66059	67234
3.... 5	2206	2249	3.... 5	66454	67711
5.... 10	2151	2057	5.... 10	130019	130758
10.... 15	933	834	10.... 15	126696	128021
15.... 20	711	658	15.... 20	108312	109985
20.... 25	834	756	20.... 25	92299	105115
25.... 30	883	863	25.... 30	88056	101003
30.... 35	1020	1146	30.... 35	85936	95811
35.... 40	955	923	35.... 40	74826	81453
40.... 45	1180	1170	40.... 45	67448	74854
45.... 50	1099	938	45.... 50	52398	59551
50.... 55	1280	1113	50.... 55	47298	56646
55.... 60	1177	1097	55.... 60	37086	45537
60.... 65	1586	1721	60.... 65	34892	44925
65.... 70	1237	1566	65.... 70	20649	28964
70.... 75	1322	2041	70.... 75	15454	23159
75.... 80	1092	1695	75.... 80	8858	13556
80.... 85	917	1446	80.... 85	4620	7487
85.... 90	414	650	85.... 90	1508	2694
Above 90	215	379	Above 90	527	988
Total of annual deaths.	36777	37488	Total of living at all ages.	1165489	1280905

TABLE II.

Showing the order of human mortality at Stockholm.

	Annual deaths, being the average of 3 years, 1761, 1762, & 1763. Males.	Females.		Number of living in 1763. Males.	Females.
Still born	54	43	Born	1406	1340
Died under 1	567	489	Living under 1	684	733
Died between 1 and 3	161	170	Living between 1 and 3	1173	1348
3.... 5	80	79	3.... 5	1022	1106
5.... 10	71	72	5.... 10	2630	2774
10.... 15	49	24	10.... 15	3151	2918
15.... 20	53	30	15.... 20	3018	2865
20.... 25	91	64	20.... 25	3070	4056
25.... 30	121	78	25.... 30	3380	4251
30.... 35	141	102	30.... 35	3705	4234
35.... 40	118	96	35.... 40	3019	3288
40.... 45	140	115	40.... 45	2846	3130
45.... 50	101	84	45.... 50	1775	1984
50.... 55	105	91	50.... 55	1581	2129
55.... 60	61	54	55.... 60	853	1329
60.... 65	79	88	60.... 65	826	1383
65.... 70	41	54	65.... 70	370	778
70.... 75	33	77	70.... 75	260	574
75.... 80	28	59	75.... 80	128	324
80.... 85	18	45	80.... 85	58	127
85.... 90	7	20	85.... 90	16	51
Above 90	3	11	Above 90	10	22
Total of annual deaths.	2068	1902	Total of living at all ages.	33575	39404

In this table it is observable, that the number of the living, in every equal division of life from birth, decreases continually till all become extinct; and that though the males born are more than the females born, in the proportion of 20 to 19; yet the males living of all ages are less in number, in the proportion of 1165489 to 1280905, or nearly of 10 to 11; notwithstanding which, the males that die annually are to the females as 52 to 53.

In this table it may be observed, that the number living at every age from birth, decreases only till 5. Between 5 and 10, Stockholm begins to receive recruits from the country, and they come in faster and faster till 35; after which age it appears, that more die than come in; and that the living in every subsequent period goes on decreasing continually till the end of life. It is further observable, that this table exhibits a greater difference than the former, between the mortality of males and females.

A comparison of these tables will show a striking contrast in other respects between the state of human mortality in the whole kingdom of Sweden and in its capital. In order to make this more obvious and unexceptionable, I will add the following table, deduced from all M. Wargentin's tables taken together.

TABLE III.

	In all Sweden for 9 years.		In Stockholm for 9 years.	
	Males.	Females.	Males.	Females.
Still-born............	1 in 36	1 in 47	1 in 32	1 in $43\frac{1}{2}$
Died under 1 of all born	1 .. $4\frac{1}{3}$	1 .. $4\frac{4}{5}$	1 .. $2\frac{1}{3}$	1 .. $2\frac{3}{10}$
Died annually of the living betw. 1 and 3	1 .. $17\frac{1}{3}$	1 .. $17\frac{3}{4}$	1 .. 7	1 .. $7\frac{1}{3}$
Between... 3.... 5	1 .. $34\frac{1}{2}$	1 .. 36	1 .. $13\frac{1}{3}$	1 .. 16
5.... 10	1 .. 71	1 .. 76	1 .. $34\frac{1}{2}$	1 .. 39
10.... 15	1 .. 149	1 .. 161	1 .. 79	1 .. 114
15.... 20	1 .. 149	1 .. 164	1 .. 59	1 .. 99
20.... 25	1 .. 108	1 .. 139	1 .. 44	1 .. 79
25.... 30	1 .. 98	1 .. 113	1 .. 33	1 .. 58
30.... 35	1 .. 85	1 .. 84	1 .. 31	1 .. 43
35.... 40	1 .. 78	1 .. 91	1 .. $26\frac{1}{2}$	1 .. 39
40.... 45	1 .. 56	1 .. 63	1 .. 23	1 .. 31
45.... 50	1 .. 49	1 .. 65	1 .. $19\frac{1}{2}$	1 .. 28
50.... 55	1 .. 37	1 .. 50	1 .. $16\frac{1}{2}$	1 .. $25\frac{1}{2}$
55.... 60	1 .. 31	1 .. 40	1 .. 14	1 .. 24
60.... 65	1 .. 23	1 .. 26	1 .. 11	1 .. 16
65.... 70	1 .. 17	1 .. $18\frac{1}{2}$	1 .. $9\frac{1}{2}$	1 .. $13\frac{1}{3}$
70.... 75	1 .. $11\frac{1}{3}$	1 .. $11\frac{1}{2}$	1 .. $7\frac{3}{10}$	1 .. 8
75.... 80	1 .. 8	1 .. $8\frac{1}{3}$	1 .. $4\frac{1}{2}$	1 .. 5
80.... 85	1 .. $5\frac{1}{4}$	1 .. $5\frac{1}{3}$	1 .. $3\frac{1}{2}$	1 .. $3\frac{1}{2}$
85.... 90	1 .. $3\frac{4}{5}$	1 .. 4	1 .. 2	1 .. $2\frac{1}{3}$
Above 90	1 .. $2\frac{1}{2}$	1 .. $2\frac{1}{2}$	1 .. $2\frac{3}{10}$	1 .. $2\frac{1}{3}$
Died of all living at all ages	1 .. $33\frac{1}{2}$	1 .. $36\frac{1}{2}$	1 .. $17\frac{1}{10}$	1 .. $21\frac{1}{4}$

XLIII. Experiments on Animals and Vegetables, with respect to the Power of producing Heat. By John Hunter, F. R. S. p. 446.

Reprinted with additions, in Mr. John Hunter's Observations on Certain Parts of the Animal Economy.

XLIV. A Comparison of the Heat of London and Edinburgh. By John Roebuck, M. D., F. R. S., in a Letter to William Heberden, M. D., F. R. S.* p. 459.

I delivered to you some time ago, a register of the thermometer at Hawkhill,

* Dr. John Roebuck was born at Sheffield, in Yorkshire, in the year 1718; and he died in 1794, at 76 years of age, in Scotland, where for many years he had conducted several important manufactural concerns, of his own establishing, in iron, coal, and chemical productions. His father, being a manufacturer of Sheffield goods, had intended his son for the same occupation; but from the young man's promising genius, he was induced to give him a more liberal education and profession. After the common grammar school foundation at Sheffield, he was sent to Dr. Doddridge's academy at Northampton, where he pursued his studies with distinguished reputation. Hence Mr. R. was removed to the university of Edinburgh, where having gone through a regular course of studies and practice in physic and chemistry, he next spent some time at the university of Leyden, then in high reputation as a school of medicine. There, after the usual residence and course of trials, he obtained the degree of M. D., and returned to England about the end of the year 1743. Here Dr. R. first settled and practised as a physician at Birmingham; where he afterwards established a laboratory,

for 10 years; but as these observations were made at 8 o'clock in the morning and 4 in the afternoon, and yours at 8 o'clock in the morning and 2 in the afternoon, the corresponding years of the morning's observations only admit of a comparison. It appears by your register, that the mean heat at London for 9 years, from the end of 1763 to the end of 1772, at 8 o'clock in the morning, was 47°.4; and the mean heat at Hawkhill, during the same period of time, was 46°. The difference of which is only 1°.4. A difference much less than might be expected from the difference of latitude, and not sufficient to account why nonpareils, golden rennets, peaches, nectarines, and many kinds of grapes, generally come to maturity near London, and scarcely ever near Edinburgh, without the aid of artificial heat. Before proceeding further to perplex myself with this difficulty, I procured from Hawkhill and from yourself the register of the thermometer for 3 years, at the same periods of time. And by these it appears, that the mean heat of London of these 3 years exceeded that of Edinburgh, by 4°.5. And the mean heat of the 3 hottest months in London exceeded the mean heat of the same 3 at Edinburgh, by 5°.8. And the mean heat of these 3 summer months, at 2 o'clock in the afternoon in London, exceeded the mean heat of the same months, at the same hour, in Edinburgh, by 7°.3; which sufficiently accounts why some fruit may come to maturity in one country and not in the other: and also why corn and grass, which vegetate

for the manufacture of certain useful preparations in chemistry. Extending his practice and projects in this line, he next established a manufactory of oil of vitriol at Prestonpans, in Scotland, in the year 1749; after which he made that country his chief residence. Dr. R.'s chemical practice leading him to experiments on smelting iron stone, and preparing that metal, which he did by means of pitcoal, he was thus gradually induced to establish, at Carron, the greatest manufactory of iron in this country. Thus, by the force of his own genius and great exertions, he established three very large and profitable manufactories, the laboratory at Birmingham, the oil of vitriol works at Prestonpans, and the iron works at Carron, all which are still carried on with great emolument to the several proprietors. Unfortunately however for Dr. R. he was induced successively to relinquish each of these concerns, to employ his capital on the next in succession, and finally to that of a large concern in coal-mines, in which his whole fortune was sunk and lost; to the grievous embitterment of the latter years of his life.

From a man so deeply and so constantly engaged in the detail of active business, many literary compositions were not to be expected. It has been happily said that Dr. R. left behind him many *works*, but few *writings*. The great object he kept constantly in view, was to promote arts and manufactures, rather than to establish theories or hypotheses. The above paper, on the comparison of the heat of London and Edinburgh; with another, in these Transactions, of experiments on ignited bodies; and one in the Edinburgh Transactions, on the filling and ripening of corn, are all his essays that have been published, besides two political pamphlets. The paper on ignited bodies was occasioned by a report of some experiments made by the celebrated Buffon, from which he had inferred that matter is heavier when hot than when cold. But Dr. R.'s experiments, made with great accuracy before a committee of the R. S. at London, seem to refute that notion.—See a pretty large and circumstantial account of Dr. R.'s concerns in the Supplement to the Encyclopædia Britannica, from which the above particulars are extracted.

with a more temperate heat, but require longer continuance of it, may arrive at maturity in both countries. The reason why the mean heat of London exceeds that of Edinburgh, may arise principally from the difference of latitude. But the reason why the excess is greater in proportion in the 3 hottest months of the year, at the hottest time of the day, than in the winter months, arises from Edinburgh's being situated nearer to the sea than London. We might speak with more precision on this subject, if we had a register of the thermometer at Moscow, which is nearly in the same latitude as Edinburgh; though it is well known that the heat of summer is much more intense, and the cold of winter much more severe, at Moscow, than at Edinburgh. The mean heat of springs near Edinburgh seems to be 47°; and at London 51°. It is probable, that the mean heat of good springs in any country is very nearly the mean heat of the country. A faithful account of the heat of springs in different latitudes, and of water taken from the same depth of the sea in different latitudes is yet wanted.

Mean Heat in Pall Mall, London.

	1772.		1773.		1774.		Mean Heat of 3 Years.	
	8 A. M.	2 P. M.	8 A. M.	2 P. M.	8 A. M.	2 P. M.	8 A. M.	2 P. M.
Jan.	36	38	42	44	34	39	37.3	40.3
Feb.	38	42	36	41	38	44	37.3	42.3
March	41	47	40	51	41	52	40.7	50
April	44	51	45	55	47	55	45.3	53.7
May	49	60	50	60	51	60	50	60
June	64	73	58	67	59	67	60.3	69
July	61	72	60	68	61	69	60.7	69.7
Aug.	60	70	62	72	62	70	61.3	70.7
Sept.	56	65	56	63	55	63	55.7	63.7
Oct.	56	61	51	59	48	58	51.7	59.3
Nov.	45	55	40	47	40	44	41.7	48.7
Dec.	41	44	41	45	39	43	40.3	44
Mean	49.2	56.5	48.4	56	47.9	55.3	48.5	56

Mean Heat of 3 years morning and afternoon was 52.2.

Mean Heat at Hawkhill, situated about 1 mile North of Edinburgh, and 103 feet above the level of the sea.

	1772.		1773.		1774.		Mean Heat of 3 Years.	
	8 A. M.	2 P. M.	8 A. M.	2 P. M.	8 A. M.	2 P. M.	8 A. M.	2 P. M.
Jan.	31.5	34.3	38.5	40.3	29.1	33	33.3	35.8
Feb.	30.9	36.5	35.1	40.7	36.2	40.4	34	39.2
March	37	42.8	42.1	48.4	37.1	43.2	38.7	44.8
April	42.9	48.5	45.6	51.1	44.1	48.9	44.2	49.5
May	49.1	54.5	48.6	53.1	46.6	50.8	48.1	52.8
June	57.2	62.1	55.2	60.1	51.1	59.7	54.5	60.6
July	58.7	64.6	57.7	61.9	57.4	63.3	57.9	63.3
Aug.	57.4	63.9	58.3	64.8	57.2	62.5	57.6	63.7
Sept.	51.5	58.1	51.3	55.8	51.7	57.8	51.5	57.2
Oct.	48.8	51.6	46	50.7	48.3	52.8	47.7	51.7
Nov.	41.7	44.6	38.2	42.3	38	42	39.3	42.9
Dec.	39.7	41.6	36.4	38.5	37.3	40	37.8	40
Mean	45.5	50.3	46.1	50.6	44.5	49.5	45.4	50.1

Mean Heat of 3 years morning and afternoon was 47.7.

XLV. Experiments in a Heated Room. By Matthew Dobson, M. D. p. 463.

Exper. The sweating-room of our public hospital at Liverpool, says Dr. D., which is nearly a cube of 9 feet, lighted from the top, was heated till the quicksilver stood at 224° on Fahrenheit's scale; above which the tube of the thermometer would not admit the heat to be raised. The thermometer was suspended by a string fixed to the wooden frame of the sky-light, and hung down about the centre of the room. Myself and several others were at this time inclosed in the stove, without experiencing any oppressive or painful sensation of heat, proportioned to the degree pointed out by the thermometer. Every metallic about us soon become very hot. 2. My friend Mr. Park, a surgeon, went into the stove heated to 202°. After 10 minutes, I found the pulse quickened to 120°. And to determine the increase of the animal heat, another

thermometer was handed to him, in which the quicksilver already stood at 98°; but it rose only to 99½, whether the bulb of the thermometer was inclosed in the palms of the hands, or received into the mouth.* The natural state of this gentleman's pulse is about 65. 3. Another gentleman went through the same experiment in the same circumstances, and with the same effects. 4. One of the porters to the hospital, a healthy young man, and the pulse 75, was inclosed in the stove when the quicksilver stood at 210°; and he remained there, with little inconvenience, for 20 minutes. The pulse, now 164, and the animal heat, determined by another thermometer as in the former experiments, was 101½. 5. A young gentleman of a delicate and irritable habit, whose natural pulse is about 80, remained in the stove 10 minutes when heated to 224°. The pulse rose to 145, and the animal heat to 102°. This gentleman, who had been frequently in the stove during the course of the day, found himself feeble, and disposed to break out into sweats for 24 hours after the experiment. 6. Two small tin vessels, containing each the white of an egg, were put into the stove heated to 224°. One of them was placed on a wooden seat near the wall, and the other suspended by a string about the middle of the stove. After 10 minutes, they began to coagulate; but the coagulation sensibly quicker and firmer in that which was suspended, than in that which was placed on the wooden seat. The progress of the coagulation was as follows: it was first formed on the sides, and gradually extended itself; the whole of the bottom was next coagulated; and last of all the middle part of the top. 7. Part of the shell of an egg was peeled away, leaving only the film which surrounds the white; and part of the white being drawn out, the film sunk so as to form a little cup. This cup was filled with some of the albumen ovi, which was consequently detached as much as possible from every thing but the contact of the air and of the film which formed the cup. The lower part of the egg stood on some light tow in a common gallipot, and was placed on the wooden seat in the stove. The quicksilver in the thermometer still continued at 224°. After remaining in the stove for an hour, the lower part of the egg, which was covered with the shell, was firmly coagulated; but that which was in the little cup was fluid and transparent. At the end of another hour it was still fluid, except on the edges where it was thinnest; and here it was still transparent; a sufficient proof that it was dried, not coagulated. 8. A piece of bees wax, placed in the same situation with the albumen ovi of the preceding experiment, and exposed to the same degree of heat in the stove, began to melt in 5 minutes: another piece suspended by a string, and a 3d piece put into the tin vessel and suspended, began likewise to liquify in 5 minutes.

* The scale of the thermometer, which was suspended by the string about the middle of the room, was of metal; this was the only one I could then procure, on which the degrees ran so high as to give any scope to the experiment. The scale of the other thermometer, which was employed for ascertaining the variations in the animal heat, was of ivory.—Orig.

Observations.—That heated air should have such a speedy and powerful effect in quickening the pulse, while the animal heat is little altered from its natural standard; that the human body should so easily bear to be surrounded with air heated to 224°; that the albumen ovi, which begins to coagulate in water at 150°, should remain fluid in 224°; and that the same albumen ovi, still placed in air heated to 224°, should coagulate if in contact either with tin or its own shell, are facts as singular as they are difficult of explanation. From the different effects of heated air on the pulse and the heat of the body, do we not discover the fallacy of that theory of animal heat which has been adopted by Boerhaave and other celebrated physiologists? They suppose that animal heat is produced by the attrition of the globules of the circulating fluids against the sides of the containing vessels; but in several of the preceding experiments, the circulation was amazingly quickened with little increase of the animal heat. But whence is it that the human body can bear without immediate injury, to be surrounded with air heated to 224°? And whence is it, that the albumen ovi does not coagulate in this degree of heat? Is it that fire as it passes into some bodies becomes latent, agreeable to a doctrine which has for some time been taught at Edinburgh by Professor Black? Or does fire become fixed and quiescent, according to a similar system adopted by Dr. Franklin?* Air we know exists either in a fixed or elastic state; and fire may in like manner exist in bodies, either in a latent, fixed, and quiescent; or in a sensible, fluid, and active state. Agreeable to this idea, the bees wax receives the fire in an active state, and dissolves; while the human body and the albumen ovi, receiving the fire in a latent state, are little altered in their temperature. Let each of these however be put in contact with a different body, tin for instance; and though the heat of the air continues the same, yet the fire no longer enters in a latent state, but with all its sensible and active powers; for the albumen ovi suspended in a tin vessel soon coagulates; and the human body, covered with the same metal, would quickly experience an intolerable and destructive degree of heat. Or are the above phenomena more satisfactorily explained, by considering different bodies as possessing different conducting powers; some being strong, others weak conductors of fire? All those bodies then which are weak conductors of fire from air, may be placed in air, without receiving the heat of this medium. Hence the albumen ovi remains fluid in air heated to 224°. Hence likewise the frog, the lizard, the camelion, &c. retain their natural temperature, and feel cold to the touch, though perpetually surrounded with air hotter than their own bodies. Hence also, the human body keeps nearly its own temperature, in a stove heated to

* Exper. and Observ. p. 346 and 412.

224°; or may even pass without injury into air heated to a much greater degree, according to the observations of Duhamel and Tillett, published in the Memoirs of the Acad. of Sciences for 1761. On the other hand, all those bodies which are powerful conductors of fire from air, are influenced in proportion when surrounded with this medium. The bees wax melted from the mere contact of the air in experiment 8; and in experiment 6, the albumen ovi was coagulated on the intervention of another body, which is a strong conductor of fire from air. But whether this method of reasoning on the natural cause of these effects be just or not, the final cause is obvious, and is to be resolved into the wise and benevolent appointment of the Almighty. Man is happily so framed, as to possess a power of keeping nearly the same tenor of heat, in all the variations of the temperature of the air in summer and in winter, in hot and cold climates; and consequently changes his situation on the surface of the globe, with much less inconvenience or injury, than he could otherwise have done. The same power likewise happily adapts different animals to their respective destinations. The lizard and the camelion remain cool under the equator, while the whale and porpoise retain a degree of heat above that of the human body, though surrounded with the waters of the coldest Northern Seas, and amidst mountains of ice in the neighbourhood of the Pole.

XLVI. Calculations in Spherical Trigonometry Abridged. By Israel Lyons. p. 470.

Since astronomical observations have been made with much greater precision than formerly, it has become requisite that the calculations corresponding to them should likewise be made to much greater degrees of exactness. The ancient astronomers desired only to make their observations and computations agree within a part of a degree; succeeding ones were satisfied when they corresponded within a minute; but no less exactness than seconds will content the moderns. The rules in spherical trigonometry being reduced to operations by logarithms, it is necessary to use such a number of figures in the tables as will produce the required precision; this is very different in the various parts of the quadrant, insomuch that if the arc is only 1 degree, 4 places of decimals in the logarithm of a sine are sufficient to determine the arc to which it belongs within a second: whereas if the arc is 89°, there is a necessity of using 8 figures for the same purpose: thus, the logarithm sine of 89° 0′ 0″ is 9.9999338, the same 7 figures as for the logarithm sine of 89° 0′ 1″. From this consideration it follows, that the analogies commonly laid down and used for the solutions of spherical triangles, are not in all cases equally convenient, and I might say, equally accurate; and that it would be more easy and exact in calculations to find what was required, by means of sines of arcs, which, being small, require the use of only a few places of figures. Now the cases which often occur in

astronomy, where spherical trigonometry can only be of use, are generally of such a nature that we know nearly, or at least within a few degrees, what the required side or angle is; there is nothing therefore wanted but to find how much this quantity, or first approximation, differs from the true value of the side or angle. Thus, in calculating the right ascension of any point of the ecliptic, whose longitude and declination are known, instead of finding the right ascension immediately, it will be more convenient to seek for the difference between the longitude and right ascension immediately, and as it never exceeds $2\frac{1}{2}^0$, 4 or 5 places of figures will always be sufficient to determine it within a second. And in other similar cases, rules might be made agreeable to the exigency of each particular case, which would be better than the application of the general method of solution. Some examples of which shall be shown in the following paper: the design of which is to point out a method of solving several of the most useful questions in spherical trigonometry, in a manner somewhat similar to that used in approximating to the roots of algebraic equations. This method is founded on the following

Lemma.—If the radius be supposed equal to unity, the sine of the sum of 2 arcs, α and β, is equal to sin. α + cos. $\alpha \times$ sin. β — sin. $\alpha \times$ vers. sin. β. And its cosine = cos. α — sin. $\alpha \times$ sin. β — cos. $\alpha \times$ vers. sin. β. For let the arc α be RA, fig. 7, pl. 13, and the arc β be AB, their sines Aa, BD, respectively; then Bb, being drawn perpendicular to the radius CR, will be the sine of $\alpha + \beta$. Draw Dp and An parallel to CR. Then, by similar triangles, CA : Ca :: BD : Bp, and CA : Aa :: AD : np. Therefore, Bb $(= \text{A}a + \text{B}p - pn) = \text{A}a + \frac{\text{C}a \times \text{BD}}{\text{CA}} - \frac{\text{A}a \times \text{AD}}{\text{CA}}$; that is, sine $(\alpha + \beta)$ = sin. α + cos. $\alpha \times$ sin. β — sin. $\alpha \times$ vers. sin. β. In the same manner, drawing Dq parallel to Aa, we may prove Cb $(= \text{C}a - bq - aq) = \text{CA} - \frac{\text{A}a \times \text{BD}}{\text{CA}} - \frac{\text{C}a \times \text{AD}}{\text{CA}}$, or cos. $(\alpha + \beta)$ = cos. α — sin. $\alpha \times$ sin. β — cos. $\alpha \times$ vers. β.

In what follows, for brevity sake, the arc is expressed by a Greek letter; its sine by the capital character; and the cosine by the small italic character of the same letter. In this notation, the 2 theorems will stand thus, sin. $(\alpha + \beta)$ $= \text{A} + a\text{B} - \text{A} \times \text{vs.}\,\beta$, and cos. $(\alpha + \beta) = a - \text{AB} - a \times \text{vs.}\,\beta$.

Corol. 1.—Since the tangent is equal to the sine divided by the cosine, we shall have tang. $(\alpha + \beta) = \frac{\text{A} + a\text{B} - \text{A} \times \text{vs.}\,\beta}{a - \text{AB} - a \times \text{vs.}\,\beta} = \frac{\text{A}}{a} + \frac{\text{B}}{a^2} + \frac{\text{A}}{a^3} \times \text{vs.}\,\beta$ nearly.

Corol. 2.—If we change the sign of β, we shall have sin. $(\alpha - \beta) = \text{A} - a\text{B} - \text{A} \times \text{vs.}\,\beta$. Cos. $(\alpha - \beta) = a + \text{AB} - a \times \text{vs.}\,\beta$. And tang. $(\alpha - \beta)$ $= \frac{\text{A}}{a} - \frac{\text{B}}{a^2} + \frac{\text{A}}{a^3} \times \text{vs.}\,\beta$.

By the help of these theorems, knowing nearly what any quantity in a spherical triangle is, we may find its correction, thus: if we have to find the cosine

of an arc, which arc we know is nearly equal to α whose cosine is a. Suppose the arc to be $\alpha - \beta$, and its cosine $a + c$. Then $a + c = \cos.(\alpha - \beta) = a + \text{AB} - a \times \text{vs.}\,\beta$. Therefore, $\text{B} = \frac{c}{\text{A}} + \frac{a}{\text{A}} \times \text{vers.}\,\beta$. The first term $\frac{c}{\text{A}}$ will always give a near approximation to the value of sin. β; and β being found, the correction, $\frac{a}{\text{A}} \times \text{vs.}\,\beta$, or cot. $\alpha \times$ vs. β, may be found and added to it. Among the tables requisite to be used with the Nautical Almanack, is table 4 for parallax, p. 19, which shows the value at sight of such quantities as vs. $\beta \times$ cot. α, the arc β being found in the first column of the table, and α at the top. This table I have calculated only to arcs under 63′; but it would be found useful to have a table ready computed for all arcs under 5°.

PROB. 1.—*If the two legs,* AB *and* BC, *of the spherical triangle* ABC *right-angled at* B, *are given, to find the hypothenuse* AC, *the leg* BC, *being small in comparison of* AC. Fig. 8, pl. 13.—Let AB $= \alpha$, BC $= \beta$, and suppose AC $= \alpha + \zeta$, α being a near approximation to AC, and ζ the small arc to be added to AB to make it equal to AC; then cos. AC = cos. AB × cos. BC; that is, according to our notation, $a - \text{AZ} - (a \times \text{vs.}\,\zeta) = ab$. Whence $\text{Z} = \frac{a - ab}{\text{A}} - (\frac{a}{\text{A}} \times \text{vs.}\,\zeta) = \cot.\,\alpha \times \text{vs.}\,\beta - \cot.\,\alpha \times \text{vs.}\,\zeta$.

Ex.—Let AB be 75° 0′, and BC 20° 0′, then the computation will be as follows:

Cotangent AB		9.4280	
Versed sine BC		8.7804	
Sum ζ nearly 55′ 33″		sine 8.2084	
Correction	− 7	from tab. 4	Nautical Almanack.
Therefore ζ = 55 26, and AC = 75° 55′ 26″.			

By this problem, the distance of the sun may be found from a planet whose latitude and difference of longitude are known.

PROB. 2.—*Having the hypothenuse,* AC, *and one of the angles* A, *to find the base* AB.—Let AC $= \beta$, BAC $= \alpha$, fig. 8, and suppose AB $= \beta - \zeta$, then cos. A = cot. AC × tang. AB, or $a = \frac{b}{\text{B}} \times \frac{\text{B}}{b} - \frac{\text{Z}}{b^2} + \frac{\text{B} \times \text{vs.}\,\zeta}{b^3} = 1 - \frac{\text{Z}}{\text{B}b} + \frac{\text{B} \times \text{vs.}\,\zeta}{b^2}$.

Whence $\text{Z} = \text{B}b \times (1 - a) + \frac{\text{B}}{b} \times \text{vs.}\,\zeta = \frac{1}{2}\,2\beta \times \text{vs.}\,\alpha + \text{tang.}\,\beta \times \text{vs.}\,\zeta$.

Ex.—Let A = 23° 28′ 15″, and AC = 10° 0′ 0″.

Sine 2AC 20° 0′		9.5340
Versed sine A		8.9177
Sum		8.4517
Log. 2		0.3010
Dif. ζ nearly	48′ 39″	sine 8.1507
Correction	+ 6	
ζ	48 45, and BC = 159° 11′ 15″.	

By this problem, the right ascension of any point of the ecliptic, whose obliquity and longitude are known, may be found.

PROB. 3. *Supposing the same things known as in the last, to find the perpendicular* BC, *when the hypothenuse is nearly a quadrant.*—Let A $= \alpha$, AC $= \beta$, fig. 8, as before, and suppose BC $= a - \zeta$; then sin. BC = sin. AC × sin. A, or

A — az — A × vs. ζ = AB, whence $z = \frac{A - BA}{a} - \frac{A}{a} \times$ vs. ζ = tang. α × co. ver. sin. β — t. α × vs. ζ.

Ex.—Let A = 23° 28′ 15″, and AC = 80° 0′.

Tang. A 9.6377
Vers. sin. co. AC, 10°................8.1816
Sum, ζ nearly....22° 41′........sine 7.8193
Correction ...... —1
Gives ζ ....22 40, and BC = 23° 5′ 35″.

This problem will be of use to find the declination of the ecliptic, and the latitude of a planet near the limits. And these 3 instances will suffice for an application of this method to right-angled spherical triangles; we shall now give 2 problems of oblique triangles.

PROB. 4. *Suppose* ABC *to be a spherical triangle, in which are given the two sides* AB, BC, *with the included angle* B, *to find the third side* AC. *Fig.* 9.

Solut. 1. Let ABC = β, BC = α, AB = δ. Put AC = $\beta + \zeta$, β being an approximate value of AC, when the two legs are nearly quadrants. Now the cosine of AC being equal to bDA + da,* we shall have b — BZ — (b × vs) ; ζ = bDA + da: and $z = \frac{b - bDA - da}{B} - \frac{b}{B} \times$ vs. ζ. But I — DA — da = vs. ($\delta - \alpha$), which put = w. Then $z = \frac{bw}{B} + \frac{bda - da}{B} - \frac{b}{B} \times$ vs. ζ = cot. β × vs. ($d - \alpha$) — cos. δ × cos. α × tang. $\frac{1}{2}\beta$ — cot. β × vs. ζ. Therefore ζ is the difference of two arcs whose sines are cot. β × vs. ($\delta - \alpha$), and cos. δ × cos. α × tang. $\frac{1}{2}\beta$, the difference of these two arcs being diminished by the correction cot. β × vs. ζ.

Ex.—Suppose.... B = 51° 12′ 5″
AB = 87 57 51
BC = 87 20 34

Cotangent B................9.9053	Tang. ½ B 25° 36′..........9.6804
Vers. sine AB — BC 0° 37′ 17″..5.7693	Cosine AB...............8.5506
	Cosine BC...............8.6661
Sum = 1st arc 0′ 10″.... sine 5.6746	2d arc 2′ 43″ ...... sine 6.8971

The difference of these two arcs,.................................. 2′ 33″
Subtracted from the value of the angle B, ...51 12 5
Leaves AC, ...51 9 32
The correction cot. β × vs. ζ in this example is 0.

This solution is very convenient to find the distance of two zodiacal stars, having their latitudes and difference of longitude.

Solut. 2. Let T be an arc whose cosine $t = b$ × cos. $\delta - \alpha$ = bda + bDA, and suppose AC = T — ζ, then t + TZ — t × vs. ζ = bDA + da = t — bda + da.

* It is a well known theor. that sin. BA × sin. BC : r^2 = vs. AC — vs. (AB — BC) : vs. B ; that is, sin. BA × sin. BC : r^2 = cos. (AB — BC) — cos. AC : r — cos. B. Or, in the author's notation, putting r = I, DA : I = cos. ($\delta - \alpha$) — cos. AC : I — b. Therefore DA — bDA = cos. ($\delta - \alpha$) — cos. AC. Or, cos. AC = bDA — DA — cos. ($\delta - \alpha$.) For cos. ($\delta - \alpha$) substitute its value as expressed in the second corollary of the lemma, and there arises the author's equation, cos. AC = bDA + da. —Orig. S. HORSLEY.

Whence $z = da \times \frac{1-b}{T} + \frac{t}{T} \times$ vers. $\zeta =$ cosec. $\tau \times$ cosin. $\alpha \times$ cosin. $\delta \times$ vs. $\beta +$ cot. $\tau \times$ vs. ζ.

This solution is useful to find the distance of the moon from a star at some distance from the ecliptic; in which case it coincides with the rule given by the Astronomer Royal, Phil. Trans. 1764, vol. 54, and which, taking in the correction here given, cot. $\tau \times$ vs. ζ, will always be exact to a second. It is also of use to find the declination of a star, whose longitude and latitude and obliquity of the ecliptic are known.

Solut. 3. Let the angle B be small, and the two legs AB, BC, very unequal; then the side AC will be nearly AB — BC. Fig. 10. Put this $= \kappa$, and suppose AC $= \kappa + \zeta$, then cos. AC $= k - KZ - k \times$ vs. $\zeta = ad + AD - KZ - k \times$ vs. $\zeta = bAD + ad$, whence $z = \frac{DA - bAD}{K} - \frac{k}{K} \times$ vs. $\zeta =$ sin. $\delta \times$ sin. $\alpha \times$ vs. $\beta \times$ cosec. $\delta - \alpha -$ cot. $k \times$ vs. ζ.

Ex.—Let AB = 94° 36′ 58″	as in the example to sol. 2.	
BC = 23 28 24		
B = 24 54 24		
AB — BC = 71 8 34	cosecant	0.02396
Sine AB		9.99859
Sine BC		9.60023
Versed of B		8.96851
Sum = ζ nearly 2° 14′ 11″	sine	8.59129

The value of ζ being without the limits of tab. 4, in the tables requisite to be used with the Nautical Almanack, the correction cot. $\kappa \times$ vs. ζ must be computed thus:

Cot. κ...... 9.533
V. sin. ζ.... 6.881

Sum = cor. 0′ 53″, sine 6.414, this subtracted from the first value of ζ, leaves $\zeta = 2°\ 13'\ 18''$, which added to $\delta - \alpha$, gives the side AC $= 73°\ 21'\ 52''$. This solution will help to find the sun's altitude near noon.

I have dwelt the longer on this problem because it is one that is very commonly required in astronomical calculations, and the operation by the rules of spherical trigonometry, in this as well as the next, is rather troublesome.

PROB. 5.—*Supposing the same things given, to find either of the angles, as for instance* C *opposite the side* AB. *Fig.* 10.—We have cot. C = cot. B $\times$ cos. BC — sin. BC $\times$ cot. AB $\times$ cosec. B $= \frac{ba}{B} - \frac{Ad}{BD}$. Let μ be an angle whose cot. $\frac{m}{M}$ = cot. $\beta \times$ sin. $\delta - \alpha \times$ cosec. $\delta = \frac{baD - bAd}{BD}$, and suppose C $= \mu + \zeta$; then cot. C $= \frac{m}{M} - \frac{z}{M^2} + \frac{m.\ \text{vs.}\ \zeta}{M^3} = \frac{baD - Ad}{BD}$. Whence $z = M \times \frac{Ad - bAD}{BD} + \frac{m}{M} \times$ vs. $\zeta = \sin.^2 \mu \times$ sin. $\alpha \times$ cot. $\delta \times$ tang. $\frac{1}{2}\beta +$ cot. $\mu \times$ vs. ζ.

Ex.—Let...... AB = 94° 36′ 58″			
BC = 23 28 54			
B = 24 54 24	....	Cotang.	0.3331770
Dif. AB and BC = 71 8 34	....	Sine	9.9760412
		Cosecant AB	0.0014080
μ = 26 3 44	....	Cot.	10.3166262
Sin. $^2\mu$...... 9.286			
Sin. BE 9.600			
Cot. AB 8.909			
Tang. $\frac{1}{2}$B.... 9.344			

Sum = ζ = 4′ 44″ sine 7.139, this subtracted from μ, leaves the angle C = 25° 59′ 0″.

This problem will be of use to find the right ascension of a star whose longitude and latitude, and obliquity of the ecliptic are known, or to find the sun's azimuth at any hour in a given latitude.

XLVII. *Further Experiments and Observations in a Heated Room. By Charles Blagden, M. D., F. R. S.* p. 484.

On the 3d of April, (says Dr. B.) nearly the same party as before,* together with Lord Seaforth, Sir George Home, Mr. Dundas, and Dr. Nooth, went to the heated room in which the experiments of the 23d of Jan. were made. Dr. Fordyce had ordered the fire to be lighted the preceding day, and kept up all night; so that every thing contained in the room, and the walls themselves, being already well warmed, we were able to push the heat to a much higher degree than before. In the course of the day several different sets of experiments were going on together; but to avoid confusion, it will be necessary to relate each series by itself, without regard to the order of time; beginning with that series which serves as a continuation of our former experiments.

Soon after our arrival, a thermometer in the room rose above the boiling point; this heat we all bore perfectly well, and without any sensible alteration in the temperature of our bodies. Many repeated trials, in successively higher degrees of heat, gave still more remarkable proofs of our resisting power. The last of these experiments was made about 8 o'clock in the evening, when the heat was at the greatest: a very large thermometer, placed at a distance from the door of the room, but nearer to the wall than to the cockle, and defended from the immediate action of the cockle by a piece of paper hung before it, rose 1 or 2 degrees above 260°: another thermometer, which had been suspended very near the door, stood some degrees above 240°. At this time I went into the room, with the addition to my common clothes, of a pair of thick worsted stockings drawn over my shoes, and reaching some way above my knees; I also put on a pair of gloves, and held a cloth constantly between my face and the cockle: all these precautions were necessary to guard against the scorching of the red-hot iron. I remained 8 minutes in this situation, frequently walking

* See the former experiments, p. 604, of this vol. of these Abridgements.

about to all the different parts of the room, but standing still most of the time in the coolest spot, near the lowest thermometer. The air felt very hot, but still by no means to such a degree as to give pain: on the contrary, I had no doubt of being able to support a much greater heat; and all the gentlemen present, who went into the room, were of the same opinion. I sweated, but not very profusely. For 7 minutes my breathing continued perfectly good; but after that time I began to feel an oppression in my lungs, attended with a sense of anxiety; which gradually increasing for the space of a minute, I thought it most prudent to put an end to the experiment, and immediately left the room. My pulse, counted as soon as I came into the cool air, for the uneasy feeling rendered me incapable of examining it in the room, was found to beat at the rate of 144 pulsations in a minute, which is more than double its ordinary quickness. To this circumstance the oppression on my breath must be partly imputed, the blood being forced into my lungs quicker than it could pass through them; and hence it may very reasonably be conjectured, that should a heat of this kind ever be pushed so far as to prove fatal, it will be found to have killed by an accumulation of blood in the lungs, or some other immediate effect of an accelerated circulation;* for all the experiments show, that heating the air does not make it unfit for respiration, communicating to it no noxious quality except a power of irritating. In the course of this experiment, and others of the same kind by several of the gentlemen present, some circumstances occurred to us which had not been remarked before. The heat, as might have been expected, felt most intense when we were in motion; and, on the same principle a blast of the heated air from a pair of bellows was scarcely to be borne; the sensation in both these cases exactly resembled that felt in our nostrils on inspiration. The reason is obvious; when the same air remained for any time in contact with our bodies, part of its heat was destroyed, and consequently we came to be surrounded with a cooler medium than the common air of the room; whereas when fresh portions of the air were applied to our bodies in such a quick succession, that no part of it could remain in contact a sufficient time to be cooled, we necessarily felt the full heat communicated by the stove. It was observed that our breath did not feel cool to the fingers unless they were held very near the mouth; at a distance the cooling power of the breath did not sufficiently compensate the effect of putting the air in motion, especially when we breathed with force.

A chief object of this day's experiments was, to ascertain the real effect of

* Since this experiment, I have observed the mucus from my lungs to be more serous than before, and to incline more to a saltish taste, though the lungs themselves seem perfectly sound in all other respects; which raises a suspicion that some of the smaller arteries suffered a degree of dilatation from the increased impulse of the blood.—Orig.

our clothes in enabling us to bear such high degrees of heat. With this view I took off my coat, waistcoat, and shirt, and in that situation went into the room, as soon as the thermometer had risen above the boiling point, with the precaution of holding a piece of cloth constantly between my body and the cockle, as the scorching was otherwise intolerable. The first impression of the heated air on my naked body was much more disagreeable than I had ever felt it through my clothes; but in 5 or 6 minutes a profuse sweat broke out, which gave me instant relief, and took off all the extraordinary uneasiness: at the end of 12 minutes, when the thermometer had risen almost to 220°, I left the room, very much fatigued, but no otherwise disordered; my pulse made 136 beats in a minute. On this occasion I felt nothing of that oppression on my breath, which became so material a symptom in the experiment with my clothes when the thermometer had risen to 260°: this may be partly explained by the less quickness of my pulse, the difference being at least 8 beats in a minute, and probably more, as in the experiment without my shirt the pulsations were counted before I had left the room; but there is a further circumstance to be taken into consideration, that the experiment attended with oppression on the breath was made in the evening after a very plentiful meal, whereas the other was made in the forenoon, some hours after a moderate breakfast. The unusual degree of fatigue which I felt from the experiment without my shirt, must be ascribed in great measure to the more violent effort which the living powers were obliged to exert, in order to preserve the due human temperature, when such hot air came into immediate contact with my body. In the present case it appears beyond all doubt, that the living powers were very much assisted by the perspiration, that cooling evaporation which is a further provision of nature for enabling animals to support great heats. Had we been provided with a proper balance, it would undoubtedly have rendered the experiment more complete, to have taken the exact weight of my body at going into, and coming out of, the room; as, from the quantity lost, some estimate might be formed of the share which the perspiration had in keeping the body cool; probably its effect was very considerable, but by no means sufficient to account for the whole of the cooling, and certainly not equable enough to keep the temperature of the body to such an exact pitch: for it should here be remarked, that during all the experiments made this day, whenever I tried the heat of my body, the thermometer always came very nearly to the same point; I could not perceive even the small difference of 1 degree, which was observed in our former experiments. Should these considerations however be thought insufficient to prove that evaporation was not the sole agent in keeping the body cool, I believe that Dr. Fordyce's experiments in moist air will be found to remove all doubts on this subject. Several of the gentlemen present, as well as myself, went into the room without shirts many

times afterwards, when the thermometer had risen much higher, almost to 260°, and found that we could bear the heat very well, though the first sensation was always more disagreeable than with our clothes. In all the experiments made this day it was observed, that the thermometer did not sink so much in consequence of our stay in the room as on the 23d of Jan.; probably because a much larger mass of matter had been heated by the longer continuance of the fire.

Our own observations, with those of M. Tillet, in the Mem. of the Acad. of Sciences, for 1764, had given us good reason to suspect, that there must have been some fallacy in the experiment with a dog, made at the desire of Dr. Boerhaave, and related in his Elements of Chemistry. To determine this matter more exactly, we subjected a bitch weighing 32 lb; to the following experiment. When the thermometer had risen to 220°, the animal was shut up in the heated room, inclosed in a basket, that its feet might be defended from the scorching of the floor, and with a piece of paper before its head and breast to intercept the direct heat of the cockle. In about 10 minutes it began to pant and hold out its tongue, which symptoms continued till the end of the experiment, without ever becoming more violent than they are usually observed in dogs after exercise in hot weather; and the animal was so little affected during the whole time, as to show signs of pleasure whenever we approached the basket. After the experiment had continued half an hour, when the thermometer had risen to 236°, we opened the basket, and found the bottom of it very wet with saliva, but could perceive no particular fœtor. We then applied a thermometer between the thigh and flank of the animal; in about a minute the quicksilver sunk down to 110°: but the real heat of the body was certainly less than this, for we could neither keep the ball of the thermometer a sufficient time in proper contact, nor prevent the hair, which felt sensibly hotter than the bare skin, from touching every part of the instrument. I have since found, that the thermometer held in the same place, when the animal is perfectly cool and at rest, will not rise above 101°. At the end of 32 minutes the bitch was permitted to go out of the room; on coming into the cold air she appeared perfectly brisk and lively, not in the least injured by the heat, and has now continued very well above a month. Our experiment therefore differs, in every essential circumstance of the event, from that related by Dr. Boerhaave. With respect to this last it is remarkable, if the facts be properly represented, that an intolerable stench arose from the dog? and that an assistant dropped down senseless on going into the stove.

To prove that there was no fallacy in the degree of heat shown by the thermometer, but that the air which we breathed was capable of producing all the well-known effects of such a heat on inanimate matter, we put some eggs and a beef-steak on a tin frame, placed near the standard thermometer, and farther

distant from the cockle than from the wall of the room. In about 20 minutes the eggs were taken out, roasted quite hard; and in 47 minutes the steak was not only dressed, but almost dry. Another beef-steak was rather over-done in 33 minutes. In the evening, when the heat was still greater, we laid a 3d beef-steak in the same place: and as it had now been observed, that the effect of the heated air was much increased by putting it in motion, we blew upon the steak with a pair of bellows, which produced a visible change on its surface, and seemed to hasten the dressing; the greatest part of it was found pretty well done in thirteen minutes.

About the middle of the day 2 similar earthen vessels, 1 containing pure water, and the other an equal quantity of the same water with a bit of wax, were put upon a piece of wood in the heated room. In $1\frac{1}{2}$ hour the pure water was heated to 140° of the thermometer, while that with the wax had acquired a heat of 152°, part of the wax having melted and formed a film on the surface of the water, which prevented the evaporation. The pure water never came near the boiling point, but continued stationary above an hour at a much lower degree; a small quantity of oil was then dropped into it, as had before been done to that with the wax; in consequence of which, the water in both the vessels came at length to boil very briskly. A saturated solution of salt in water, put into the room, was found to heat more quickly, and to a higher degree, than pure water, probably because it evaporated less; but it could not be brought to boil till oil was added, by means of which it came towards evening into brisk ebullition, and consequently had acquired a heat of 230°. Some rectified spirit of wine in a bottle slightly corked, which had been immersed into this solution of salt while cold, began to boil in about 2 hours, and soon afterwards was totally evaporated. Perhaps no experiments hitherto made, furnish more remarkable instances of the cooling effect of evaporation, than these last facts; a power which appears to be much greater than has commonly been suspected. The evaporation itself, however, was more considerable in our experiments than it can be in almost any other situation, because the air applied to the evaporating surface was uncommonly hot, and at the same time not more charged with moisture than in its ordinary state. A powerful assistant evaporation must undoubtedly prove, in keeping the living body properly cool, when exposed to great heats; but it can act only in a gross way, and by no means in such a nice proportion to the momentary exigencies of the animal, as would be requisite for the exact preservation of its temperature: that other provision of nature which seems more immediately connected with the powers of life, is probably the great agent in preserving the just balance of temperature; exerting a greater effort in proportion as the evaporation is deficient, and a less effort as the evaporation increases. This idea corresponds with the general analogy of the animal economy, the nicer balances of which

are almost universally effected in that part of the body which is formed with the most subtle organization.

The heated room will, I hope, in time become a very useful instrument in the hands of the physician. Hitherto the necessary experiments have not been made to direct its application with a sufficient degree of certainty. However, we can already perceive a foundation for some distinctions in the use of this uncommon remedy. Should the object in view be to produce a profuse perspiration, a dry heat acting on the naked body would most effectually answer that purpose. The histories of dropsies and some other diseases, supposed to have been cured by such means, are well known to every physician. In some cases also, a moist heat, and in others heat transmitted through a quantity of clothes, might have their peculiar advantages. That the danger likely to ensue from such applications is less than has been commonly apprehended, our former experiments gave sufficient reason to believe; and the same was amply confirmed by those which make the subject of this paper. For during the whole day, we passed out of the heated room, after every experiment, immediately into the cold air, without any precaution; after exposing our naked bodies to the heat, and sweating most violently, we instantly went out into a cold room, and staid there even some minutes before we began to dress; yet no one received the least injury. I felt nothing this day of the noise and giddiness in my head, which had affected me in making the former experiments; and, whether from the force of habit, or any other cause, the shaking of our hands was less, and we felt less languor, though the heat had been so much more intense.

XLVIII. A Proposal for Measuring the Attraction of some Hill in this Kingdom by Astronomical Observations. By the Rev. Nevil Maskelyne, B. D., F. R. S., and Astronomer Royal. p. 495.

If the attraction of gravity be exerted, as Sir Isaac Newton supposes, not only between the large bodies of the universe, but between the minutest particles of which these bodies are composed, or into which the mind can imagine them to be divided, acting universally according to that law by which the force which carries on the celestial motions is regulated; namely, that the accelerative force of each particle of matter, towards every other particle, decreases as the squares of the distances increase; it will necessarily follow, that every hill must, by its attraction, alter the direction of gravitation in heavy bodies in its neighbourhood, from what it would have been from the attraction of the earth alone, considered as bounded by a smooth and even surface. For, as the tendency of heavy bodies downwards, perpendicular to the earth's surface, is owing to the combined attraction of all the parts of the earth on it, so a neighbouring mountain ought, though in a far less degree, to attract the heavy body towards its centre of at-

traction, which cannot be placed far from the middle of the mountain. Hence the plumb-line of a quadrant, or any other astronomical instrument, must be deflected from its proper situation, by a small quantity towards the mountain; and the apparent altitudes of the stars, taken with the instrument, will be altered accordingly.

It will easily be acknowledged, that to find a sensible attraction of any hill, from undoubted experiment, would be a matter of no small curiosity, would greatly illustrate the general theory of gravity, and would make the universal gravitation of matter as it were palpable, to every person, and fit to convince those who will yield their ascent to nothing but downright experiment. Nor would its uses end here, as it would serve to give us a better idea of the total mass of the earth, and the proportional density of the matter near the surface, compared with the mean density of the whole earth. The result of such an uncommon experiment, which I should hope would prove successful, would doubtless do honour to the nation where it was made, and the society which executed it.

Sir Isaac Newton gives us the first hint of such an attempt, in his popular Treatise of the System of the World, where he remarks, "That a mountain of an hemispherical figure, 3 miles high and 6 broad, will not, by its attraction, draw the plumb-line 2 minutes out of the perpendicular." It will appear, by a very easy calculation, that such a mountain would attract the plumb-line $1' \, 18''$ from the perpendicular.

But the first attempt of this kind was made by the French academicians, who measured 3 degrees of the meridian near Quito in Peru, and who endeavoured to find the effect of the attraction of Chimboraço, a mountain in that neighbourhood, which is elevated near 4 miles above the sea, though only about 2 miles above the general level of the province of Quito. By their observations of the altitudes of fixed stars, taken with a quadrant of $2\frac{1}{2}$ feet radius, they found the quantity of $8''$ in favour of the attraction of the mountain, by a mean of their observations. This indeed was much less than they expected; but then it is to be considered, that their instrument was too small and imperfect for the purpose; and that they themselves were subject to great inconveniencies, being sheltered from the wind and weather by nothing but a common tent, and placed so high up the mountain as the boundary where the snow begins to lie unmelted all the year round. And indeed their observations, doubtless owing to these causes of error, differ greatly from each other, and are therefore insufficient to prove the reality of an attraction of the mountain Chimboraço, though the general result from them is in favour of it. Accordingly, one of the French gentlemen themselves, M. Bouguer, who drew up the account of their experiment, expresses his wishes, that a like experiment might be made, to find the

attraction of a mountain in France or England, where he thinks some might be found of sufficient bulk for the purpose. This experiment and these remarks were made in the year 1738, or above 30 years ago, yet I believe no similar experiment has ever been made in Europe.

I have made inquiries after a proper hill in this kingdom, for trying so curious an experiment, and have been informed of 2 places in particular, very convenient for the purpose. The one is situated on the confines of Yorkshire and Lancashire; where, within the compass of 20 miles, are situated 4 very remarkable hills, called Pendle-hill, Pennygant, Ingleborough, and Whernside, which have been estimated to be from 600 to 750 yards elevated above the plane of the vales between them. By calculation on these data it follows, that the sum of the contrary attractions of Whernside, the largest of these hills, on the plumb-line placed half-way up the hill, would not be less than 30″, and might amount to 46″, which it is evident is a very considerable quantity, and sufficient to give room to hope for a favourable and satisfactory success of the experiment. The other place pointed out for this purpose, is a valley 2 miles broad, between the hills Helwellin and Skidda, in Cumberland: which hills, according to a plan of them and the adjacent country, communicated by Mr. Smeaton, F.R.S., are elevated more than 1000 yards above the intermediate valley By a calculation made according to this plan, the sum of the contrary attractions of the plumb-line, placed alternately on the north side of Helwellin and the south side of Skidda, amounts to about 20″, which is likewise a quantity large enough for the experiment. And though the density of the earth near the surface should be 5 times less than the mean density, as there is some reason to suspect, and the attractions, as here stated, should consequently be diminished in the proportion of 5 to 1, still the sum of the contrary attractions of Whernside would be 6″ or 9″, and the sum of the contrary attractions of Helwellin and Skidda would be 4″; which quantities are not too small to be measured and demonstrated by an accurate zenith sector, such as that belonging to the R. S., which I made use of at St. Helena, would be, if the fault in the suspension of the plumb-line, which I there discovered, was corrected, in the manner suggested in the Philos. Trans., vol. 54, p. 351.

XLIX. An Account of Observations made on the Mountain Schehallien for finding its Attraction. By the Rev. Nevil Maskelyne, B. D., F. R. S., and Astronomer Royal.* p. 500.

In the year 1772, I presented the foregoing proposal, for measuring the at-

* For this paper Dr. Maskelyne was honoured with the Society's gold medal. And the calculation of the earth's density, from these observations, amply confirmed the expectations and predictions of it; as fully appears in a future volume of this work.

traction of some hill in this kingdom by astronomical observations, to the R. S.; who, ever inclined to promote useful observations which may enlarge our views of nature, honoured it with their approbation. A committee was in consequence appointed, of which number I was one, to consider of a proper hill on which to try the experiment, and to prepare every thing necessary for carrying the design into execution. The Society was already provided with a 10-feet zenith sector made by Mr. Sisson, furnished with an achromatic object glass, the principal instrument requisite for this experiment, the same which I took with me to St. Helena in the year 1761; which wanted nothing to make it an excellent instrument but to have the plumb-line made adjustable, so as to pass before and bisect a fine point at the centre of the instrument. This was ordered to be done, and a new wooden stand provided for it, capable of procuring a motion of the sector about a vertical axis, by means of which it could be more easily brought into the plane of the meridian, or turned half round for repeating the observations with the plane of the instrument placed the contrary way, in order to find the error of the line of collimation. A large parallelopiped tent, 15½ feet square and 17 feet high, was also provided for sheltering both the instrument and the observer who should use it, composed of joists of wood well framed together, and covered with painted canvas. The Society was also possessed of most of the other instruments requisite for this experiment; as, an astronomical quadrant and transit instrument made by Mr. Bird, and an astronomical clock by Shelton, which had all been provided on occasion of the observations on the transit of Venus in 1761 or 1769. A theodolite of the best sort was wanting, a necessary instrument for obtaining the figure and dimensions of the hill. One of Mr. Ramsden's construction of 9 inches diameter, was thought the fittest for the purpose, on account of the excellence of the plan on which it was made, and the number of its adjustments, being capable of measuring angles for the most part to the exactness of a single minute. The other instruments prepared for this business were, 2 barometers of M. de Luc's construction, made by Mr. Nairne; a common Gunter's chain; a roll of painted tape 3 poles long, having feet and inches marked on it; 2 fir poles of 20 feet each, and 4 wooden stands, for supporting them when used in measuring the bases, and a brass standard of 5 feet, for adjusting them. The poles and stands were provided on the spot.

Though accounts had been received from various persons of several hills supposed proper for the intended purpose, some better and some worse authenticated; yet, in order to be sure of finding the best hill for the experiment, it was determined to send a person furnished with proper instruments, to make such observations on various hills in England and Scotland, as might enable us to choose the fittest for the purpose. Accordingly Mr. Charles Mason, who had been employed on several astronomical occasions by the R. S., was appointed to make

a tour through the Highlands of Scotland in the summer of the year 1773, taking notice of the principal hills in England which lay in his route, either in his going or in his return. It appeared from his observations, that scarcely any hill was so well adapted to the purpose as our sanguine hopes had led us to expect; for either they were not high enough, or not sufficiently detached from other hills, or their greatest length fell in a wrong direction, too near the meridian, instead of lying nearly east and west, which is a circumstance requisite to make a hill of a given height afford the greatest effect of attraction. In particular, the hills on the confines of Yorkshire and Lancashire, mentioned in the foregoing proposal, were found not to answer the description that had been given of them. Fortunately however Perthshire afforded a remarkable hill, nearly in the centre of Scotland, of sufficient height, tolerably detached from other hills, and considerably larger from east to west than from north to south, called by the people of the low country Maiden-pap, but by the neighbouring inhabitants, Schehallien; which, I have since been informed, signifies in the Erse language, constant storm; a name well adapted to the appearance which it so frequently exhibits to those who live near it, by the clouds and mists which usually crown its summit. It had also the advantage, by its steepness, of having but a small base from north to south; which circumstance, at the same time that it increases the effect of attraction, brings the two stations on the north and south sides of the hill, at which the sum of the two contrary attractions is to be found by the experiment, nearer together; so that the necessary allowance of the number of seconds, for the difference of latitude due to the measured horizontal distance of the two stations, in the direction of the meridian, would be very small, and consequently not subject to sensible error from any probable uncertainty of the length of a degree of latitude in this parallel. For these reasons the mountain Schehallien was chosen, in preference to all others, for the scene of the intended operations, and it was concluded to make the experiment in the summer of the year 1774.

It was foreseen that this experiment would be attended with considerable expence, and such as might easily have exceeded the common funds of the R. S., without some extraordinary assistance. The bounty of his majesty, our patron, happily removed this difficulty. At the humble request of the Society, his majesty had been graciously pleased to grant a very ample sum to their disposal, for defraying the expences of the observations of the late transit of Venus in 1769, as his majesty had before done with respect to the former transit of Venus in 1761. Out of this benefaction, after all expences had been paid, there was a considerable remainder; and, the Society humbly requesting to know his majesty's pleasure about the disposal of it, he was graciously pleased to direct them, to lay it out in such manner as they thought proper, and was most agreeable to

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the end of their institution. As this bounty of his majesty had been originally granted for an astronomical purpose, the Society thought they could not dispose of it on any more important object, or in any manner more consistent with the intentions of their royal patron and benefactor, than by expending it on this astronomical experiment of the attraction of a mountain, as what could hardly fail of throwing light on the principle of universal gravitation, and was likely to lead to new discoveries concerning the constitution of this earth which we inhabit, particularly with respect to the density of its internal parts.

The experiment being thus resolved on, the next thing to be done was to fix on a proper person to carry it into execution. Numerous and interesting as my literary engagements are at the Royal Observatory, I had no thoughts of undertaking this care and labour myself, till the council of the R. S. were pleased to do me the honour to think my assistance necessary to insure the success of so important and delicate an experiment. Their thinking so was a sufficient motive with me to encounter whatever difficulties and fatigues might attend operations carried on in so inconvenient and inclement a situation. But it was requisite I should also have his majesty's permission for absenting myself so long from my duty at the Royal Observatory. This his majesty was graciously pleased to grant, and to allow me to stay as long as I thought necessary, to complete my very important observations. Such were the motives for undertaking this experiment, and the preparations made for putting it in execution. I am now to give an account of the operations themselves.

The quantity of attraction of the hill, the grand point to be determined, is measured by the deviation of the plumb-line from the perpendicular, occasioned by the attraction of the hill, or by the angle contained between the actual perpendicular and that which would have obtained if the hill had been away. The meridian zenith distances of fixed stars, near the zenith, taken with a zenith sector, being of all observations hitherto devised capable of the greatest accuracy, ought by all means to be made use of on this occasion: and it is evident, that the zenith instrument should be placed directly to the north or south of the centre of the hill, or nearly so. In observations taken in this manner, the zenith distances of the stars, or the apparent latitude of the station, will be found as they are affected by the attraction of the hill. If then we could by any means know what the zenith distances of the same stars, or what the latitude of the place would have been, if the hill had been away, we should be able to decide on the effect of attraction. This will be found, by repeating the observations of the stars at the east or west end of the hill, where the attraction of the hill, acting in the direction of the prime vertical, has no effect on the plumb-line in the direction of the meridian, nor consequently on the apparent zenith distances of the stars; the differences of the zenith distances of the stars taken on the

north or south side of the hill, and those observed at the east or west end of it, after allowing for the difference of latitude answering to the distance of the parallels of latitude passing through the two stations, will show the quantity of the attraction at the north or south station. But the experiment may be made to more advantage on a hill like Schehallien, which is steep both on the north and south sides, by making the two observations of the stars on both sides; for the plumb-line being attracted contrary ways at the two stations, the apparent zenith distances of stars will be affected contrary ways; those which were increased at the one station being diminished at the other, and consequently their difference will be affected by the sum of the two contrary attractions of the hill. On the south side of the hill, the plumb-line being carried northward at its lower extremity, will occasion the apparent zenith, which is in the direction of the plumb-line, continued backwards, to be carried southward, and consequently to approach the equator; and therefore the latitude of the place will appear too small by the quantity of the attraction; the distance of the equator from the zenith being equal to the latitude of the place. The contrary happens on the north side of the hill; the lower extremity of the plumb-line, being there carried southward, will occasion the apparent zenith to be carried northward, or from the equator; and the latitude of the place will appear too great by the quantity of the attraction. Thus the less latitude appearing too small by the attraction on the south side, and the greater latitude appearing too great by the attraction on the north side, the difference of the latitudes will appear too great by the sum of the two contrary attractions; if therefore there is an attraction of the hill, the difference of latitude by the celestial observations ought to come out greater than what answers to the distance of the two stations measured trigonometrically, according to the length of a degree of latitude in that parallel, and the observed difference of latitude subtracted from the difference of latitude inferred from the terrestrial operations, will give the sum of the two contrary attractions of the hill. To ascertain the distance between the parallels of latitude passing through the two stations on contrary sides of the hill, a base must be measured in some level spot near the hill, and connected with the two stations by a chain of triangles, the direction of whose sides, with respect to the meridian, should be settled by astronomical observations.

If it be required, as it ought to be, not only to know the attraction of the hill, but also from it the proportion of the density of the matter of the hill to the mean density of the earth; then a survey must be made of the hill, to ascertain its dimensions and figure, from which a calculation may be made, how much the hill ought to attract, if its density was equal to the mean density of the earth; it is evident, that the proportion of the actual attraction of the hill, to that computed in this manner, will be the proportion of the density of the hill to the mean density of the earth.

Thus there were three principal operations requisite to be formed. 1. To find by celestial observations the apparent difference of latitude between the two stations, chosen on the north and south sides of the hill. 2. To find the distance between the parallels of latitude. 3. To determine the figure and dimensions of the hill.

I arrived at the hill of Schehallien on the last day of June, and found the observatory and instruments there, which had been brought down some time before from London to Perth on board a ship, and thence conveyed over land to the hill under the care of Mr. Reuben Burrow, my late assistant at the Royal Observatory. The observatory was fixed half-way up the south side of the hill; as the place where the effect of the hill's attraction would be at the greatest, and it was placed in the like manner when it was afterwards removed to the north side. A circular wall was raised, 5 feet in diameter, and covered at top with a moveable conical roof for sheltering the astronomical quadrant; and a square tent was set up for receiving the transit instrument, all near the observatory. A bothie, or temporary hut, was also made near it, for my residence, while attending the astronomical observations on this side of the hill. I first put the sector, nearly in the meridian, by means of the variation compass; but, through the badness of the weather, which was almost continually cloudy or misty, I could not before the middle of July get a sufficient number of observations with the astronomical quadrant, to know the state of the clock, in order to draw a meridian line on the floor of the observatory, for setting the sector truly in the plane of the meridian. The first observations which I made with the sector, after it was set truly in the meridian, were on the 20th of July. Between this time and the end of the month, I observed the zenith distances of 34 stars, some to the north and some to the south of the zenith; and many of them several times over, having taken 76 observations in all, with the plane of the sector turned to the east. On the first of August I turned the plane of the instrument about, to face the west, and set it in the meridian again, by means of the meridian line drawn on the floor the 26th of July, and secured by piquets driven into the ground; and between that and the 15th of the same month, I observed 39 stars, including most of those taken in the former position of the instrument, and took 93 observations in all.

And here let me take notice of a method, which I fell upon, of verifying the position of the sector, with respect to the plane of the meridian, which, had I thought of it at first, would have saved me much trouble; and therefore I will now mention it, as it may be useful to future observers. It consists in observing the transits of two stars, differing considerably in declination from each other, across the vertical wire of the sector, and comparing the observed difference of their transits with the known difference of their right ascensions. If they agree,

it may be safely concluded, that the instrument is truly placed in the meridian. If not, by comparing the alteration that would be produced in the difference of the transits, by supposing the instrument out of the meridian, by any small quantity, as 1 degree or 10 minutes with the observed error, the deviation of the instrument from the meridian may be inferred. In this manner I found that the instrument had been set very exactly in the meridian, by means of the meridian line; the difference by the two methods coming out only $2\frac{1}{2}$ minutes of azimuth. As to the continuance of the instrument in the plane of the meridian, I had a constant proof of it by the same means, and also a further security, which I did not fail to attend to, by noting the degree and minute which an index depending on the vertical axis of the instrument pointed out on a fixed azimuth circle. Being apprehensive of error in an instrument supported on a wooden frame, I frequently examined the parallelism of the fore arch to the back arch, by measuring their perpendicular distances at the two ends with a brass scale, whose vernier showed the 500th part of an inch, and found it liable to variations of a minute or two, owing probably to the force used in setting the sector to different zenith distances, and the weakness of some screws at the top of the frame; which small error I corrected, till I found it liable to continual returns: and I satisfied myself, that the plane of the sector never deviated above 3 minutes from the meridian, in any of the observations taken on the south side of the hill, which it is evident could not in the least affect the observed zenith distances of stars. I hardly ever observed, without examining the bisection of the point at the centre of the instrument, by the plumb-line; which was absolutely necessary, on account of the gradual changes of the wooden frame. My view in mentioning these minute circumstances, is to caution future observers, as well as to confirm my own observations. But whoever makes use of an instrument of this kind, supported on a wooden frame, will find the greatest attention necessary to attain the same degree of accuracy in his observations, as if his instrument was fixed to an immoveable wall. In the mean time, by observations taken with the quadrant and transit instrument, I got a meridian line, and planted a pole to preserve it on the top of the hill, to the south of the instrument, and another at the foot of the same hill; from which, by measuring off an equal distance to the east (as the south-west corner of the observatory lay to the east of the transit instrument) and setting up another pole, another meridian line was gotten, passing through the south-west corner of the southern station of the observatory. The reason for making the meridian line pass through the south-west corner of the observatory, rather than through the middle of it, was, that this part of it had been taken when the observatory had been used as an object in taking angles by the theodolite, in the survey of the hill.

While I was engaged in these astronomical observations, Mr. Burrow, at-

tended by Mr. William Menzies, a land surveyor in the neighbourhood, who had been recommended by some of the principal gentlemen of the country, as a proper person for this work, went out every day that the weather permitted, to take sections of the hill, and angles between several objects, for determining its figure and dimensions. The method made use of was this, which was proposed by Mr. Burrow, and was well adapted to the purpose. A number of station poles were set up at convenient distances all round the foot of Schehallien; but rather without its base, and chiefly on little eminencies rising from the foot of it, which formed a polygon of many sides, surrounding the hill; and when delineated on paper, show very nearly the shape of its base. At each station, the angular position of 2 or more of the other stations being observed with the theodolite, and one side being determined by means of a measured base, all the other sides will be known. From these stations, sections of the hill up to the top were taken in the following manner. The theodolite, being placed at any station, was pointed towards the hill; and a labourer was sent with a number of poles, which he was to plant in the ground truly upright, at regular distances and in a vertical plane, according to signals which he received from the person that stood at the theodolite, who also took the altitude of the foot of each pole, and the horizontal angle contained between the plane of the section poles and the next station pole to the right or left. The theodolite was then removed, and planted directly over the centre of this station pole, which was removed for this purpose; and the horizontal angle taken between a pole now planted at the first station and each of the poles of the section. The horizontal distance of the 2 station poles being known, the horizontal distance of each of the section poles from the first station, and their respective perpendicular altitudes above it, or depth below it, will be given.

It is manifest that these operations, when connected by angles with the 2 stations of the observatory and the meridian line, would at the same time give the shape and dimensions of the hill, and the distance of the parallels of latitude passing through the 2 stations of the observatory, as well as their respective elevations above the base of the hill. But errors being apt to accumulate in a long chain of triangles; to obviate this danger, as well as to produce a check on any great mistakes, that might happen to be made in reading off, or writing down, the angles, I caused a heap of stones, or cairn, as it is called by the people of the country, to be raised in a circular figure 6 feet high, at the highest point of the ridge of the hill, which is to the west of it, as a signal to be observed from the several angles of the polygon, and as a means of connecting the 2 stations of the observatory by a smaller number of triangles. Another cairn towards the eastern end of the ridge of the hill was afterwards set up for the like purpose. I proposed to determine the distance of the 2 cairns by connecting

them by angles with a base, to be measured in a level spot in the vale below the hill, and then to make use of the said distance as a secondary base for determining the sides of the polygon, and the distance of the 2 stations of the observatory. Had the 2 cairns been visible from the 2 stations of the observation, 2 triangles would have sufficed for connecting the 2 stations together. But notwithstanding that this was not the case, and that only the 2 cairns were visible from each other, yet all the angles of these 2 triangles were measured by Mr. Burrow in the following method, suggested by himself. He went with the theodolite to the neighbouring hill on the south side of Schehallien, which runs parallel to it; and, by varying his situation, found a point whence the western cairn and southern observatory appeared by the theodolite to be in one vertical plane; and, removing the theodolite, he planted a pole there. In like manner he planted another pole on the same hill, in a vertical plane with the southern observatory and eastern cairn. Then returning to the observatory, he took the horizontal angle contained between the 2 poles, which it is evident is equal to its opposite angle, or that contained between the cairns. And going to the west cairn, he took the angle contained between the east cairn and the pole planted on the opposite hill, in a line with the southern observatory and west cairn, which is the same with the angle between the east cairn and southern observatory. And lastly, going to the east cairn, he took the angle contained between the western cairn and the pole placed on the opposite hill in a line with the east cairn and southern observatory. Thus were the 3 angles found of the triangle made by the southern observatory and 2 cairns. In the like manner were the angles of the triangle made by the northern observatory and 2 cairns found afterwards. And, as a proof that the angles of the 2 triangles were rightly determined, their sum in the first case differed from 180° by little more than 2 minutes; and in the second case by only half a minute.

Notwithstanding the advantages which attended this method of finding the distance of the 2 stations of the observatory, I thought it proper to make use also of the other method of doing the same thing by a small number of triangles, carried directly across the hill, thinking it expedient, in a matter of such consequence, to rely on no single operation; but, as far as possible, to confirm every deduction by another found in an independent manner. I had caused 2 poles to be set as far up the hill of Schehallien as they could be placed; one as near the western, and the other the eastern cairn, as they could be, so as to be visible from the southern station of the observatory: also 2 others in like manner visible from the north observatory; one of which was very near the east cairn, and the other only 269 feet distant from the westernmost of the 2 poles visible from the south observatory; so narrow was the ridge of the hill in that part, though it grew wider both to the west and east, but much more

towards the latter. With these 4 poles, the east cairn, and the 2 stations of the observatory, 5 triangles were formed, connecting the 2 stations of the observatory, the relative situation of which to each other would be determined as soon as the length of any one of the sides of these triangles was known, either by comparing it with a base measured in the valley below, or with the distance of the 2 cairns settled in that manner.

I had got sufficient observations of zenith distances of stars with the sector on the south side of the hill by the 15th of August; I prepared therefore for removing the observatory and instruments to the new station on the north side. This was a work of great labour and difficulty, as every thing was carried over the ridge of the hill on men's shoulders, and some of the packages were very weighty; it employed the labour of 12 men for a week, and was completed on the 26th. A large level area had been cut away, with great labour, here, in the side of the hill, for receiving the observatory, as had before been done on the south side of the hill. A new bothie was also erected, and places for holding the quadrant and transit instrument, as before, adjoining to the observatory.

The badness of the weather prevented me from beginning my observations with the sector till the 4th of September; but, that being a clear night, I had a fair opportunity of putting in practice the method of bringing the instrument into the meridian by the transits of the stars across the plane of the sector, before-mentioned. The sector being put up with its plane facing the west, and set near the meridian by the variation compass, allowing for the variation, I found, by the transit of o Draconis, on the north side of the zenith, compared with those of κ, ι and θ Cygni on the south side, that the instrument deviated $49\frac{1}{2}$ minutes to the west of the south in azimuth; which being corrected, by turning the instrument about on its vertical axis, towards the east, by the help of the divisions on the azimuth circle; I then found by the transit of η Cephei, compared with that of π Cygni on the south side, that the instrument deviated 7 minutes to the east of the south in azimuth, which I corrected accordingly. And so near was it brought to the meridian in this manner, that by the most exact comparison of the transits of several stars on the 7th and 8th instant, it appeared to be only 2 minutes out of the meridian, and that to the east of the south; which small error I also attempted to correct; the instrument rested 1 minute out of the position which I intended to give it, owing to the difficulty of turning it about to such great nicety, and so I let it remain.

It was indeed a most fortunate circumstance, that I thus got the instrument so near the meridian by the very first night's observations, those of September 4th; for the badness of the weather in the day prevented me from getting a meridian line by the sun till the 15th. Had I therefore been obliged to wait

for setting the instrument by the sun, I should have lost 4 good days observations, which were $\frac{2}{3}$ of those I took on this side of the hill, with the plane of the instrument turned to the west, and been retarded near 3 weeks in my observations; and, as the opportunities of weather fit for observing at all were but very rare, I might have been thrown back into the winter, and defeated of making so complete a set of observations on the north side of the hill as I had got on the south side, whose correspondence would thus have been rendered less perfect. I had the satisfaction, however, when I drew the meridian line on the floor of the observatory, by the equal altitudes of the sun taken on the 15th, to find it agree perfectly, even to the same minute, with the position of the instrument, as determined by the transits of the stars. But no one will doubt of the superior ease and readiness afforded by the latter method in preference to the other.

On the 20th of September I completed the observations with the plane of the sector, turned to the west, having observed 32 stars, and taken 68 observations in all. On the 22d, I turned it about with the plane to face the east, and set it again in the meridian, by putting it parallel to its former position, by means of the meridian line secured by marks made on picquets let into the ground perpendicularly below the plane of the instrument, before it was turned. Between this time and the 24th of October, I observed 37 stars, and took 100 observations in all, with the plane of the instrument facing the east: and thus I completed my whole series of observations with the sector, having observed 43 different stars in all, on both sides of the hill, and taken 337 observations.

As a few observations, taken with so excellent an instrument as this zenith sector, would have been sufficient to determine the apparent difference of latitude of the 2 stations of the observatory, to a second or 2; I am apprehensive I may be thought by many to have multiplied observations unnecessarily. However that may be, I apprehend, that doubling the observations in each station of the observatory, by taking them with the plane of the instrument alternately facing the east and west, will be allowed to be a proper step, as the line of collimation of the instrument is thus separately determined at each station, and so all danger of any alteration happening in the same, in its removal from one side of the hill to the other, is entirely obviated. I had indeed all the reason in the world to think, that the sector was carried from one station to the other without the least accident: but still it was proper to guard against what was possible to happen.

But I had reasons also for multiplying the observations made in the same position of the instrument. It was important to demonstrate the exactness of the instrument from the near agreement of a number of observations taken with it, as its excellence was not to be entirely presumed, unless this proof could be

shown in its favour. Besides, it might be expected that some unsteadiness or warping of the wooden stand, on which it was supported, might affect the accuracy of the observations; or there might be variable and discordant refractions, even near the zenith, on the side of so steep a hill, more than are found in lower situations. Add to this, that when I began my observations on the south side of the hill, having a prospect of bad weather before me, and not knowing how few observations I might be able to get on either side of the hill, I thought it prudent to endeavour to observe most of the stars in the British catalogue, which came within the reach of the instrument, that I might be sure of being provided with observations of some at least of the same stars, which I might afterwards observe when I should be removed to the north side of the hill; where, after an interval of perhaps some months, many stars, that before passed the meridian in the night, would pass it in the day, and consequently be either invisible through the telescope of the sector, or more precarious of being seen.

Though a meridian line had been found by the transit instrument at the south observatory, by which the relative situation of the 2 stations of the observatory, as well as of the other points of the hill, with respect to the meridian, might be determined; yet I judged it would be more satisfactory to confirm this by another meridian line drawn at the northern observatory. This I found, as I had done the former, by setting the transit instrument to agree with the pole star at the computed time of its passing the meridian, and confirmed it by comparing the difference of the transits of the pole star and of α Pegasi, α Andromedæ, and γ Pegasi, with their difference of right ascension, in the same manner by which I had put the sector in the plane of the meridian, and found it to agree with the former meridian line within 2 minutes.

It remains to give an account of the manner in which the two bases were measured; one in a level spot at the foot of the hill, to the southward; and the other at the distance of about $2\frac{1}{2}$ miles from the hill to the north west, in the plain of Rannoch. I caused 2 measuring poles to be made of straight grained well seasoned fir, in the form of square tubes, 3 inches square and 20 feet long, and strengthened with square pieces within side at several distances. These were carefully compared with the brass standard made by Mr. Bird, the same which was used in the measure of the degree at Pennsylvania, immediately before they were applied to the measure of the bases, and the height of the thermometer noted at the time, in order to make allowance for the expansion or contraction of the brass standard by heat or cold. Four wooden stands were provided for supporting the poles; each having a triangular base with 3 iron spikes beneath, at each of the angles. An upright pole, 6 feet high, rose from the middle of one side of the triangle, and 2 short braces were joined to it

from the 2 ends of this side, and a long slant pole from the opposite angle. Two sliding arms were put on the upright pole, capable of being raised or depressed, one above and the other below the place where the slant pole was fastened to the upright pole, for supporting the measuring poles at a convenient height above the ground. In measuring the base, one end of a pole was supported on one of the stands, and the other end on another stand; and it was set horizontal by means of a spirit level laid on it about the middle, and by raising or depressing the arm on which it rested at one or the other end. The other pole was then, in like manner, supported on the 2 other stands truly level, and in the same vertical plane with the former pole, namely, that of the intended base; without regarding whether they were exactly of the same height, and with some small horizontal interval between their ends. This interval was measured by laying one leg of a brass rectangle, which was divided into inches and tenths, along one pole, while the other, or vertical leg, touched the end of the other pole; for it was not thought advisable, to bring the ends of the poles to touch exactly, as that would have taken up a great deal of time, and might have endangered the altering the position of the hindermost pole, if it should chance to receive any shock by laying down the foremost pole. It is evident that the inches and tenths given by the divisions of the brass rectangle are to be added into one sum together with the poles, in computing the length of the base. When the foremost pole was truly placed, and the interval between them had been measured by the divided side of the brass rectangle, the hindermost pole was taken up, and the stands on which it had rested were advanced forwards, and the pole again laid on them, truly level, and in the true direction of the base. In order to set the poles continually in the proper direction of the base, the following method was used. The theodolite was first set up at one end of the base, and an upright pole at the other, and another in the middle, and a third was from time to time advanced to a little distance forward; and the measuring poles were sometimes placed in the proper direction by the eye, looking along the lengths of both poles together to the upright pole before them, and sometimes by the help of the theodolite. In this manner, about the middle of September, a base was measured by Mr. Burrow and Mr. Menzies of 3012 feet, in the valley at the foot of the hill to the south west; but not so accurately as this method is capable of, owing to the stands being very unsteady, through the looseness of the spikes in the feet and other faults, during the measuring the first quarter of the base, though they were mended before the mensuration of the remainder of it. The mensuration of another base of the length of 5897 feet, in the meadow of Rannoch, about $2\frac{1}{2}$ miles to the north-west of the centre of the hill, which I attended myself, was performed with the greatest accuracy, according to the same method, on the 10th, 11th, and 12th of Oct., with new stands, more substantial and firm than the former.

The extreme badness of the weather no less retarded the operations of the survey than the celestial observations; for there was almost constant rain, mist, or high wind, to obstruct the use of the theodolite: indeed all the people of the country agreed, it was the worst season that had ever been known. So that it was not till the 20th of October that the sections had been carried all round the hill. Nor would this work have been so much forwarded as it was, had it not been for the use of an additional theodolite of the same construction, and by the same maker, as the former, which was lent me, on my request, by the right honourable James Stuart Mackenzie, lord privy seal for Scotland; who, having long cultivated a distinguished taste for astronomy, was pleased to honour the experiment of attraction with every assistance which his interest or recommendation could procure. I am particularly to acknowledge the favour he conferred on me, by introducing me to the acquaintance of Sir Robert Menzies, Baronet, his brother-in law, a gentleman conversant in mathematical and philosophical learning, who honoured me with his friendship during my residence in the country; and, besides many personal civilities shown to myself, rendered many material assistances to the main purpose of carrying on the experiment. It is with pleasure also, that I acknowledge the civilities of all the neighbouring gentlemen, who often paid me visits on the hill, and gave me the fullest conviction that their country is with justice celebrated for its hospitality and attention to strangers. I was honoured also by visits from many learned gentlemen who came from a great distance; particularly the lord privy seal, Dr. Wilson, professor of astronomy at Glasgow, and his son, and Dr. Reid, professor of moral philosophy, and Mr. Anderson, professor of natural philosophy, also at Glasgow, Lord Polwarth, Mr. Ramsay, professor of natural history at Edinburgh, Mr. Commissioner Menzies, of the customs at Edinburgh, Mr. Copland, and Mr. Playfair, of the university of Aberdeen, the Rev. Mr. Brice, and my esteemed friend Col. Roy, who had been my companion in the journey as far as Edinburgh. So great a noise had the attempt of this uncommon experiment made in the country, and so many friends did it meet with interested in the success of it!

The use of the 2 theodolites at once, as mentioned above, much forwarded the completing of the sections all the month of October; Mr. Menzies observing the bearings at one station with one theodolite, while Mr. Burrow observed the altitudes or depressions with the other theodolite at the other station; and the labourer, who used to plant the poles in the hill, taking only one with him, and fixing it up at one place to be observed at both theodolites, and then removing it to the next station for the like purpose. Notwithstanding which, the weather became at length so bad, by the early coming in of frost and snow in the beginning of November, when the survey was nearly completed, as to

render it impossible to do any thing more that season. It became therefore necessary to finish this astronomical campaign, leaving the theodolite in the care of Mr. Menzies, to complete what little remained to be done the next season.

I have thus described the plan which was adopted for the operations on Schehallien, and the manner in which it was carried into execution; it only remains to give the result of computations made on those operations for deducing the effect of the attraction of the mountain. The operations themselves at large shall be communicated at another opportunity.

I had caused the arch of the sector to be divided by fine points, according to a new and arbitrary division adapted to the method of continual bisection. One 8th part of the radius of the instrument was found by 3 bisections, and applied as a chord to the arch from the middle on each side, intercepting each way $7^{\circ}\ 9'\ 59''.917$. These spaces were each divided by points into 128 parts, by continual bisection; therefore one division will contain $3'\ 21''.561854$. Hence the number of degrees and minutes, answering to any number of divisions, may easily be found. Twenty-four additional parts were also set off, taken from the former, and added at the 128th division on each side, to fill up the whole extent of the arch, which thus consisted of 152 divisions on each side of the centre, answering to an angle of $8^{\circ}\ 30'\ 37''.4$, which was therefore the greatest angle the instrument was capable of measuring. To find the value of the parts of the micrometer in seconds, I measured the distance of the points on the limb, by 5 at a time, by means of the plumb-line, in parts of the micrometer from 0 to 128, on each side of the middle or point marked 0 on the arch. By a mean of all these measures, one division of the arch, or $3'\ 21''.562$, came out equal to 4 revolutions and 34.8272 parts of the micrometer, 41 of which make one revolution; and therefore one part is equal to $1''.0137545$, and 41, or one revolution, is equal to $41''.5639345$. Hence the value of any number of revolutions and parts of the micrometer may be easily found. At all observations of the same star, whether on the north or south side of the hill, I brought the same point of the arch, namely, that which agreed nearest with the zenith distance of the star, under the plumb-line, so that the difference of the apparent zenith distances of the same star, on contrary sides of the hill, is given in parts of the micrometer, and has no reference to the divisions of the instrument, whether they be equal or unequal; and, the parts of the micrometer screw being perfectly equal, as I had formerly satisfied myself by measuring the interval of 2 given points on the arch with different parts of the screw, that difference of apparent zenith distances may be perfectly relied on, as far as depends on the instrument, provided the bisection of the star by the wire in the telescope, and that of the point on the arch by the plumb-line, were accurately performed. As the plane of the instrument was placed both east and west, at both stations of

the observatory, the difference of the latitude of the 2 stations may be found as well from the observations made in one position of the instrument, as the other. If the instrument had suffered no change by being carried over the hill, that is if the line of collimation was not altered by it, the results should come out equally true from the observations taken in both positions of the instrument. On the contrary, if the line of collimation should, by any means, have suffered any alteration between the observations made at the 2 stations, this would cause the difference of latitude to appear too small, by the observations made in one position of the instrument, by the quantity of the alteration, and as much too great, in the other position of the instrument. But still the mean between the 2 results, deduced from the observations taken in the 2 different positions of the instrument, would give the true difference of latitude; and that equally, whether the line of collimation had suffered any change or not. Therefore this will be the best method of comparing the observations together, and I shall take a mean of all the results, deduced from the observations taken in each position of the instrument separately, and then a mean of those means for the true difference of latitude. By single observations of 10 stars; viz. β, α, and η Cassiopeæ, and ι, η, β, 39, 45, 46, and 53 Draconis, made on both sides of the hill, with the plane of the sector facing the west, after making the proper allowance for precession, aberration, and deviation, and semi-annual solar nutation of the earth's axis (see my tables annexed to my Observations made at the Royal Observatory,) the apparent difference of latitude between the 2 stations of the observatory, comes out 54″.1, 54″.7, 54″.0, 55″.4, 55″.0, 55″.0, 52″.2, 54″.0, 54″.3, 53″.1, respectively; the mean of all which is 54″.2; the greatest difference of any one result from the mean being only 2″. In like manner, by single observations of as many stars; viz. β and α Cassiopeæ; ε Ursæ majoris; β, 39, 46, 0, 49, and 53, Draconis; and 23 Cygni; made on both sides of the hill, with the plane of the sector facing the east; after making all the allowances as before, the apparent difference of latitude comes out 54″.5, 52″.3, 56″.8, 53″.5, 54″.5, 57″.2, 56″.1, 55″.3, 54″.1, 55″.1, respectively; the mean of all which is 55″; the greatest difference of any one result from the mean being 2″. The two means 54″.2 and 55″.0 differ only 0″.8, which should argue only an alteration of 0″.4 in the line of collimation; but this is too small a quantity to be depended on; and therefore it is most probable, that the state of the instrument remained unvaried. However, whether it did or not, the mean of the 2 means, or 54″.6, is to be esteemed the apparent difference of latitude between the 2 stations of the observatory, and, when compared with the difference of latitude which should result from the trigonometrical measures, will give the sum of the 2 contrary attractions of the hill. It must be owned, that this point will be settled with rather more certainty, when all the observations made with the sector,

which exceed 300, shall have been computed; but, as from the agreement of these results together, as well as from the small differences that are usually found in observations made within a few days of one another, we may presume, that the result from the whole will not differ materially from that deduced above from 40 observations, I thought I had better take this opportunity of gratifying the impatience of the society in presenting them with these my first computations, before their summer recess, than delay giving them any account at all of this experiment, till I had leisure to complete the whole of my calculations.

I am now to show, what the distance is between the parallels of latitude passing through the 2 stations of the observatory in feet, according to the trigonometrical mensuration; and thence, what the difference of latitude ought to have been, if the hill had been away, or had exerted no sensible attraction. This depends on the enumeration of several particulars. The length of the base measured in the meadow of Rannoch, was 5897.119 feet, according to the state of the brass standard, when the thermometer was at 40°; but, to reduce it to answer to the state of the brass standard in the heat of 62°, we must subtract 16.721 feet; we should also subtract further 0.327, for the diminution which the brass standard has suffered by wear, and there remains 5880.071 feet for the true length of the base in Rannoch. See Phil. Trans. vol. 58, p. 313, 324, 326. Hence, with the help of the angles taken with the theodolite at the ends of the base in Rannoch, and at the west cairn, the horizontal distance between the east and west cairns comes out 4047.4 feet. Nearly the same result comes out from the base measured on the south side of the hill, though with less exactness; this, when all corrections are made, is 3011.684 feet, whence the distance of the 2 cairns should come out 4058 feet, or about 10 feet longer than results from the base in Rannoch. But I prefer the deduction from the base in Rannoch as most to be depended on. Hence, by the calculation of the 2 triangles formed by the 2 cairns and the two stations of the observatory, the distance between the parallels of latitude passing through the 2 stations comes out 4364.4 feet, which, according to M. Bouguer's table of the length of a degree in this latitude of $56^\circ\ 40'$, at the rate of 101.64 English feet to one second, answers to an arc of the meridian of $42''.94$. The other series of triangles carried across the hill, gives the same distance of the parallels only 10 feet less, and consequently the arc of the meridian only $\frac{1}{10}$ of a second less. Thus the difference of latitude found by the astronomical observations, comes out greater than the difference of latitude answering to the distance of the parallels, the former being 51 .6, the latter only $42''.94$. The difference $11''.6$ is to be attributed to the sum of the 2 contrary attractions of the hill.

The attraction of the hill, computed in a rough manner, on supposition of its density being equal to the mean density of the earth, and the force of at-

traction being inversely as the square of the distances, comes out about double this. Whence it should follow, that the density of the hill is about half the mean density of the earth. But this point cannot be properly settled till the figure and dimensions of the hill have been calculated from the survey, and thence the attraction of the hill, found from the calculation of several separate parts of it, into which it is to be divided, which will be a work of much time and labour; the result of which, will be communicated at some future opportunity.

Having thus come to a happy end of this experiment, we may now consider several consequences flowing from it, tending to illustrate some important questions in natural philosophy. 1. It appears from this experiment, that the mountain Schehallien exerts a sensible attraction; therefore, from the rules of philosophizing, we are to conclude, that every mountain, and indeed every particle of the earth, is endued with the same property, in proportion to its quantity of matter.

2. The law of the variation of this force, in the inverse ratio of the squares of the distances, as laid down by Sir Isaac Newton, is also confirmed by this experiment. For, if the force of attraction of the hill had been only to that of the earth, as the matter in the hill to that of the earth, and had not been greatly increased by the near approach to its centre, its attraction must have been wholly insensible. But now, by only supposing the mean density of the earth to be double to that of the hill, which seems very probable from other considerations, the attraction of the hill will be reconciled to the general law of the variation of attraction in the inverse duplicate ratio of the distances, as deduced by Sir Isaac Newton from the comparison of the motion of the heavenly bodies with the force of gravity at the surface of the earth; and the analogy of nature will be preserved.

3. We may now therefore be allowed to admit this law; and to acknowledge, that the mean density of the earth is at least double of that at the surface, and consequently, that the density of the internal parts of the earth is much greater than near the surface. Hence also, the whole quantity of matter in the earth will be at least as great again as if it had been all composed of matter of the same density with that at the surface; or will be about 4 or 5 times as great as if it were all composed of water. The idea thus afforded us, from this experiment, of the great density of the internal parts of the earth, is totally contrary to the hypothesis of some naturalists, who suppose the earth to be only a great hollow shell of matter; supporting itself from the property of an arch, with an immense vacuity in the midst of it. But were that the case, the attraction of mountains, and even smaller inequalities in the earth's surface, would be very great, contrary to experiment, and would affect the measures of the degrees of the meridian

much more than we find they do; and the variation of gravity in different latitudes, in going from the equator to the poles, as found by pendulums, would not be near so regular as it has been found by experiment to be.

4. The density of the superficial parts of the earth, being however sufficient to produce sensible deflections in the plumb-lines of astronomical instruments, will thus cause apparent inequalities in the mensurations of degrees in the meridian; and therefore it becomes a matter of great importance to chuse those places for measuring degrees, where the irregular attractions of the elevated parts may be small, or in some measure compensate one another; or else it will be necessary to make allowance for their effects, which cannot but be a work of great difficulty, and perhaps liable to great uncertainty.

After all, it is to be wished, that other experiments of the like kind with this were made in various places, attended with different circumstances. We seldom acquire full satisfaction from a single experiment on any subject. Some may doubt, whether the density of the matter near the surface of the earth may not be subject to considerable variation; though perhaps, taking large masses together, the density may be more uniform than is commonly imagined, except in hills that have been volcanos. The mountain Schehallien however bears not any appearance of having ever been in that state; it being extremely solid and dense, and seemingly composed of an entire rock. New observations on the attraction of other hills, would tend to procure us satisfaction in these points. But whatever experiments of this kind be made hereafter, let it be always gratefully remembered, that the world is indebted, for the first satisfactory one, to the learned zeal of the R. S. supported by the munificence of George the Third.

Tables are then added of all the zenith distances of the several fixed stars, that were observed, at the two observatories, from which was deduced the preceding quantity of the celestial arc, answering to the geographical distance between the parallels of latitude passing through the two observatories.

END OF THE SIXTY-FIFTH VOLUME OF THE ORIGINAL.

I. On the Nature of the Gorgonia; that it is a real Marine Animal, and not of a Mixed Nature, between Animal and Vegetable. By John Ellis, Esq. M. D., F. R. S. In a Letter to Daniel Solander, M. D., F. R. S. Anno 1776, *Vol. LXVI.*

It was your particular request, before you went to the South Seas, that I should continue my researches into the formation and growth of Zoophytes, more particularly of those formerly called Ceratophytons, now Gorgoniæ; and known in English by the name of sea-fans, sea-feathers, and sea-whips, to which class

the red coral should be added. This you thought the more necessary, as the accounts already published of them by Dr. Linnæus and Dr. Pallas seemed to make them of a mixed nature in their growth, between animals and vegetables: a thing not easily to be reconciled to the usual operations of nature. I was so fortunate about that time to receive a most excellent collection of different species of these animals, preserved at the sea side in spirits, at Dominica, which has enabled me to show more clearly, that they are true animals, growing up in a branched form, and in no part vegetable.

From the following observations it will appear, that the gorgonia is an animal of the polype kind, resembling the common fresh water polype in many of its qualities, but differing from it in the remarkable circumstance, of producing from its own substance a hard and solid support, serving many of the purposes of the bone in other animals. Every one knows, that the common polype sends out its young from its side, like buds, which being grown to the form of the parent animal, to which they still adhere, send out again their own young, like buds, adhering to themselves; and this is repeated, till at length the whole acquires a branched appearance, resembling a vegetable; see fig. 1, pl. 15. The gorgonia grows nearly in the same manner; and hence arises its resemblance to a shrub, which has given occasion to the mistake of placing it in the vegetable kingdom. But though the nature of these animals is so much like the polypes, they differ in several circumstances; the most remarkable is that already mentioned, the hard bone by which the gorgonia is supported. This is not formed by any kind of vegetation, but by a concreting juice thrown out from a peculiar set of longitudinal parallel tubes, running along the internal surface of the fleshy part. In the coats of these tubes are a number of small orifices, through which the osseous liquor (if I may use the expression) exudes; and concreting, forms the layers of that hard part of the annular circles, which some, judging from the consistence rather than the texture, have erroneously denominated wood.

Dr. Pallas, in his Elench. Zoophytorum, p. 162, is of opinion, that the layers of which the wood, as he calls it, of the tougher gorgoniæ is composed, may be separated into numerous longitudinal fibres; that the longitudinal striæ, which frequently appear on its external surface, are owing to this structure; and that these fibres are in fact hollow, like the wood of trees, the cavity of the tubes being closed up, as they become hard and rigid.

I was nearly of the same opinion when I was writing my Essay on Corallines, as may be seen in the Philos. Trans. vol. 48, p. 18, and also p. 504, where I have compared the herring-bone coralline, which is composed of many little tubes, to the growth of sea-fans and sea-feathers, now called gorgoniæ; and likewise in my Observations on the Growth of the red and white Coral, see

Philos. Trans., vol. 48, p. 504; but experience has since fully convinced me of the contrary: for on the strictest examination with the microscope, of the internal horny parts of several of those gorgoniæ fresh from the sea, and immediately preserved in spirits, not the least appearance of tubes within the horny part can be discerned, either in the longitudinal or transverse sections. There is indeed a regular cannulated appearance on the surface; but this seems to be only an external moulding, and not formed by a series of longitudinal tubes with interstices, as in plants; nor is it difficult to explain whence such a moulding may arise. I have observed, that the inner surface of the fleshy part contiguous to the bony or horny part, is furnished with longitudinal parallel tubes, which through certain pores supply the osseous matter; this, being soft at first, and only afterwards becoming hard, so as necessarily to take the form of the concave surface by which it is closely pressed, and therefore assumes a striated appearance. This is plainly seen in fig. 2, A, where the ends of the tubes and the striated appearance on the gorgonia flabellum are expressed; and at fig. 2, B, two of them are magnified.

In the isis hippuris, or black and white jointed coral, which is very nearly a-kin to this genus, these tubes are still more clearly to be seen, as they are larger, and the channels much deeper; see fig. 3, where A is a part of the coral of its natural size; B is an extremity of one of the branches magnified, with the bony part laid bare; C a part of the same, with the bony part taken out, to show the tubes with their internal orifices, through which the osseous juice is supposed to exude, and form the layers of the bony and horny part. This formation of the hard part, or bone of the stem, seems to be a principal use of the longitudinal tubes; but they have another also, of great consequence in the growth of the gorgonia: for it is by means of these, that the animal spreads itself downwards over the substances which serve for its basis, thence deriving a firmness proportioned to its bulk. By means of these likewise it repairs any deficiencies arising either from accident or natural decay, by which the life of the whole would be endangered. At fig. 2, C, D, the broken stem in the gorgonia flabellum is strengthened and made firm by the lateral reticulations being covered over with the horny substance by means of these fleshy tubes and polype suckers. This is very different from any natural repairs of broken or wounded branches in trees. Besides, these tubes extend themselves any way, creeping over every substance which may serve for their support and preservation of the animal, throwing out the horny or osseous juice to make the whole texture firmer. This wonderful contrivance of nature is certainly instinct in this low order of animals. To give a better idea of this kind of instinct, and to show in what it differs from what is called radication in plants, with which some people, for want of better information, are apt to confound it, I have given a

figure of the manner in which the flustra foliacea fastens itself to shells; see fig. 4. This figure is a little magnified, to show the form of the cells, as they have spread themselves over the surface of the scollop-shell. The advocates for the vegetation of zoophytes, I hope, will be convinced, that the part that sticks to the shell is not a root, but only a single course or layer of cells of the same animal. As it rises into leaf-like branches they become double, or 2 layers of cells, placed in such an opposition to one another as to strengthen the whole, like the cells in the honey-comb; and what is very singular, the narrow part of the stem near the shell, often consists of 4 or more layers of cells, which the animal, by this kind of instinct, most certainly applies to strengthen that slender part against the force of the waves. For another instance of the base of a zoophyte spreading downwards to secure itself, we have an example in the madrepora muricata, which is extending itself over a dead animal of the same species, as in fig. 5.

The following remark of Dr. Pallas will show, that as he conceives the wood or horny stem to be composed of tubes, so he thinks that there is a communication of juices from the polypiferous pores on the cortical part, to the inside or horny part, as in trees: for he observes, that as the trunk of the gorgonia is always proportioned to the size of its branches, the wood or horny part of the trunk, notwithstanding its hardness, must necessarily thrive, grow, and increase every way, even though the organs of the bark, or surrounding fleshy substance, at the trunk and base are obliterated; and hence he concludes, that the trunk must receive nourishment from the branches, and apprehends, this nourishment to be absorbed and prepared by polypiferous pores. Now it is evident, that the idea of the trunk and base of a tree growing in thickness, when it is divested of its surrounding bark, is contrary to the known laws of vegetation. The only method of increase in the trunks of trees is by the apposition of new layers from the bark, which cannot be produced but while the bark is subsisting.

Nor can the gorgonia increase in size, in those parts where it is deprived either of the flesh with the polype suckers, or the surrounding fleshy tubes, which communicate with these suckers; for these suckers and tubes are the organs that prepare and deposit the several thin layers, which form the support or bony part, here called wood, as I have shown before. If, on examining the internal structure of these zoophytes, it were found, that their growth and fabric anywise resembled that of vegetables, this would indeed afford a presumptive argument, that they did participate of a vegetable nature. Yet even in that case, it would be much more reasonable to suppose them animals of the lowest order, raised but one degree above the vegetable tribe, than to conjecture a monstrous metamorphosis repugnant to the general analogy of nature. But the truth is, that though the hard parts of many gorgoniæ have very much the

external appearance of wood, yet the internal structure differs in the most essential points from vegetables.

In order to prove this, I have compared different sections of the gorgonia with correspondent sections both of sea and land plants, and find they differ in the following particulars: the longitudinal sections of the stems of the larger fuci, such as the fucus digitatus, esculentus, nodosus, and saccharinus, appear composed of parallel jointed tube-like figures, the joints of which are composed of gland-like cells; these tubular appearances, when highly magnified, are discovered to be connected together by transparent reticulated fibres, or very minute transverse tubes, interwoven with the upright ones. In a horizontal section, the ranges of cells, which look like rays from the centre, as they approach the bark, become smaller and smaller, and most probably correspond with the minute pores which cover the outer surface of the plant; for when the sides of the dry stems are soaked in water, they quickly imbibe it, and soon become full of a gelatinous liquor; all which is totally different from the texture of the gorgonia.

We come now to compare them with land plants, such as shrubs, like to which they are generally supposed to grow. The gorgonia has no regular series of hollow fibres or little tubes, in what is called the wood, either longitudinal or horizontal. It appears composed of a sort of irregular laminæ like horn; the fibres of which take no certain direction, nor preserve in any two places the same thickness. It has no series of utricular vessels, as the transverse vessels of wood are called by Malpighi; or insertions as they are called by Dr. Grew. These are essentially necessary, as forming a communication from the bark and the internal parts of the wood quite through. On the contrary, the concentric circles of the gorgonia have no connection with each other; they run like so many parallel curves, and are connected by no insertions or utricular vessels; but to all appearance have been formed by separate depositions of concreting matter. So the shells of snails and oysters are formed; their respective animals throw out periodically the osseous juice or testaceous matter, which adheres to the former shell and concretes, and thus successive layers are produced. In the same manner I suppose the concentric circles of the gorgonia to be formed, successive layers of juice exuding from the fleshy tubes that surround the hard part or bone of the animal. Thus the stem of the gorgonia verticillata, or Minorca white sea-feather, is composed of different layers of a shell-like substance, (see fig. 6,) where a broken part of the stem is represented, and a piece of it magnified, to show that there is evidently no more communication between the different laminæ than there is between those of an oyster-shell. In a transverse section of the gorgonia pretiosa, or true red coral, Donati has observed, Philos. Trans., vol. 47, p. 97, [Abridgement, vol. x, p. 158.] "Different lines or annual

bands, whereof one part is of a rose colour, another yellowish, others white, others more or less charged with colours, that form concentric circles like the coats of an onion." This diversity of colours could hardly have taken place, had there been a circulation of juices through the stem; and it was probably owing to the different food which the animals had lived on at different periods.

There is another genus of zoophyte, which, though it swims freely about in the sea, yet approaches near to the gorgonia, and will serve further to explain the growth of its stem, and that is the pennatula, or sea-pen. This genus has a bone along the middle of the inside, which is its chief support. This bone receives the supply of its osseous matter by the same polype mouths, that furnish it with nourishment. Dr. Bohadsch has very judiciously brought to this genus the great Greenland clustered polype formerly described by me under that name, and now called pennatula arctica. In a cross section of the bone, (see Philos. Trans., vol. 48), the several laminæ are magnified, to show that they are formed in layers like shells, and are not full of tubes as in a vegetable growth. These animals are ranged among the vegetating kind, and so called by Dr. Pallas. There is a great affinity between the gorgonia and isis, so that the increase of the bone of the latter will greatly illustrate that of the former. The longitudinal section of the bone to the stem of the isis hippuris will show, that it has been increased in diameter by successive layers of stony matter that surround it; see fig. 7. In this instance we can trace the bone in its infant state, when nature had given it pliable black horny joints, that it might yield the better to the violence of the waves; but as soon as it became stronger, these horny black joints were no longer necessary, as we find the lower part of the stems totally overgrown with the bony substance. The furrows in this coral are deeper than those of any other; insomuch that not only the longitudinal fleshy tubes that surround the bone, but even the minute pores in them, through which the osseous juice exudes, are very discernible; see fig. 3.

We now come to a very singular circumstance in the growth of the gorgonia, in which it differs remarkably from that of trees. Fig. 8, is the figure of the naked stem or bone of a gorgonia, to which we find several tree oysters and other shells have adhered. These shell fish seem to have killed the gorgonia; for the same stem seems to be covered over with another gorgonia of the same kind; which in its growth has almost covered the shells, and likewise the branches to which they were fastened, leaving only part of the ends of the branches of the first gorgonia yet uncovered. The size and weight of the shells probably gave the waves so great a power over the stem, that it was at last broken off, and cast on shore in the state in which it is here represented. This instance of a gorgonia growing over one of its own kind, seems sufficient to account for the circle of calcareous matter found now and then in the cross sections of old stems,

between the horny circles, as has been observed by Dr. Pallas, Elench. Zooph. p. 162. "Interjecto quandoque tenui materiæ calcareæ strato." But I believe no one has ever seen the bark of trees inclosed in the same manner in the inner circles of the wood; and indeed it is so contrary to the laws of vegetation, that Dr. Pallas has not attempted to account for it, by showing any parallel instances in the transverse sections of timber. There is another remarkable instance of the manner of growing of these animals, in which the upper part of the gorgonia flabellum, meeting with an obstruction in growing upwards, grew downwards over its own fleshy substance, and evidently inclosed and covered over its own reticulated branches, with a continuation of its own flesh and bone. Dr. Pallas, in a note on the growth of the gorgonia, has the following extraordinary observation, that a gentleman in Holland is possessed of a gorgonia, which has on the same shrub, the bark partly of a gorgonia verrucosa, and partly of the gorgonia coralloides, without any visible difference of the branches; which he accounts for by comparing it to the growth of vegetables, saying: "So different lichens are often found incorporated in such a manner together, that they might easily be mistaken for one and the same plant." But I think it rather paradoxical to suppose the flesh of one animal to grow on the bones of another. If he examined it attentively, he would have found what we have advanced to be the case. It is not unusual for a gorgonia of one species to grow on the decayed branches of an individual of another, where the soft or fleshy part is already perished; but the upper or living gorgonia must have its own hard as well as soft parts; for should there be the fleshy part, and not the bony part, it would belong to the genus of alcyonium, and occasion such another remarkable mistake as this author has already made in his sertularia gorgonia, see Elench. Zooph. p. 188, where he has described an alcyonium, growing on and surrounding the stem and part of the branches of the sertularia frutescens, as a new species of sertularia. This, he says, most closely unites the genus of gorgonia with that of the sertularia; and to convince me of the truth of what he asserts, he has sent me part of the original specimen, of which fig. 9, exhibits an exact representation. At A is a magnified figure of this alcyonium, on a piece of the branch of the sertularia. It is of a fleshy substance with warts, having each 12 rays; we have many species of alcyonia from the West-Indies not much unlike this. The reader, by attending to the Doctor's own description of his sertularia gorgonia, will soon be convinced of the error, especially when he considers, that the character of a sertularia is that of a branched animal, with the hard parts without, and the fleshy parts within; and that the gorgonia, on the contrary hath its fleshy or soft parts without, and its bone or hard parts within.

There is another essential difference hitherto unnoticed, between the growth

of the gorgonia and that of trees; and that is, in the connection between the side branches and stem of the one, and the side branches and stem of the other. The side branches of vegetables proceed from the pith; of course, when a stem and side branch is divided lengthwise, the pith is seen continued through the main stem into the branch; see fig. 10, where A is the natural size of a small twig of the lime tree, and B the same magnified. It must be observed, that in some trees the channel or continuation of the pith, which leads from the stem to the side branch, is very much contracted, and the communication very narrow; in which case it will be necessary to make cross sections, which will soon discover the course of the pith from one to another. M. Du Hamel, an author of the first reputation, has clearly demonstrated this in his Physique des Arbres, vol. 2, p. 119, tab. 2, f. 91. Now in the gorgonia, the support, or what is called the woody part, is indeed furnished with a kind of a pith or medulla: but when we cut the stem or branch through the middle lengthwise, we find no passage whatever between the pith of the stem and that of the branch, each being surrounded with a hard covering of its own, which has no perforation, nor admits of any communication. Every branch of a gorgonia therefore has its own pith or medulla peculiar to itself, which is never found passing into that of another, see fig. 11, A, the natural size, B magnified. Again, in trees, the pith is largest in young shoots; and disappears in old stems: in the gorgonia the medulla is of the same diameter in the old stems as in the young branches. In the longitudinal sections of fresh shoots of trees, the pith in the microscope looks like a number of jointed tubes united together; and in the cross sections, it appears like so many circles. In dried specimens the tubular appearance in the longitudinal sections is more irregular; they look rather like longitudinal ranges of little transparent blebs, and the cross sections appear like circles intersecting each other in the margin; but there are many varieties of figures in the pith of different vegetables; what is mentioned here, is the common appearance of pith in most plants. When we cut a dry branch and stem of a gorgonia through the middle lengthwise, the pith appears divided into many little transverse membranes, like small white diaphragms, separated from each other about the distance of their own diameter. But these cross membranes are found to be more numerous in such as have been preserved directly from the sea in spirits; and when they are examined in the microscope, they appear to be of the nature and substance with the laminæ that compose the horny tube that surround them.*

* While comparing the longitudinal sections of the young branches of trees with those of the gorgonia, I was surprized to find such a similitude between the pith of a branch of a walnut tree, of a year's growth, and that of the gorgonia, see Grew, Anat. of Plants, tab. 19, fig. 4, A and B; especially as we are told by a modern author, who has published many microscopical observations on the

I come now to the outside covering or skin of the animal. As few have been at the pains to examine the surface of the gorgonia accurately, it has scarcely yet been noticed, that they are clothed with a kind of scales, and some of them so remarkably covered, and the scales so well adapted to the particular parts, that one might reasonably be induced to think, that nature has given them this defence, as she has done in like manner to the several parts of snakes and lizards, as a kind of armour to protect them from external injuries. As instances of the above, I shall only mention, that the surface of the stem, as well as the mouth of the cells of the gorgonia placomus, are defended by long pointed scales; see Essay on Corallines, p. 27, t. A, 1 to 3; and the gorgonia verticillata (of which an elegant specimen is to be seen in the British Museum) has also very remarkable scales of different sizes round the mouths and on the skin; see Essay of Corallines, t. 26, f. S, T. The gorgonia lepadifera has a most remarkable variety, placed like tiles, one over another, for the defence of the mouth of the cells that inclose the polype suckers; besides, there is a small kind of scales, that covers the surface of the stem and branches; see fig. 12.

From the skin we are naturally led to speak of the flesh of the gorgonia, or what the modern naturalists call the bark or cortex. Whoever has examined the flesh of the gorgonia, well preserved at the sea-side in spirits, will find, on dissecting them, proper muscles and tendons for extending the openings of their cells; for sending forth from thence their polype suckers in search of food; for drawing them in suddenly, and contracting the sphincter muscles of these starry cells, in order to secure these tender parts from danger; and also that there is, as we have already mentioned, proper secretory ducts, to furnish and deposit the osseous matter, for the supply of the bone, both of the stem and branches as well as the base, to secure its station with firmness, amidst the boisterous ele-

construction of timber, that the cell-like divisions in the branch of a walnut tree are only a row of single blebs of pith. But the microscope discovers to us, on viewing one of these cross membranes, that it is composed of many cells shrunk up and united together; for, on viewing the flat surface of one of them, it appeared full of circles intersecting each other, like a thick transverse section of many other dried piths pressed together: besides, the thicker part of this shrunk-up walnut pith, all round next the inside close to the wood, when magnified, plainly showed the same appearance of blebs as in other pith. To confirm this observation, May 23, 1772, I procured a young green shoot of a walnut tree, growing from a branch of the preceding year; and examining the pith, both in upright and transverse sections of this new shoot, I found that they exactly resembled the pith of many other trees, but were full of sap: and that the ranges of cells or blebs, that occupied one of these spaces, could not be less than 100, perhaps double that number of blebs. Dr. Grew takes notice, p. 120 in his Anatomy of Plants, that there are other trees, besides the walnut tree, whose pith in the last year's shoot shrinks up and forms such cavities; and an ingenious friend of mine, now engaged in an inquiry into the structure of plants, has shewn me a last year's stem of the brassica sylvestris, or shrubby cabbage, whose pith is shrunk and divided into a single row of cells, like those of the walnut tree of last year's growth.—Orig.

ment where it is appointed to be. That there are ovaries in these animals is without doubt; for in most of those that were sent to me preserved in spirits, the eggs were very visible on making longitudinal sections, in the same manner and form as in the alcyonium digitatum, called dead man's hand; see Philos. Trans., vol. 53, Abridg. p. 41, vol. 12, but much larger; and it is very probable that many of these animals are viviparous, as we have seen among the sertulariæ.

So that I must conclude, that though they grow in a branched form, they are no more allied to vegetables than they are to the ramified configurations of sal ammoniac; to the elegant branched figures in the Mocha and other gems, called dendrites; to the arbor Dianæ, or the arborescent figures of the Cornish native copper: consequently, that animal life does not depend on bodies growing according to a certain external form. Hence it appears, that this metamorphosis of a plant to an animal is a flowery expression, and in my opinion, better suited to the poetical fancy of an Ovid, than to that precise method of describing which we so much admire in a natural historian.

II. The Variation of the Compass; containing 1719 *Observations to, in, and from, the East-Indies, Guinea, West-Indies, and Mediterranean, with the Latitudes and Longitudes at the time of Observation. The Longitude for the most Part reckoned from the Meridian of London. By Mr. Robert Douglass.* p. 18.

It is unnecessary to repeat these observations, as they, with many thousand other observations, have been employed by Messieurs Mountaine and Dodson, in constructing their universal magnetical charts.

III. Propositions selected from a Paper on the Division of Right Lines, Surfaces, and Solids. By James Glenie, A. M., of the University of Edinburgh. p. 73.

Prop. 1. *If from the angles at the base of any right-lined triangle, right lines be drawn to the alternate angles of rhombi, described on the opposite sides, and applied reciprocally to the sides produced; and from the vertex, through the intersection of these lines, a right line be drawn to meet the base: the segments of the base, thus made, will have to each other the duplicate proportion of the sides.*— Let ACB be any right-lined triangle, fig. 11, pl. 13. Let AFEC, CDGB be rhombi; on any two sides AC, CB of this triangle, applied respectively to CB, AC produced: from the alternate angles EFA, DGB, of which let FB, GA, be right lines drawn to the angles at the base, or third side, AB. Then, if through the intersection O of these lines, a right line COL be drawn from the vertex C to meet the base AB; the segments AL, LB, of the base thus made, will have to each other

the duplicate proportion of the sides AC, CB. This Mr. G. demonstrates geometrically, and then adds the following corollaries.

Cor. 1. If the triangle be isosceles, the right line drawn from the vertex to the base is perpendicular to it, and the segments of the base are equal to each other.

Cor. 2. When the triangle is right-angled, the line drawn from the vertex to the base is always perpendicular to it (as appears from E. 8, 6, and its cor.); and the rhombi become squares on the sides comprehending the right angle.

Cor. 3. The segments of the sides adjacent to the base, are respectively 3d proportionals to the sum of the sides, and the sides themselves.

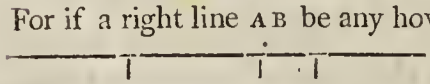
Cor. 4. The segments of the sides adjacent to the vertex are equal to each other, and each of them is a 4th proportional to the sum of the sides and the sides themselves.*

Cor. 5. The segments of the base are proportional to the segments of the sides, which are adjacent to them.

PROP. 2. *Let there be any two right lines given: there is an angle which may made by these lines, such, that if from their extremities which do not meet, right lines be drawn to the alternate angles of rhombi described on them, and reciprocally applied to them when produced; and from the said angle through the intersection of these lines, a right line be drawn to meet the right line joining the said extremities; the segments of this line thus made, shall be respectively equal to the adjacent segments of the given lines.*—Let AC, CB, be any two given right lines, fig. 12, pl. 13; and let CD, in AC produced, be equal to CB. On AD describe a semicircle; draw CN at right angles to AD, and equal to CD; join A, N, and apply a right line AM in the semicircle equal to AN. From the point M draw the right line MS at right angles to AD. Make a triangle ACB, having its sides equal to AC, AS, and CB; and ACB is the angle required to be found; and the segments AL, LB, of the right line AB joining the extremities A and B, of the given lines, are respectively equal to the segments AP, BQ, of the given lines, which are adjacent to them. This Mr. G. demonstrates as before.

PROP. 3. *To multiply the square of a given finite right line by any number.*—On an indefinite right line AP set off the given right line AB, fig. 13, pl. 13; draw BC at right angles to AP and equal to AB; and from A through C draw an indefinite right line AQ. Take AD equal to AC, and draw DE parallel to BC; AF equal to AE, and draw FG parallel to BC, and so on. Then it appears, from

* And it may be added, a mean in proportion between the two segments adjacent to the base. For if a right line AB be any how divided in C, and from the two segments CA, CB, 3d proportionals



to the whole line and each segment respectively, CD, CE, be taken away, the remainders AD, EB, are equal, and each is a mean in proportion between the two CD, CE.—Orig. S. HORSLEY.

47 E. 1, that the square of AC is equal to the square of AB multiplied by 2; the square of AE equal to the square of AD or AC multiplied by 2; that is, equal to the square of AB multiplied by 4, and so on. Thus the squares of AC, AE, AG, AI, &c. are respectively equal to the square of AB multiplied by the terms of the following series 2, 4, 8, 16, 32, &c. where the 63d term gives the square of AB multiplied by the last term of Sessa's Series for the Chessboard.

If CX be drawn parallel to AP, the squares of A*a*, A*b*, AC, A*d*, &c. will be respectively equal to the square of AB multiplied by 3, 5, 9, 17, 33, 65, 129, &c. Also if A*g* be taken equal to A*a*, and *ge* be drawn parallel to BC, and this be repeated, the squares of A*e*, &c. wiil be equal respectively to the square of AB multiplied by 6, 12, 24, 48, &c. And the squares on A*o*, &c. will be equal to the square on AB multiplied by 4, 7, 13, 25, 49, &c. In like manner, if AM be taken equal to A*b*, and MN be drawn parallel to BC, the squares on AN, &c. will be equal respectively to the square on AB multiplied by 10, 20, 40, 80, 160, &c. And the square on AS &c. will be equal respectively to the square on AB multiplied by the terms of the series, 6, 11, 21, 41, 81, 161, &c.

In the same way, if right lines be drawn from E, *e*, G, N, I, &c. there will arise numberless other series. And if BC be taken equal to AB multiplied by any number, surd, fractional, or mixed, there will be obtained a great variety of series, consisting respectively of terms, which are surd, fractional, or mixed. And by dividing BC, DE, *ge*, FG, MN, HI, &c. in different ways, according to pleasure, we may apply the same method to fractional numbers, without altering the magnitude of BC. Thus, if BC be bisected, and a right line be drawn through the point of bisection parallel to AP, there will be found right lines, the squares on which are respectively equal to the square on AB multiplied by a great number of fractions, having 4 for their common denominator, and so on.

PROP. 4. *To find a right line, the square on which shall be equal to the square on a given right line, divided by any number.*—If, using the figure of the immediately preceding problem, we suppose the given right line to be denoted by AI, the squares on AH, AF, AD, AB, &c. will respectively be equal to the square on AI multiplied by $\frac{1}{2}$, $\frac{1}{4}$, $\frac{1}{8}$, $\frac{1}{16}$, $\frac{1}{32}$, $\frac{1}{64}$, $\frac{1}{128}$, $\frac{1}{256}$, $\frac{1}{512}$, $\frac{1}{1024}$, &c. or divided by 2, 4, 8, 16, 32, 64, 128, 256, 512, 1024, &c.; and so on for other numbers, whole, surd, fractional, or mixed.

PROP. 5. *To cut off from a given right line a part expressed by any odd number.*—Let AB be the given right line, fig. 4. pl. 13. At right angles to it, at one of its extremities B, draw an indefinite right line BE. Let n be the given odd number, expressing the part of AB to be cut off. Take BC such a right line (prop. 3) that the square on it shall be equal to the square on AB, multiplied by the number $\frac{n-1}{2}$. Draw CL as in the first theorem, and take LS equal to AB. Then AS is that part of AB, which is expressed by the odd number n.

For the square on AC, being equal to the squares on AB, BC, is equal to the square on AB multiplied by the number $\frac{n-1}{2}+1$, or $\frac{n+1}{2}$. Therefore it appears (from prop. 1 and cor. 1 to 20 E 6), that AL is to LB as $\frac{n-1}{2}+1$ to $\frac{n-1}{2}$. Consequently, AS is equal to the part required. Q. E. F.

Thus, if the square on BC be supposed successively equal to the square on AB multiplied by the terms of the series 5, 6, 7, 8, 9, 10, 11, 12, 13, 14, 15, 16, 17, 18, &c. the numbers of the several parts denoted by AS, will be 11, 13, 15, 17, 19, 21, 23, 25, 27, 29, 31, 33, 35, 37, 39, 41, &c. which series comprehends all odd numbers after 9, and might have begun from 3, had the other series begun from 1.

PROP. 6. *To cut off from a given right line a part expressed by any even number.*—Let m denote any even number in general. Draw any indefinite right line BH, and at right angles to it another BE, fig. 15, pl. 13. On BE set off the given right line BA, and from A, with the distance equal to a right line, the square on which is equal to the square on AB multiplied by the number $m-1$, intersect BH in some point C. From the vertex A of the triangle BAC draw AL as was directed in prop. 1, and draw LS parallel to CA. Then BL is such a part of BC as is expressed by the number m; and BS is the same part of AB. Thus, if the square on AC be successively denoted by the square on AB multiplied by 3, 5, 7, 9, 11, 13, 15, 17, 19, 21, 23, 25, &c. then BS will be successively such a part of AB as is expressed by 4, 6, 8, 10, 12, 14, 16, 18, 20, 22, 24, 26, 28, &c.

PROP. 7. *If from the angles of the base of any right lined triangle, right lines be drawn to the alternate angles of rhombi described on the other two sides, and reciprocally applied to them produced; and through the intersection of these lines, a right line be drawn from the vertex to the base; the rectangle contained by the sines of the angles at the extremities of one of the sides, will be equal to the rectangle contained by the sines of the angles at the extremities of the other; and the parallelopiped contained by the sines of the angles of one of those triangles, into which the original one is divided by the said line drawn from the vertex, will be equal to the parallelopiped contained by the sines of the angles of the other.*

Cor. The two triangles, adjacent to the segments of the base, have to each other the proportion of the two adjacent to the sides containing the vertical angle, or the proportion of the two into which the original triangle is divided; and any one of these pairs of triangles are as similar figures described on the sides, being as the segments of the base, which have to each other the duplicate proportion of the sides.

PROP. 8. *If from the angles at the hypothenuse of any right angled right lined triangle, right lines be drawn to the alternate angles of squares described on the sides containing the right angle; and from the point where the right line drawn from the right angle, through their intersection, meets the hypothenuse, right lines*

be drawn to the points where these lines meet the sides; the lines so drawn will make equal angles with the hypothenuse, and the right line drawn from the right angle to meet it; and will also have to each other the proportion of the sides containing the right angle.

Cor. 1. The alternate triangles of those 4, which have their vertices in the point where the right line drawn from the right angle meets the hypothenuse, are similar, and have to each other the proportion of the segments of the hypothenuse, or the duplicate proportion of the sides containing the right angle.

Cor. 2. Either pair of the adjacent triangles lying on different sides of the right line drawn from the right angle, and having their vertices in the intersection of the right lines drawn from the angles at the hypothenuse, have to each other the proportion of the alternate triangles, having their vertices in the intersection of the first-mentioned line and the hypothenuse.

Cor. 3. The trapezium or quadrilateral figure formed by the segments of the sides adjacent to the right angle, and the right lines joining their extremities with the intersection of the hypothenuse and the right line drawn from the right angle to meet it, is capable of being inscribed in a circle; and is divided at the intersection of right lines drawn from the angles at the hypothenuse to the alternate angles of squares, described on the sides containing the right angle, into triangles which are proportional to each other, and when taken two by two, as they lie adjacent on different sides of the diagonal, are proportional to the unequal sides of the trapezium, and to the two triangles into which the diagonal divides it.

Prop. 9. *If from the angles at the base of any right lined triangle, right lines be drawn to the alternate angles of rhomboids described on the other two sides, and reciprocally applied to them produced; a right line drawn from the vertex, through the intersection of these lines, will cut the base into two parts, having to each other the proportion compounded of the proportion of the sides, and of the proportion of the other two lines comprehending the rhomboids.*—Let the triangle be ACB, the base AB, the rhomboids ACEF, BCDG, fig. 11, pl. 13; and let the right lines BF, AG, be drawn. Then, if from the vertex C through their intersection O, a right line COL be drawn to meet the base, the segments AL, LB, will have to each other the proportion compounded of the proportions of AC to CB, and of CE to CD.

Scholium. If CE, CD, be equal to each other, then AL has to LB the proportion of AC to CB, and CL bisects the angle ACB; if CE have to CD the inverse proportion of AC to CB, AL is equal to LB; if CE have to CD the proportion of AC to CB, AL has to LB the duplicate proportion of AC to CB; and universally, if CE have to CD any multiplicate proportion, n, of AC to CB, AL has to LB such a multiplicate proportion of AC to CB as is expressed by the number $n + 1$. And

if CE have to CD any multiplicate proportion m of CB to AC, AL will have to LB such a multiplicate proportion of CB to AC, as is expressed by the number $m - 1$.

IV. A new Method of Finding Time by Equal Altitudes. By Alex. Aubert, Esq., F. R. S. p. 92.

Among the various methods practised for finding time, that by equal altitudes of the sun or of a star, has hitherto been esteemed the most eligible for observers, who are not furnished with a good and well-adjusted transit instrument. But this method, though one of the best, is generally attended with inconveniencies, which render the practice of it more difficult, and the result less perfect than could be wished. If the sun or stars near the equator be employed, as usual, and the altitudes be taken near the prime vertical, where the change of altitudes is the quickest, the interval of time between the observations must, in most latitudes, be of so many hours, that the observer cannot always attend to the corresponding altitudes: the weather may prove variable, so as to disappoint him at last; the clock or watch may go irregularly during so long an interval; and if the altitudes cannot, on account of their great distance from the meridian, be taken very high; an alteration in the state of the atmosphere may produce a variation of the refraction, and occasion the horary arcs to be different, though the apparent altitudes will be the same. To which may be added, the difficulty of making the instrument follow the object in its motion in azimuth, without danger of disturbing its adjustment in regard to altitude. To remedy all these inconveniencies, the following method was thought of; and having been practised with constant success, it is presumed the communication of it may be acceptable to astronomers.

If a star be selected, of which the polar distance is very little less than the complement of the latitude of the place of observation, it will, at equal distances from the meridian, come to vertical circles, which touch its parallel of declination. The star, when in these vertical circles, will be near the meridian, near the prime vertical, and near the zenith; and consequently, if it be observed there, the interval between the eastern and the western altitudes will be short; the alteration in altitude will be quick; the star cannot be affected by a different refraction; besides, it will have no motion in azimuth. To observe the star in these vertical circles, 2 things are necessary: the first is, to be provided with an astronomical quadrant, having 3 or more horizontal wires in the telescope, and if it has also a speculum at the eye-end of the telescope, to bring the vertical ray horizontal, it will be found very convenient. The next thing is, to make a computation of the apparent zenith distance of the star in the vertical circles which touch its parallel of declination; for if the telescope be fixed to

this zenith distance, as soon as the star is found to come to it, it will be in the proper vertical circle.

The advantage of this method will appear in the following example of equal altitudes, taken July 15, 1773, at Loam-pit hill, near Deptford, in latitude 5° 28′ 7″ N. and longitude 5″ in time W. of the Royal Observatory at Greenwich. The star selected was γ Draconis, having 38° 28′ 21″ apparent north polar distance, being very little less than the complement of the latitude 38° 31′ 53″. Then,

As cos. 38° 28′ 21″	: rad.	:: cos. 38° 31′ 53″	: cos. 2° 19′	the zenith distance;
and sin. 38 31 53	: rad.	:: sin. 38 28 21	: sin. 87 5 20″	the azimuth;
also rad.	: tan. 38 28 21	:: cotan.... 38 31 53	: cos. 3 43 13	the horary arc = 14^m

$52^s.9$ in sidereal time, or 14^m $50^s.5$ in mean time.

The true zenith distance being 2° 19′ the same was diminished by 2″ for refraction, and the telescope fixed to 2° 18′ 58″, the apparent zenith distance; and when the star came to the wires, the times by the clock were as follow:

	Eastern altitudes.			Western altitudes.	Meridian passage.
1st wire at..	9^h 55^m 43^s	2^m 14^s	2^m 14^s	10^h 29^m 46^s...........	10^h 12^m $44^s.5$
2d........	9 57 57	2 12	2 12	10 27 32...........	10 12 44.5
3d........	10 0 9			10 25 20...........	10 12 44.5

so that in about 34^m the complete set of altitudes was obtained near the prime vertical, free from the effects of a different refraction, and any motion in azimuth. The horary arc observed by the middle wire not turning out exactly according to the computation, is of no consequence to the observations. Some little difference may arise in it from small inaccuracies in the estimation of the star's apparent polar distance, the latitude of the place, or the error of the line of collimation; or from not setting the telescope exactly to the proper zenith distance; but as the chief intention of the computation is to find the vertical circles in which the star has no motion in azimuth, the other parts of it need not be strictly attended to.

The following manner of inferring mean time from the star's meridian passage, being more convenient and concise than the usual one, may also be acceptable. From the star's apparent right ascension, increased by 24 hours if necessary, subtract the sun's apparent right ascension for apparent noon; diminish the remainder by the proportional part of the star's acceleration, at the rate of 3^m $55^s.91$ for 24 hours, of which a table is easily computed; to this last remainder apply the equation of time for apparent noon, according as it is additive or subtractive; the result will be the mean time of the star's passing the meridian.

Ex.—The AR apparent of γ Draconis July 15, 1773, was........	17^h	51^m	$24^s.0$
— the apparent AR of the sun at apparent noon..............	7	39	59.0
First remainder......	10	11	25.0
— the star's acceleration for 10^h 11^m 25^s, at 3^m $55^s.91$ for 24 hours.		1	40.2
Second remainder...........................	10	9	44.8
+ the equation of time at apparent noon, additive		5	27.7
Gives the star's meridian passage in mean time	10	15	12.5

1

But the clock showed $10^h\ 12^m\ 44^s.5$ when the star passed, consequently it was $2^m\ 28^s.0$ too slow for mean time.

Observers, who are not furnished with tables of the sun's right ascension, and of the equation of time for the apparent noon of their meridian, may apply both as they are given in the Nautical Ephemeris for the meridian of the Royal Observatory at Greenwich; the result will be the mean time of the star's passing the Greenwich meridian. And by applying the proportional part of the foregoing acceleration of $3^m\ 55^s.91$, belonging to the difference of longitude in time of the place of observation from Greenwich, the mean time of the star's passing the meridian of the place of observation will be found. If the place be to the east of Greenwich, the acceleration will be additive; if to the west, subtractive.

In a similar manner, the mean time of any observation made with a clock, regulated to sidereal time, may be inferred, provided the preceding transit of the sun has been observed; for if from the time of the observation by the clock, increased if necessary by 24 hours, the time of the observed transit of the sun be subtracted, the remainder, diminished by the proportional part of $3^m\ 55^s.91$, and duly corrected by the equation of time for the preceding noon, will give the mean time required. It is understood that the clock keeps the rate of sidereal time exactly; for if not, a further correction for the loss or gain since noon must be applied.

END OF VOLUME THIRTEENTH.

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